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SATURDAY, OCTOBER 3, 1835.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

INTRODUCTORY LECTURE.

GENTLEMEN,—If we compare the two kingdoms of nature, the organized and the mineral, we can scarcely fail being struck with one very remarkable distinction—namely, that the first has what may be called two modes of existence, the one termed *health*, which I shall take the liberty of designating the *normal* condition; the other denominated *disease*, which, by way of contrast, we may regard as an *abnormal* state. Each of these conditions has a science appropriated to itself; physiology treats of organized beings in the healthy state, while pathology is devoted to the condition of disease. Among minerals, however, we find no existence analogous to that of disease, nor any science corresponding to that of pathology; their active properties never vary either in degree or kind, and therefore their phenomena can be anticipated and calculated. Among living beings, on the other hand, we find, as the illustrious Bichat has remarked, that the functions are subject to numberless varieties, frequently exceeding their natural degree, and baffling all calculation. We cannot foresee or foretell their phenomena: we judge only by analogy, which in the vast proportion of instances is extremely uncertain.

The existence of all living beings, whether we regard them in their normal or their abnormal states, is limited; and therefore death is common to both conditions—"*Fugienda nunquam est mors.*" But as the latter of these two states, namely disease, has a tendency to shorten this limited existence, and as it is generally accompanied with more or less pain, we almost instinctively seek some means of restoring health, and thereby of preserving our lives, and alleviating our sufferings. These means, whatever may be their nature, have been denominated *remedies*, or therapeutic agents; and that branch of the science of medicine which treats of them has been called *acology*.

They include those which may be denominated *hygienic*, as aliment, bodily exercise, mental impressions, and other agents, which were formerly very absurdly termed the non-naturals; 2dly, *surgical* remedies, as bloodletting, acupuncture, &c. 3dly, certain *physical* agents, as heat and electricity; and 4thly, *pharmacological* agents, or *medicines*, and which we may define to be those substances used in the treatment of diseases, which, when applied to the body, produce certain alterations or modifications in the functions, and which are not essentially alimentary.

The present course of lectures is devoted to the last class, and will contain an account of the natural history, the physical and chemical properties, and the physiological effects and uses, of medicines. I should prefer announcing the course simply as one on *Pharmacology*; but, in conformity with the regulations of the examining boards, I am obliged to employ the term *Materia Medica*, which is so far objectionable, that writers are not agreed on its limits, its uses, or its applications.

History.—It is customary to devote a portion of the introductory lecture to an historical outline. I may remark, however, that in the short space of a single lecture it is impossible to offer more than a very rudimentary sketch. I am of opinion also, that it is much better, and far more interesting, to study the history of a science when we have made ourselves acquainted with most of its details; so that, in fact,

any historical discussion would be more appropriate at the conclusion than at the commencement of a course.

With this impression I have thought it best to put a few of the leading particulars relating to pharmacology into a tabular form, to which each of you can refer at your leisure. [See p. 10.]

In justice I ought to add, that I have received considerable assistance in the arrangement of this table from Dr. Choulant's *Tafeln zur Geschichte der Medizin.* Those who desire to enter more fully into the subject may consult the posthumous work of Dr. Voigtels, entitled, *Vollständiges System der Arzneimittellehre*, and also Dr. Bischoff's *Handbuch der Arzneimittellehre.* We are remarkably deficient in writings in our language on this subject; and, indeed, the only book to which I can refer you, is the *Treatise of Materia Medica*, by Dr. Cullen, in which you will find a brief critical survey of the history of this branch of medical science. As pharmacology forms a part of the science of medicine, you may refer with advantage to writings on the history of medicine in general. Sprengel's work on this subject is the most elaborate and complete yet published. As an introduction, I would strongly recommend to your notice the admirable *Sketch of the History of Medicine*, by Dr. Bostock.

Means of learning the effects of medicines.—Leaving, therefore, historical details, I proceed to examine the different means we may adopt to acquire a knowledge of the effects of medicines. I may premise, that the statements made by the ancient writers on this subject are not to be relied on, for in some cases they are totally devoid of truth, and in others much overcharged. This is particularly remarkable in the writings of Dioscorides and Galen.

The doctrines of *astral influences*, and of *signatures*, adopted by Paracelsus, are too absurd to require much notice. They form part of a supposed hidden science of divine mysteries, called *cabala*. On account of their resemblance to the testicles, the bulbs of some orchideous plants were supposed to act on the organs of generation; while the fancied resemblance between the leaves of the *Pulmonaria* and the lungs of man was thought to indicate the the use of this plant in pulmonary affections.

Discarding all absurd notions of this kind, we have *four* principal methods which in modern times have been resorted to, for the purpose of ascertaining the effects of medicines. These are founded—

1. On the sensible properties of medicines.
2. On their natural historical properties.
3. On their chemical properties.
4. On the experience of their effects.

1. The *sensible properties*, such as colour, taste, and smell, have long been used. We are, in fact, led instinctively to this mode of investigation. The lower animals evidently derive considerable assistance in judging of the qualities of their food by the organs of their senses. We find them for the most part avoiding poisonous substances, selecting such as are wholesome and nutritious, and when sick, frequently resorting to such herbs as are best adapted to assist in restoring health.

There are two important objections to this mode of estimating the properties of medicines: the first is the difficulty, if not absolute impossibility, of defining sensations; for supposing a new substance should be discovered, whose taste is remarkable and peculiar, how is it possible to convey by words to a second person a correct notion of the kind of impression made on the gustatory organ? Secondly, the idiosyncrasies of sensation are so extraordinary, that two persons may disagree as to the character of the sensible property; for a sensation which may be grateful and pleasing to one, may be disagreeable and loathsome to another.

This mode of judging of the virtues of medicines has been confined principally to vegetable substances; but even here cannot be relied on. Both *Quinia* and *Strychnia* are devoid of smell, are white, and have an intensely bitter taste, and yet, notwithstanding this agreement in their sensible properties, differ widely in their effects, the one being a tonic, the other a powerful poison.

2. *Natural-historical properties* are those made use of in natural history to determine the affinities of natural bodies; they comprehend exterior form, and structure. They have been resorted to for the purpose of determining the effects of certain substances. It has long been supposed that those plants which resemble each other in their external appearances are endowed with analogous medicinal properties. Cæsalpinus was, I believe, the first who taught this doctrine. Baptista Porta says, "*tali formæ tales vires conveniunt*;" and Linnæus also remarks, "*Plantæ quæ genere conveniunt, etiam virtute conveniunt; quæ ordine naturali continentur, etiam virtute propius accedunt; quæque* classe *naturali congruunt, etiam viribus quodammodo congruunt.*" The first distinct work on the subject is that by Decandolle, entitled, *Essai sur les Propriétés Médicales des Plantes*, the first edition of which appeared in 1804. In the year 1831 we had another interesting treatise on the same subject by Dierbach, entitled, *Abhandlung über die Arzneikräfte des Planzen, verglichen mit ihrer Structur und ihren chemischen Bestandtheilen.*

It is undeniable that in numerous instances there exists an analogy between

the exterior forms and the medicinal properties of plants, so that we can sometimes predict the active principle and mode of operation, merely by knowing to what part of a natural arrangement a plant properly belongs. Cruciferæ (fig. 1), for exam-

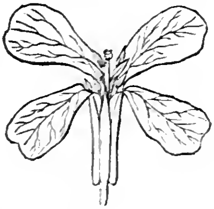


FIG. 1.—*Raphanus sativus.*

ple, present the greatest uniformity in their botanical, chemical, and medicinal characters. They contain a volatile acrid principle, which renders them stimulant; and having been employed successfully in scurvy, are frequently termed anti-scorbutics. The Labiatæ (fig. 2),

FIG. 2.—*Glechoma hederacea.*

which constitute, perhaps, the most natural family of the whole vegetable kingdom, contain a bitter resinous, or extractive matter, and an etherial, aromatic, or volatile oil; which two principles, mixed in different proportions, are found in all the species, to which they communicate tonic and carminative properties. Neither Cruciferæ nor Labiatæ contain a single unwholesome or even suspicious species. In Coniferæ (fig. 3) we find the different species per-

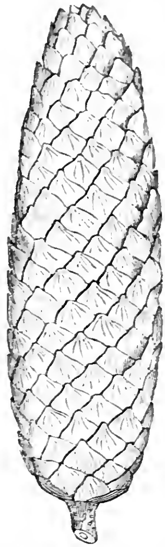


FIG. 3.—*Picea vulgaris.*

vaded with an oleo-resinous juice, in consequence of which they possess stimulant properties.

We are obliged, however, to admit the existence of numerous exceptions to the before-mentioned general rule. We can point out many families, the plants of which appear to possess the greatest botanical affinity for each other, but whose remedial properties are very different. Take Umbelliferæ (fig. 4) as an example. The root and leaves of the *Daucus carota* are wholesome and nutritive, but the analogous parts of *Conium maculatum* are highly poisonous. In some cases we even find plants of the

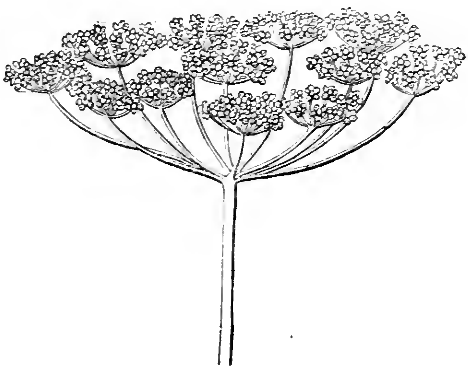


FIG. 4.—*Fœniculum vulgare.*

same genus differing considerably from each other in a medicinal point of view. I need only mention as examples in proof, *Cucumis melo* and *Cucumis colocynthis.* If

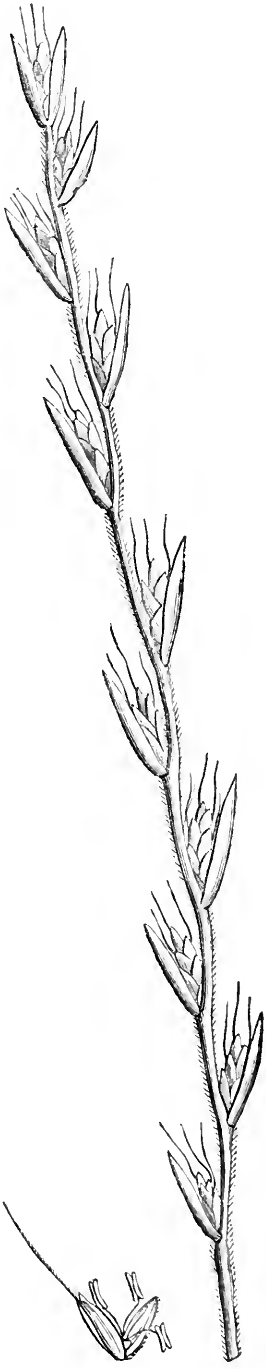


Fig. 5.—*Lolium temulentum.*

we are to believe the statements of competent authorities, even Gramineæ, which Decandolle declares to be "la famille la plus naturelle," contains more than one exception to the general statement in question. For the most part the plants of this family are farinaceous and nutritive. "None," says Dr. Lindley, "are unwholesome in their natural state, with the single exception of *Lolium temulentum* (fig. 5), a common weed in many parts of England, the effects of which are undoubtedly deleterious, although perhaps much exaggerated." I may remark, however, that several other grasses have been asserted to be unwholesome. Loudon tells us that the seeds of *Bromus mollis* are said to bring on giddiness in the human species and quadrupeds, and to be fatal to poultry. The root of *Bromus purgans* is said to be used in Canada as an emetic, in doses of forty grains; but I confess the authority for this statement is somewhat doubtful. *Bromus catharticus*, a Chilian plant, has a thick root, which acts as a purgative. I am aware that these statements require further proof, for *Bromus secalinus*, which was stated by some writers also to be poisonous, has been found by Cordier to be innocuous. We have, however, another case, on much better authority (that of Humboldt);—I allude now to a Peruvian grass, called by Kunth the *Festuca quadridentata* (fig. 6), and which is denominated in Sprengel's *Species Plantarum* the *Sessleria quitensis*. Humboldt tells us that this plant is very poisonous, and even fatal to animals. Perhaps this may be the grass described by some under the name of *Carapoucha*, and which, by others, has been called *Carapullo*. Frezier, in his *Voyage to the South Sea and along the Coasts of Chili and Peru*, in the years 1712, 1713, and 1714, says, in speaking of Lima, "There is an herb called *Carapullo*, which grows like a tuft of grass, and yields an ear, the decoction of which makes such as drink it delirious for some days. The Indians make use of it to discover the natural disposition of their children. All the time when it has its operation, they place by them the tools of all such trades as they may follow,—as by a maiden, a spindle, wool, scissars, cloth, kitchen furniture, &c.; and by a youth, accoutrements for a horse, awls, hammers, &c.: and that tool they take most fancy to in their delirium, is a certain indication of the trade they are fittest for,—as I was assured by a French surgeon, who was an eye-witness of this verity."

In the family *Solaneæ* we meet with other exceptions. Compare, for example, the fruits of *Capsicum annuum* and *Atropa Belladonnæ*. I might select many other instances (especially from the family *Leguminosæ*), to the same effect, but I shall content myself with the examples already

Fig. 6.—*Festuca quadridentata.*

adduced, as sufficiently warranting the assertion that, in the present state of science, botanical affinities cannot be confidently relied on by the medical practitioner for determining the effects of remedial agents. I cannot, therefore, agree with Dr. Lindley, in his assertion, that "a knowledge of one plant is a guide to the practitioner, which enables him to substitute some other plant *with confidence* which is naturally allied to it." In drawing this conclusion, I must caution you against running into an opposite extreme, and assuming that botanical affinities are of no use whatever to the medical practitioner. It will be my duty, when I speak of the vegetable materia medica, to shew you that as a *general rule* we may admit that plants of the same family agree in the *nature* of their medicinal operation, but that this general rule has many remarkable exceptions which diminish its utility in practice, though they do not absolutely destroy it. Doubtless in some

cases the apparent exceptions arise from our imperfect knowledge of the affinities of plants.

No one has even attempted to infer the medicinal properties either of minerals or of animals from their natural-historical qualities. In the mineral kingdom, it has of late been shewn that two different substances may assume the same form; and that this depends not on a similarity in the *nature* but of the *number* of the molecules entering into the constitution of the substances. Such substances are said to be *isomorphous*. This fact proves that there is no necessary relation between the exterior forms and medicinal properties of minerals.

3. The *chemical properties* of medicines have sometimes been resorted to for the purpose of determining their medicinal properties. That this mode of investigation is fallacious, is, I think, demonstrated from the following circumstances:—

(*a*). Substances whose chemical properties are exceedingly dissimilar, sometimes concur in the nature of their medicinal effects; and as examples of this, I may mention manna and supertartrate of potash.

(*b*). Similarity of chemical properties is not always attended with a similarity of medicinal effect. *Baryta* and *Strontia*, for example, agree in so many chemical characters, that they were at first confounded; yet the salts of the first of these substances are energetic poisons, while those of the latter act feebly on the body.

(*c*). We cannot determine the operation of medicines by knowing either the nature or number of the ultimate particles. Who could have anticipated that a compound of carbon, nitrogen, and hydrogen (all inert substances) could possess such an energetic operation as that of *hydrocyanic acid?* *Carbonic* and *oxalic* acids are composed of the same kind of constituents the one as the other, but in somewhat different proportions; yet in how opposite a manner do these substances act when taken into the stomach! Compare the per centage composition of *Quinia*, *Strychnia*, and *Morphia*, and the slight differences in composition will not be found sufficient to explain their dissimilar operation.

	Quinia.	Morphia.	Strychnia.
Carbon ..	75·76	72·20	77·21
Hydrogen .	7·52	6·24	6·73
Nitrogen ..	8·11	4·92	5·96
Oxygen ..	8·61	16·66	10·10
	100·00	100·02	100·00

(*d*). But it may be said we ought to take proximate and not ultimate principles as our guide. To this I reply, we cannot always determine what are *products* and what *educts*, in our analysis of organic substances, and therefore we cannot tell precisely what are and what are not proximate principles. It was at one time supposed that the essential oil of bitter almonds was contained in the seeds from whence it is obtained. It is now well established that this oil, as well as the hydrocyanic acid it contains, are formed by the action of the water (employed in the process) on the almond cake. The same statement may be applied to the volatile oil of mustard-seed. If, then, this really happen in two cases, we may fairly suspect it in many others. Raspail regards the vegetable alkalies (such as quinia, cinchonia, morphia, &c.) as artificial products; but the balance of evidence is certainly against this notion.

(*e*). In some instances the active principle of a medicine is much more difficult to isolate than to discover the nature of the operation of the agent by other means. Ergot of rye is a good example of this. We have yet no satisfactory results from chemical investigations as to the nature and properties of the active principle of this substance.

(*f*). Lastly, it appears that two substances may be composed of the same constituents, and in the same ratio, and yet be endowed with different properties. Such substances are said to be *isomeric*.

4. *Experiment on man and other animals* constitutes the last and most important method of ascertaining the virtues of medicines; for I think I have made it sufficiently evident that for this purpose we cannot implicitly confide in the sensible, natural-historical, or chemical properties. The action of medicines and poisons on man is for the most part similar in kind or quality, though different in degree, to that on other animals,—modified somewhat by the variations in the development of the several organs and functions. It has been asserted that some substances which are energetic poisons to man are innocuous to animals, and *vice versâ*. Let me caution you against implicitly believing vague assertions of this kind. Thus Voigtels states that horses may eat *Aconitum napellus* with impunity; but Viborg has shewn that they, like man, are poisoned by it, though the quantity requisite to produce death is large. It is a popular notion that arsenious acid is innocuous to horses. I am told that doses of a scruple, or even half a drachm, are frequently exhibited to them, with no evidently hurtful consequences; nay, it is said with beneficial re-

sults. But notwithstanding these statements, we are quite certain that arsenious acid acts as a poison to the horse. In the year 1812, a man of the name of Dawson was indicted for poisoning several horses at Newmarket, by injecting an arsenical solution with a syringe into the watering-troughs. Mr. Bowles, a veterinary surgeon, in his evidence, mentioned that he had poisoned horses by way of experiment, and had found, after death, extensive inflammation of the stomach and intestines. The prisoner was condemned, and executed.

Let me not be misunderstood. I do not deny the diversity of action of poisons and medicines on different animals, but I decidedly agree in opinion with Dr. Christison, that "if the subject is studied more deeply, the greater number of the alleged diversities will prove rather apparent than real."

One objection to experiment on animals as a means of learning the operation of medicines, is that we can obtain little or no information respecting the peculiar influence which certain substances exercise in particular diseases. The beneficial effects of cinchona bark in agues could hardly have been discovered by comparative experiments, though the reverse has been asserted, as I shall hereafter notice.

A very important circumstance to bear in mind, in experiments on any animals, is that the effects of the same medicine vary with the part of the body to which it may be applied; for example, carbonic acid acts as a deadly poison when introduced into the lungs, whereas, taken into the stomach, it operates as an agreeable excitant. The volatile stimulants act more powerfully on the system when taken into the stomach, than when applied to the rectum.

In ascertaining the operation of remedial agents on man, it is necessary that we examine their influence in the two modes of existence—health and disease. A reliance on either solely may lead to most erroneous inferences. The beneficial influence of arsenious acid in agues, or in lepra, could never have been anticipated by any experiments made with this substance in health; nor could we form a correct notion of the effects and proper doses of opium by employing it in tetanus, nor by using mercurials in fever.

Thus, then, it appears, that although we may draw some general inferences as to the operation of medicines by the examination of their sensible, natural-historical, or chemical properties, yet the information thus gained is not sufficiently precise to be of much practical utility; and we are therefore necessitated to resort to careful observation of the effects on man and other animals, in order to arrive at accurate conclusions.

Cause of the effects of medicines.—Before I proceed to examine the nature of the effects produced by the action of medicines on the body, I think it expedient to bring before you some theoretical, or perhaps I ought to call them hypothetical, details, connected with our subject. Linnæus adopts as a canon a remark of Hoffman, that "*Duo in medicina fulcra sunt, ratio et experientia; experientia præcedit, ratio sequitur; hinc rationes in rebus medicis experientia non conditæ, nihil valent.*" I feel that I may be charged with inverting the order of proceeding inculcated in this sentence, by commencing my examination of the operation of pharmacological agents with subjects purely speculative. I feel also that this charge is so much the more valid, inasmuch as the hypothetical details in question lead to no practical results. But I would remark, that while, in framing general laws, it is essential we should be previously well conversant with particulars, yet it is not a necessary consequence that the same order should be followed in teaching; nay, in some instances such a mode of proceeding would be attended with manifest inconvenience. Thus, in explaining the law of universal gravitation, few persons would think it requisite to follow Newton through all those elaborate calculations by which he arrived at this general principle.

But it may be said, if a subject lead to no practical result, why discuss it? To this I cannot do better than reply in the language of Dr. Thomas Brown, in his work on the Philosophy of the Human Mind:—"To know well what hypotheses truly are in themselves, and what it is which they contribute to the explanation of phenomena, is, I am convinced, the surest of all preservatives against that too ready assent which you might otherwise be disposed to give them."

I am sure all of you will feel interested in the inquiry into the cause of those changes in the vital actions of the system produced by the agency of medicines. To what primary qualities are we to refer the vesicating property of cantharides, for example? This kind of discussion necessarily resolves itself into two parts—first, an examination of the agent (the medicine) producing the changes; and secondly, an inquiry into the nature and properties of the being in which these changes may be or are produced.

1. *Active force of Medicines.*—Medicines give rise to certain alterations in the system, which we denominate their *effects*; and we say, therefore, these agents are en-

dowed with an *active force.* But in what primary quality does this force reside? Some have directed their attention to the intimate structure, others to the chemical composition of medicines; thinking by these means to discover the cause of those changes produced on the living body by the agency of medicines. "I doubt not," says Locke, "but if we could discover the figure, size, texture, and motion of the minute constituent parts of any two bodies, we should know, without trial, several of their operations one upon another, as we do now the properties of the square or a triangle. Did we know the mechanical affections of the particles of rhubarb, hemlock, opium, and a man, as a watchmaker does those of a watch, whereby it performs its operations, and of a file, which, by rubbing on them, will alter the figure of any of the wheels, we should be able to tell before-hand that rhubarb will purge, hemlock kill, and opium make a man sleep." These mechanical notions of Locke harmonized well with those of the iatromechanical or iatromathematical sect of the age in which he lived: a sect which ranked amongst its supporters Borelli (its founder), Bellini, and others, in Italy; Sauvages, in France; and Pitcairn, Keill, Mead, and Freind, in England. They explained the functions of the body, the production of diseases, and the operation of medicines, on mechanical principles. The action of stimulants, for example, was supposed to depend on the pointed and needle-like form of their particles, and the operation of emollients on their globular form. I need hardly say, the existence of particles with the peculiar shapes assumed, is quite imaginary; and, indeed, if, for the sake of argument, we assume their existence, the action of medicines is, notwithstanding, quite inexplicable. We can, indeed, easily fancy that a ball of glass may be swallowed with impunity, and that the same substance, reduced to the form of a coarse powder, might cause considerable irritation by the mechanical action of the angular particles on the tender alimentary tube; but we cannot, on this hypothesis, explain why one medicine acts on one part of the body, and a second on another part. Quicksilver, tin-filings, and the hairs of the pod of the *Mucuna pruriens*, are still indeed employed, on account of their mechanical influence on the body. In the passage just quoted from Locke there are, in fact, two errors: first, the assumption that all the changes which take place in the external world are reducible to mechanical affections; and secondly, the presumption that we should be able to predict, without experience, the changes which would take place, supposing the assumption just mentioned were correct. A moment's reflection will satisfy us that, without experience, we should never have been able to predict occurrences far simpler than those just alluded to.

Chemical properties have been by some supposed to be those primary qualities to which we ought to refer the changes produced by medicines in the condition of the living body. At one time, indeed, it was thought that all the operations of the animal machine were dependent on chemical action. Diseases, for example, were supposed to depend on the too acid or too alkaline condition of the fluids, and medicines were presumed to act by neutralizing the one or the other of these states. Sylvius may be considered as the founder of the iatrochemical sect, while our countryman Willis may be regarded as one of its leading advocates. Although this sect has long since disappeared, among the revolutions to which medicine has always been subject, yet a modified trace of it may still be observed in the opinions of those writers who assert that to chemical properties we ought to refer the activity of medicinal substances. Barbier—one of the most respectable of French pharmacological writers, though one, I think, whose theoretical views frequently lead him to regard facts through a distorting medium — considers the active force of a medicine as consisting in a tendency which the particles of the latter have to unite with the constituents of the organized tissues. So that when a medicine is applied to the living body, its component parts endeavour to combine with the organic matter, the vital properties resist, and a new action is set up, which constitutes what we call the medicinal *effects*.

I would remark on this hypothesis, that some of our remedial means are undoubtedly to be regarded as chemical agents—for example, the caustics, properly so called; and it will be admitted, I think, that the influence possessed by alkalies and acids, in modifying the condition of the urine, is referable, in part at least, to their chemical peculiarities. In some other cases also, circumstances lead us to suspect the same kind of influence; thus we find, for the most part, that if we increase the affinity of a medicine for other agents, we frequently increase its influence over the body, and *vice versâ*. Thus the oxides of the metals are more active than the regulus, while sulphate of potash is much less active than either of its constituents.

Let us not, however, be led away by loose analogies. It is quite impossible, I

think, to admit that medicines generally are to be regarded as chemical agents. The few cases in which we have distinct evidence of a chemical operation are insufficient data on which to found a general law. Will any one be hardy enough to assert that the influence of hydrocyanic acid, or of opium, on the animal body, is of a chemical nature?

If, then, the greater number of our articles of the materia medica act, as far as we have evidence, neither by their mechanical nor by their chemical properties, to what other qualities are we to refer their operation? This question, gentlemen, is one which, in the present state of our knowledge, we are incompetent to answer. Considering the immense number of inorganic changes dependent on the agency of the electric fluid—taking into account also the wonderful relationship between electrical and vital phenomena, and the fondness of medical men for hypothesis—you will not be surprised to find that some have attempted an electrical theory of the operation of medicines. The instantaneous death caused by the application of concentrated hydrocyanic acid to certain parts of the body, is something like an electrical phenomenon.

2. *Vital properties.*—"Medicamenta non agunt in cadaver," is an axiom to which all persons will assent; and our inquiries immediately lead us to investigate the properties of living beings by which medicines are enabled to exercise their peculiar and remarkable influence. These properties or qualities are neither physical nor chemical: we denominate them vital or living: they are, therefore, essential to every organized or living part. Let me here caution you against confounding the terms *property* and *function*: the latter is the product of a peculiar organ or apparatus of organs, while the former is common to all living parts. Two vital properties have been usually described—the one called *sensibility*, the other *contractility*: by the first is meant the property or capability of receiving impressions, and by the latter, the capability of contracting or dilating—that is, of executing certain movements when the requisite impression has been made. A bean, for example, it is said, must possess both these qualities; for the act of growth is a proof that there exists internal movements; and these may be regarded as evidences of the reception of an impression of heat and moisture, without which germination cannot be effected.

But if the subject be a little more accurately examined, we shall find these supposed two vital properties are reducible to one. When a part feels an impression, it changes its mode of existence—that is to say, some kind of movement is effected in the part, which we can refer neither to physical nor chemical agencies. So that to assert a part possesses sensibility, is to express the capability of the part to act in a certain manner on the application of certain substances. Writers have in some instances described four, even five, vital properties. Bichat, for example, admits five; but three of these are evidently functions;—I mean perceptive sensibility, and the two kinds of sensible contractility (voluntary and involuntary). They are evidently performed by a distinct apparatus.

Whether we admit one or five, you will naturally inquire what agent confers this one, or these five, properties on organized beings? To this question no answer can be rendered. By one class of writers the phenomena of life have been ascribed to organic structure, just as the sounds of a musical instrument are referred to the mechanical arrangement of its parts; by another class it has been assumed that there exists a living internal principle (some have compared it to the electrical fluid), distinct from the body, and which is the cause of the organization. To enter into any discussion on these hypotheses would be out of place in a course of lectures like the present. I may refer such as are desirous of pursuing the inquiry to Dr. Barclay's work on Life and Organization.

The ultimate cause, then, of the operation of medicines (as well as of all other vital phenomena) is involved in impenetrable mystery. We know when a medicine is applied to the body it occasions some molecular movements to take place therein which are not sensible to us, and are only recognisable by their effects. But the cause and the nature of these motions are perfectly unknown. Let us turn this deficiency of knowledge to account; let it serve as a stimulus to our further inquiries. We ought, perhaps, as a celebrated German physiologist has observed, rather to be thankful that we have something yet to learn—something yet to obtain: for possession only makes us quiet, indolent, and proud. "If," says this writer, "the Deity held in his right hand all truth, and in his left only the ever-active impulse, the fond desire, and longing after truth, coupled with the condition of constantly erring, and should offer me the choice, I should humbly turn towards the left, and say, 'Father, give me this; pure truth is fit for thee alone!'"

[*Historical Table, &c.*

HISTORICAL TABLE OF MATERIA MEDICA.

BEFORE CHRIST. Chronology doubtful.	**EGYPTIAN MEDICINE.** First practised by Priests.—A temple erected to the Squill plant, under the name of *Crommyon* (Κρομμυον); Lapis ætites (a native oxide of iron); Fumigations with *Cyphi* (Κυφι), a mixture of 16 drugs; Slime of Nile; Nitrum (Carbonate of Soda?); Plasters and Unguents; Dietetical means. **HINDU MEDICINE.** Regimen; Unguents; Cataplasms; Fumigations; Baths. **HEBREW MEDICINE.** Hygiene; Cataplasms; Ablutions, &c.
To 430 B.C.	**GREEK MEDICINE.** BEFORE HIPPOCRATES.—1398 B.C. Melampus cured Madness by Hellebore, and Impotence by Iron Wine. 1270, Chiron the Centaur (*Chironea Centaureum*). 1263, Esculapius: his sons Machaon and Podilirius; the latter used Bloodletting. Hecate, Circe, and Medea, (Enchantresses), acquainted with poisonous plants. Aconite; Moly (Allium?); Nepenthes (Opium?). Asclepiadeæ, descendants of Esculapius. Mineral waters; Hemlock; Elaterium(?); Scammony; Euphorbium; Cnidia coccus (Daphne Gnidium?); Colocynth, &c. Silphium (Asafœtida?) discovered by Aristæus, 617. 580—500, Pythagoras: employed Magic, Dietetics, Mustard, Aniseed, Vinegar of Squills, &c.
From 430 B.C. to 150 AFTER CHRIST.	HIPPOCRATES. 460— (?) B.C. Hippocrates of Cos, the founder of Scientific Medicine. Employed Hygienic means (Diet, Baths, Exercise, &c.); Venesection and Cupping; Malva, Linum, Glycyrrhiza, Mel, Cera, Rosa, Gallæ, Punica granatum, Allium, Scilla, Veratrum, Helleborus, Staphisagria, Sinapis, Elaterium(?), Scammonia, Daphne, Ruta, Κανθαρις (Mylabris Cichorii?), Piper, Cardamomum, Cinnamomum, Cassia, Mentha, Anisum, Coriandrum, Cuminum, Anethum, Ammoniacum, Sagapenum, Galbanum, Silphium (Asafœtida?), Pix, Juniper, Styrax, Mastiches, Myrrha, Castoreum, Crocus, Conium, Hyoscyamus, Opium, Sulphur, Nitrum, Alumen, Sal commune, Cerussa, Acetas Cupri, Ferrum, Arsenicum, &c. ANCIENT DOGMATIC SCHOOL. 308 B.C. Founded by Thessalus and Draco (sons of Hippocrates) in conjunction with Polybius. NATURAL HISTORIANS. 350 B.C. Diocles of Carystus, Ριζοτομικα. 384—322 B.C. Aristotle on Animals (also Plants and Pharmacy). 371 to 286, Theophrastus of Eresus, founder of Botany. ALEXANDRIAN SCHOOL. 300 B.C. Medicine separated into Dietetics, Pharmacy, and Surgery. 295, Herophilus of Chalcedony employed Compound Medicines. 293, Erasistratus of Cos rejected Bloodletting: Simple Medicines. EMPIRIC SECT. 286 B.C. Founded by Philinus of Cos. 240, Serapion of Alexandria. Heraclides of Tarentum (the "Prince of Empirics.") Attalus. 138, Nicander, (Θηρ]ακα and Αλεξιφαρμακα). 135, Mithridates. METHODIC SECT. 100, B.C. Asclepiades of Bithynia: 60, his pupil Themison founder of the sect. All medicines Astringents or Relaxants. 54 A.D. (?) Pedacius Dioscorides, the most celebrated of the old writers on Materia Medica. Date uncertain. **ITALIANS.** 23 A.D. Menecrates; employed Escharotics; Diachylon Plaster. 50, Celsus (from 13 to 55.) Nourishing Glysters; Frictions with Oil. 70. Pliny, the Natural Historian; died 79. 230, Cœlius Aurelianus.
From 150 A.D. to 700 A.D.	GALEN TO THE FALL OF THE GREEK SCHOOL. 131—200 A.D. Claudius Galen, born at Pergamus. Diseases produced by alterations of the Humours, or πνευμα. Medicines act by their elementary qualities (heat, cold, dryness, moisture,) of each of which there are four degrees. This doctrine was held for 1400 years. 360 A.D. Oribasius. 550, Ætius: Camphor, Musk, &c. 560, Alexander of Tralles: Rhubarb, mild Laxatives. 650, Paulus Egineta.

AFTER CHRIST.	CHRISTIANS.	ARABIANS.
From 700 A.D. to 1100 A.D.	SCHOOL OF SALERNUM. Founded by Benedictine Monks: became celebrated about the eighth century. 1087, Constantine the African died. 1100, John of Milan (" Regimen Sanitatis Salerni.") 1110, Nicolus Præpositus. Diplomas granted. 1140, Matthew Plauter.	Doctrines of Galen. Mild Laxatives (Manna, Tamarinds, Senna, Rhubarb, Cassia) in place of Drastics. Chemical Medicines. Syrups, Juleps, Conserves, Loochs, Robs. Schools of Nisapour, Bagdad, Damascus, and Cordova. 702 A.D. Geber, the patriarch of Chemistry. 852 to 932, Rhazes. 865, Mesue the Elder. 880, Alkendi. 978 to 1036, Avicenna: for many centuries regarded as infallible. 1002, Serapion the Younger. 1017, Mesue the Younger.
From 1100 A.D. to 1500 A.D.	DARK AGES. Medicine practised by Monks, who mixed superstition, magic, and astrology, with the grossest imposition. 1215, Gilbert: Acetate Ammonia; Oil of Tartar per deliquium; Bath Waters. 1193—1282, Albertus Magnus. 1260, John of St. Amand. 1214—1284, Roger Bacon. 1240—1313, Arnold of Villa Nova. 1235—1315, Raymond Lully. 1317, Matthew Sylvaticus. 1343, Dondis (father and son). J. Platear. St. Ardouin: Hydrarg. Oxyd. Rub. 1394, Basil Valentine: Prepared Chemical Medicines; introduced Antimonials, Preparations of Lead, Ammonia, &c. 1407, Mercurial Fumigations. 1508, Guaiacum. 1518, Mercury internally. 1545, Rad. Chinæ. PARACELSUS. 1493—1541. Doctrine of Signatures. Overturned Galenism. Used Chemical Medicines; formed Tinctures, Essences, and Extracts.	

From 1500 A.D. to the Present Time.

GREAT BRITAIN.

1579 Winter's Bark
1683 Serpentaria (Johnson)
1674 Willis (Pharmaceutice)
1682 Lister (Mineral Waters)
1693 Dale (Dispensatory)
1695 Epsom Salt (Grew)
1697 Sulphuric Acid from Sulphur
1702 Mead (Poisons)
1736 Senega (Tennent)
1739 Spigelia (Brown)
1753 Lewis (Dispensatory)
1758 Kino (Fothergill)
1763 Willow Bark (Rev. E. Stone)
1770 Alston (Lect. Mat. Med.)
1774 Oxygen (Priestley)
1775 Digitalis (Withering)
1778 Simaruba (Wright)
1779 Red Bark (Saunders)
1780 Brown, (Elem. Medicinæ). All medicines regarded as stimulants.
1788 Angustura Bark (Ewer)
Monro, Donald (Pharmacy)
1789 Cullen, (Mat. Med.) A solidist. All medicines act by motions excited and propagated in the nervous system.
1791 Woodville, (Med. Bot.)
1793 Rhus Toxicodendron (Alderson)
1794 Yellow Bark (Relph)
1796 Pneumatic Medicine (Beddoes and Watt)
1800 Duncan, Jun. (Dispensat.)
1803 Cinchonia (Duncan)
1804 Murray (Mat. Med.)
1805 Hamilton (Purgatives)
1808 Pearson (Synopsis)
1811 A. T. Thomson (Dispensatory)
1812 Paris (Pharmacologia)
1813 Ainslie (Mat. Indica)
1813 Young (Classificat. Mat. Med.)
1820 Iodine in Bronchocele (Coindet)
1825 Phillips (Pharmacopeia)
1825 Brande (Pharmacy)
1829 Christison (Poisons).

GERMANY.

1536 Sulphuric Æther (Valer. Cordus)
1538 Cammerarius, Jun. (Bot.)
1631 Tartar Emetic (Mynsicht)
1658 Sulphate of Soda (Glauber)
1669 Phosphorus (Brandt)
1677 Wedelius (Pharmacy)
1679 Wepfer (Poisons)
1681 Nitric Æther (Kunkel)
1687 Cascarilla (Stisser)
1688 Magistery of Opium (i. e. Morphia: Ludwig.)
1701 Rivinus
1712 Kæmpfer
1717 Ol. Cajuputi (Locher)
1718 Hoffmann
1740 Neumann (Pharm. Chem.)
1758 Vogel, R. A. (Hist. Mat. Med.)
1760 Stœrck. Crantz
1767 Munch (Belladonna)
1774 Murray (App. Med.)
1790 Gren (Pharmacology)
1791 Arnemann (Mat. Med.)
1792 Trommsdorf (Pharmacy)
1793 Schlegel (Thesaurus)
1795 Gmelin (Min. Mat. Med.)
1800 Swediaur (Mat. Med.)
1802 Frank (Mat. Med.)
1802 Hayne (Botany)
1804 Morphia and Meconic Acid (Serturner)
1805 Bertele (Pharmacodynam.)
1807 Burdach (Mat. Med.)
1808 Pfaff (Mat. Med.)
1810 Hahnemann (Homoiopathia)
1816 Hüfeland (Consp. Mat. Med.)
Voigtels (Mat. Med.)
1819-22 Schwartze (Pharm. Tables)
1820 Tiedemann and Gmelin (Physiol.)
1821 Vogt (Pharmacodynam.)
Nees von Esenbeck (Botany)
1825 Bischoff (Mat. Med.)
1826 Bergen (Cinchona)
Richter (Mat. Med.)
1827 Geiger (Pharmacy)
Göbel (Pharmacognosia)
Brandt and Ratzburg (Med. Zool.)
1830 Creosote (Reichenbach)
1832 T. W. C. Martius (Pharmacog.)

AFTER CHRIST.

From 1500 A.D. to the Present Time.

FRANCE.

- 1544 Sylvius (Meth. Medic. Compon.)
- 1566 Antimony proscribed
- 1666 Antimony permitted
- 1672 Tartarized Soda (Seignette)
- 1694 Pomet (Drugs)
- 1697 Lemery (Pharmacopœia)
- 1700 Goulard (on Lead)
- 1702 Boracic Acid
- 1707 Kermes Mineral
- 1709 Chomel (Plants)
- 1717 Tournefort (Mat. Med.)
- 1723 Simaruba
- 1741 Geoffroy (Mat. Med.)
- 1762 Baumé (Pharmacy)
- 1780 Venel (Mat. Med.)
- 1789 Desbois de Rochefort
- 1803 Narcotine (Derosne)
- 1804 Decandolle (Med. Prop. Plants)
  - Alibert (Mat. Med.)
- 1805 Schwilgue (Mat. Med.)
  - Barbier (Mat. Med.)
- 1809 Magendie (Act. Strychni)
- 1812 Chlorides of Lime and Soda (Mazyer)
  - Picrotoxine (Boullay)
- 1813 Iodine (Courtois)
- 1814 Morphia (Robiquet)
- 1814 Orfila (Toxicology)
- 1817 Emetia (Pelletier and Magendie)
  - Medicinal use of Prussic Acid (Magendie)
- 1818 Stryçhnia (Pellet. and Caventou)
- 1819 Brucia (do.)
  - Veratria (do.)
- 1819 Loiseleur-Deslongchamps (Med. Plants)
- 1820 Quinia (Pelletier and Caventou)
  - Virey (Nat. Hist. Med.)
  - Guibourt (Drugs)
- 1821 Chlorides of Lime and Soda (Labarraque)
- 1822 Barthez (Mat. Med.)
- 1823 Richard (Med. Bot.)
- 1824 Fee (Nat. Hist. Med.)
- 1825 Chevallier
- 1826 Bromine (Balard)
- 1829 Merat & De Lens (Dict. Mat. Med.)
- 1832 Narceine (Pelletier)
  - Codeine (Robiquet)
  - Meconine (Couerbe)

SPAIN.

- 1530 Sarsaparilla
- 1538 Sassafras
- 1563 Garcias ab Orta (Drugs of India)
- 1574 Monardes (Drugs of West Indies)
- 1578 Costa (Drugs of East Indies)
- 1638 Cinchona
- 1787 Rodriguez Tavares (Pharmacol.)
- 1789 Rancé (Mat. Med.)
- 1796 Krameria (Ruiz)

ITALY.

- 1538 Mathiolus
- 1545 Brassavola (Simple Med.)
- 1592 Valerian (Columna)
- 1675 Calumba
- 1701 Sugar of Milk (Testi)
- 1734 Mazini (Medicam.)
- 1797 Brera (Medicines by Friction)
- 1798 Chiarenti (do.)
- 1808 Doctrine of Contra Stimulus
- 1811 Chrestien (Iatraleptice)
- 1817 Alberti (Flora Medica)
- 1826 Stellati (Mat. Med.)
- 1828 Bruschi (Inst. Mat. Med.)
  - Taddei (Pharmacology)

HOLLAND, &c.

- 1552 Jalap (Dodoens)
- 1574 Dodoens (Plants)
- 1576 Clusius (Plants)
- 1574—1644 Van Helmont (Chem.)
- 1648 Ipecacuanha (Piso)
- 1674 Maregraave (Mat. Med.)
- 1719 Boerhaave (Mat. Med.)
- 1799 Ypey (Mat. Med.)

SWEDEN.

- 1749 Linnæus (Mat. Med.)
- 1756 Quassia (Rolander)
- 1774 Chlorine (Scheele)
- 1778 Bergius (Mat. Med.)
- 1797 Björnlund (Mat. Med.)

AMERICA.

- 1798 Barton (Mat. Med.)
- 1817 Chapman (Mat. Med.)
- 1817 Bigelow (Botany)
- 1822 Eberle (Mat. Med.)
- 1825 Coxe (Dispensatory)

VARIÆ.

- 1588 Treatise on Medicines in Russian
- 1633 Simon Pauli (Veg. Med., Denmark)
- 1665 Botanical Garden at Moscow
- 1670 Borrich (Chem. and Bot., Denm.)
- 1673 Iceland Moss (Borrich)
- 1749 Haller (Botany, Switzerland)
- 1802 Ipecacuanha plant (Gomes & Brotero, Portugal)
  - Cinchonia (Gomes, Portugal)

HYDRIODATE OF POTASS IN ASCITES.

To the Editor of the Medical Gazette.

SIR,

I BEG leave to transmit to you the following case, as it illustrates well the happy effects of hydriodate of potass in ascites, depending upon hepatic derangement. Should you conceive it worthy a place in your valuable journal, you will oblige, sir,

Your obedient servant,

J. G. CUMMING,
M.R.C.S.E.

98, High-Street, Edinburgh,
Sept. 17, 1835.

Mrs. L., aged 56, of a delicate nervous habit of body, has for the last three or four weeks past felt her clothes getting gradually tight, from swelling of the abdomen, which swelling increased very rapidly the two days previous to that on which I saw her. I found the abdomen very perceptibly swollen, as if she had been seven or eight months pregnant; she complained of erratic pains passing through it; her legs were anasarcous; her bowels costive; her breathing frequent and interrupted; the pulse 120, open and compressible; and the urine dark, sparing, and voided with uneasiness. She mentions having experienced severe dyspeptic symptoms—diarrhœa, alternating with constipation, and considerable pain and uneasiness in the right hypochondrium, previous to the accession of the tumefaction, at which time she was conscious of having exposed herself to cold.

5th August.—I ordered

℞ Pulv. Jalap. C. ℈i.; Submur. Hyd. gr. vi. M.; and Supertart. Potassæ, to be freely administered.

6th.—The medicines operated freely; the pulse 70; and the swelling of the limbs somewhat less. The supertart. potassæ was ordered to be continued, and the preceding powder to be taken, the half at bed-time, and the remaining half in the morning.

7th.—The limbs the same; the abdomen fully as tense. The powder answered better in divided doses, and was ordered to be repeated in the same manner.

8th.—No improvement. The following mixture ordered:—

℞ Lig. Quassiæ, ℈ij.; Rad. Gentianæ, ℈j.; Aquæ Ferventis, ℥xvj.; Macera per horam, et cola. Liq. colat. add, Hydriodatis Potassæ, gr. xxxvj.; Carbon. Potassæ, ℈ij. A table-spoonful in a glass of water three times a day.

By the use of this mixture she so rapidly improved that in six days the effusion seemed completely absorbed; in short, by the administration of a single repetition of this mixture, and purgative pills occasionally, she was completely and permanently cured.

NOTICE OF DR. EDWIN HARRISON'S

MODE OF DISTINGUISHING THE BOUNDARIES OF THE LUNGS, LIVER, &c.

BY INSPECTION OF THE CHEST.

By DR. C. J. B. WILLIAMS.

WITHIN the last few years the physical signs of diseases have been continually receiving additions from several zealous and intelligent pathologists, both at home and abroad; and although even with these new aids perhaps none of us will speak on these subjects with the confidence which characterizes the writings of the sagacious but too sanguine discoverer of auscultation, yet the general knowledge of physical diagnosis may be fairly said to be considerably more extended than it was in his time. But the very extent of this knowledge becomes in practice no small obstacle to its being attained or used. The multiplicity of matters requiring attention, and the care and time necessary to make a physical examination unequivocal or satisfactory, have thrown many an impatient student back on the general symptoms, uncertain and fallacious as they are now known to be. Any mode of abridging or facilitating examinations which are tedious to the patient and laborious and perplexing to the physician, is therefore especially worthy of attention; and if it should have no other than these advantages, they are sufficient to recommend the simple fact which has been lately pointed out to myself and others by Dr. Edwin Harrison, at St. George's Hospital, and which, as far as he knows, has never before been noticed.

When the naked chest is viewed in a

good light, certain depressions and elevations may be observed, especially at the lower lateral and anterior parts, which are independent of the inequalities of the ribs. These Dr. Harrison considers to indicate the general shape and position of the contained organs, and he expects (I think with reason) that the observation of them may be of material use in exploring diseases of the chest. Thus the prominence caused on the right side by the liver gives a pretty distinct representation of the parts of that organ that are in contact with the walls of the chest; and the depression or furrow, which is discernible above it, generally marks the commencement of the lungs. This depression does not accompany the line of the ribs, but where it happens in its course to run between two ribs, that intercostal space is deeper than others. Above this furrow percussion will give the resonance of the lung; below it, the dead sound of the liver; and the distinction is generally plain enough to the eye to enable us to point out, before hand, what will sound clear and what dull.

On the left side there are also inequalities, which in a general way indicate the position of the several subjacent viscera, but not, as far as I have been able to judge, so accurately as those on the right side.

I refrain at present from entering into further details, and from dilating on the physical and pathological bearings of these matters, both because I have yet had little experience of their applications, and because the chief object of this communication is to announce them to the profession as original observations of my friend Dr. Harrison, whose peculiar repugnance to authorship deters him from doing so himself. He is investigating the matter further, and liberally wishes that others may be enabled to do so likewise, that the true value of these facts may be speedily estimated.

Half-moon Street,
Sept. 23, 1835.

TEMPORARY TRANSPOSITION OF THE HEART.

To the Editor of the Medical Gazette.

Sir,

In your valuable journal of the 22d of August, a case was reported by M. Raiken, of Volterra, of the temporary transposition of the heart; and I beg to forward a similar case, which was admitted into the Middlesex Hospital the latter end of last summer.

I am, sir,
Your obedient servant,
Edward W. Tuson.

Sept. 23, 1835.

Robert Ashby, a boy, about 13 years of age, was admitted into the Middlesex Hospital, September 21, 1834, having been run over by one of the carts employed on the rail-road near the Hampstead Road, the wheels having passed over his chest from the left to the right side. Upon examining the boy, the ribs were found to be much bent and flattened on the left side, causing the right side of the chest to project very considerably; at the same time no fracture could be discovered. There was much difficulty in breathing, the pulse very irregular, and the heart's action could not be felt on the left side, but on placing the hand on the right side of the sternum, its pulsation could be very distinctly discovered. It appeared that the force of the wheels had bent the ribs, and propelled the heart to the right side of the thorax. It was considered almost impossible for the boy to survive. He was bled two or three times while at the hospital, and saline medicines, with squills, administered. A few days after his admission the metallic tinkling was very distinctly heard at the upper part of the right side of the chest, and this sound could be heard for about three weeks, during which period the boy gradually got better, and left the hospital, nearly well, on the 14th of October, at which time the action of the heart could be felt in its natural situation, and the shape of the thorax had resumed its natural appearance.

ACCOUNT OF THE CEPHALOTRIBE OF M. BAUDELOCQUE.

To the Editor of the Medical Gazette.

Sir,

As I have not met with any notice of an instrument which appears to be calculated to effect a great improvement in the obstetrical branch of the profession, I have been induced to send you the following description of it. Should you

deem it worthy of a place in your valuable journal, you will, by inserting it, much oblige, sir,

Your most obedient servant,

JOHN-CHAS. COOKE,

Member of the Royal College of Surgeons of Edinburgh, and Licentiate of the Society of Apothecaries of London.

Coventry, Sept. 22, 1835.

In 1819, M. Baudelocque, nephew of the celebrated accoucheur of that name, presented a memoir to the Royal Academy of Sciences, entitled, "Du broiement de la tête de l'enfant, mort dans le sein de sa mère; nouveau procédé pour terminer l'accouchement laborieux;" in which he described an instrument invented by him for crushing the head of the fœtus when dead, and in this manner reducing it to the smallest possible dimensions. Before entering upon the description of the instrument itself, which he terms the cephalotribe, he points out the cases in which he would have recourse to it.

When the pelvis is so distorted, or so small, as to render delivery by the natural passages altogether impossible by the aid of the ordinary instruments, and when the child is still living, he considers the Cæsarean operation to be necessary; but when, on the other hand, the fœtus is dead, he maintains that we ought to have recourse to the cephalotribe, and to discard the perforator and crotchet entirely. He admits the difficulty of obtaining an accurate admeasurement of the diameters of the pelvis, and also the want of certain signs of the death of the fœtus. It is obvious, however, that these objections apply as forcibly to the old mode of proceeding, by the perforator and crotchet, as to the use of M. Baudelocque's instrument.

When we have at once the certainty of the death of the fœtus, and the impossibility of accomplishing the delivery by the aid of the forceps alone, the majority of authors recommend us to open the cranium, and to empty it of its contents, in order to destroy the disproportion existing between the child's head and the dimensions of the pelvis. Now although this may be done without risk to the maternal structure, yet, as it in no way affects the base of the skull, we are obliged to use the crotchet in order to break it up, as it frequently offers a most serious impediment to the termination of the labour.

And here the freedom from danger ceases, as even with the greatest precaution on the part of the practitioner the uterus and vagina will often be lacerated to a formidable extent, in consequence of the instrument slipping from its hold.

It has been stated by Sir C. M. Clarke, that a living child cannot pass if the conjugate diameter of the pelvis be only $3\frac{1}{4}$ inches, and that even this will frequently require the use of the perforator. Now the distance between one zygoma and the other being commonly $3\frac{1}{2}$ inches, and in some instances 4 inches, it is clear that the perforator can be of no advantage in these cases, as the incompressible base of the cranium will be wider than the aperture through which it has to pass.

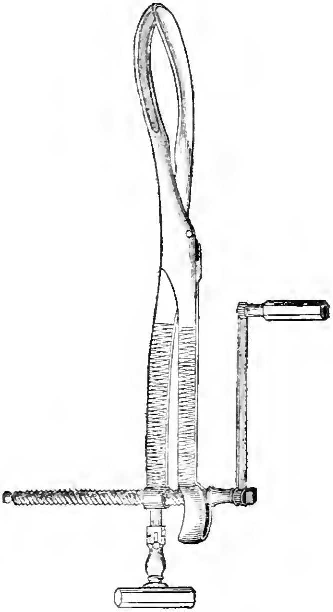
It is, then, the crotchet that M. Baudelocque proposes to supersede by the use of the cephalotribe.

In the report of MM. Boyer and Dumeril, who formed the commission appointed by the Academy to report on the value of the instrument, I find some observations on its weight, &c. These objections M. Baudelocque has obviated, by considerably reducing its size. The reporters conclude by saying, "L'invention de M. Baudelocque est une preuve de son zèle pour le perfectionnement de son art, et ce zèle nous parait digne d'éloges."

The cephalotribe consists, as the annexed sketch will show, of two blades, resembling in form Smellie's forceps, and locking in a similar manner. The blades have no fenestræ, are three lines thick, and but sixteen broad; so that M. Baudelocque has been able to introduce them into a pelvis the diameter of which was only twenty lines. Of course they would pass with facility when the diameter amounts to two inches. The length of the whole is sufficient to enable the operator to seize the head when placed above the brim. The curvature corresponds to that of the canal to be traversed.

The handles are an inch longer than the blades, as measured from the lock or axis of the instrument; they are five lines in thickness, and seven in breadth; they are each perforated at their extremities, to receive a screw six and a half inches long. To this screw a handle is attached, which measures six inches in length. The weight of the whole is about five and a half pounds, as manu-

factured by Sirhenry, of Paris. Probably, in the hands of an English cutler, its weight might be still farther reduced. The mode of introducing the blades corresponds in every respect with that usually observed in cases requiring the forceps. The head being firmly grasped, the handle is to be turned, when the cerebral mass is pressed out at the orbits, nares, ears, &c.; and the bones forming the base of the cranium are crushed by the compression exerted by the instrument. It is worthy of remark, that the scalp is never lacerated, and, consequently, no chance of injuring the uterus or vagina by the bones splintering exists.



I can myself bear testimony to the great physical power of the cephalotribe, as I have repeatedly seen the Professor crush the head of a full-grown fœtus, and in one instance that of a child a month old. In none of these instances was the scalp torn even in the smallest degree.

In a letter dated July 17, 1835, M. Baudelocque informs me that he has operated seven times, although he has published the details of five cases only. One of the unpublished cases occurred whilst I was in Paris, and an opportunity was afforded me of witnessing it. MM. Paul Dubois, Barbette ainé, and Rivallié, have operated four times; so that altogether there are eleven cases on record in which the cephalotribe has been used, and in all, I believe, with success. In all, too, the Cæsarean operation had been considered to be absolutely necessary, from the impossibility which existed of delivering by the crochet.

The details of these cases will be found in M. Baudelocque's "Mémoire sur le Cephalotripsie," and in the number for April, 1835, of the "Journal de Médecine," edited by M. Lucas-Championnière.

M. Baudelocque has invented other instruments and processes, which may form the subject of a future communication.

EXAMINATION OF PANIZZA'S OPINIONS

ON THE

FUNCTIONS OF THE LINGUAL BRANCHES OF THE FIFTH AND EIGHTH NERVES.

To the Editor of the Medical Gazette.

Sir,

In an abstract, by Dr. George Burrows, of "researches relating to the nerves," by Professor Panizza of Pavia, printed in the two last numbers of the Medical Gazette, evidence is adduced to shew that the lingual nerves have in some respects different offices to those hitherto assigned to them by physiologists.

The lingual branches of the ninth (*motor linguæ*) are held by Panizza to be the voluntary nerves of the muscles of the tongue, in accordance with the opinions of other physiologists.

The lingual branches of the fifth (*nervus gustatorius*) are held by the Professor to be nerves of touch, which again accords with the opinion generally received; but he argues against their being nerves of *taste, as well as of touch*, which they are commonly esteemed to be.

The glosso-pharyngeal nerve he supposes to be the nerve on which taste depends.

Upon carefully weighing the evidence adduced by Panizza, I find myself unable to subscribe my assent to his conclusions. My conviction remains the same as it was before I pe-

rused Dr. G. Burrows's paper—namely, that the lingual branches of the fifth are for taste as well as touch; and that the glosso-pharyngeal has a different office. I believe some of the filaments of the glosso-pharyngeal to be sentient, others voluntary—the sensations, to which the nerve ministers, being those which alone can be excited at the root of the tongue, namely, vague sensations of touch, which, when kept up an instant, produce convulsive deglutition, nausea, and spasmodic elevation of the pharynx;—the voluntary actions, to which the nerve ministers, being the motion of some of the upper pharyngeal muscular fibres in swallowing.

The remarks which I have to offer on the present occasion will be directed to two objects: first, to shew that the experiments of Panizza admit of a different interpretation to that which he adopts; secondly, to recapitulate the evidence in proof of the functions of the nerves in question being what I represent them to be.

I.—*Experiments by Panizza considered.*

1. *The lingual branches of the fifth pair divided in a dog.*

"If some milk, or bread, or meat, is offered to the dog, he eats and drinks readily, although, after the operation, it sometimes appears that the dog laps up the milk and masticates his food rather slowly, which may arise from the loss of the sense of touch, or in consequence of the deep wound. If, on the other hand, just enough colocynth, or infusion of quassia, be added to the milk, so as to give a bitter taste without altering the colour, or a piece of bread be dipped into this milk, or, indeed, merely a few drops of the bitter liquid poured between the fibres of a piece of flesh, the animal, which up to this moment had evinced the strongest desire for meat and drink, immediately refuses both one and the other, after taking one mouthful of either. If a piece of meat prepared with the bitter solution is mixed with several others, and the dog accidentally take it into his mouth, he immediately rejects it, and often refuses to eat any more *."

Objection.—The disagreeable impression which the dog evinced may have resulted from the bitter being *tasted* by the soft palate. This objection was foreseen by Panizza; accordingly, Dr. G. Burrows goes on to say—

2. "These facts alone would not prove that the sense of taste still exists in the tongue, because the bitter flavour may be perceived by other parts of the mouth. In order to dissipate all doubt, it is therefore desirable that the bitter substance should merely touch the tongue itself; and to accomplish this, it is best to take a small feather, previously slightly dipped in the bitter fluid, and to draw it lightly along the dorsum of the tongue, taking great care that the fluid does not extend beyond the points touched by the feather. With these precautions the animal still evinces the same marks of strong distaste for the flavour."—Pages 850, 851.

Objection.—In this experiment the dog's mouth must either have been held open all the time of the observation, or have been allowed to close. In the latter case the bitter would have been applied to the soft palate, an organ of taste. In the former it is not singular (without supposing the creature to have been unpleasantly affected by the *bitter*) that it should have moved its tongue and mouth, as if it experienced something disagreeable there.

3. *The glosso-pharyngeal nerves divided in a dog.*

"As soon as the dog had a little recovered himself after the operation of the division of the glosso-pharyngeal nerves, he licked up some water, and ate as freely as if no injury had been done to him. The animal had no other guide than the sense of smell in the choice of his food, so that he will equally take into his mouth disagreeable and hurtful substances as well as pleasant and wholesome. The dog after this experiment ate with equal voracity plain meat as well as that imbued with colocynth, and equally drank the plain milk and that rendered bitter by colocynth. The dog not only devoured a piece of meat which had been beaten up with some colocynth, but even lapped up the remainder of the liquid in the plate."—Page 885.

Objection.—This experiment proves too much. The middle of the soft palate in man, and it is presumable in dogs, enjoys a strong sense of taste: how came that part not to be disagreeably affected with the colocynth? Be-

* Med. Gaz. vol. xvi. p. 850.

sides, there is a tendency in animals, when wounded, to feed; there is likewise a difference of sensibility in different animals; and it is very possible that after such a wound as had been inflicted in this case, the animal should disregard a moderately unpleasant *taste*, when combined with an agreeable *flavour*—that, namely, of milk.

4. Dr. G. Burrows continues—

"Similar experiments were made at the same time with a dog whose lingual nerves of the fifth pair had been divided. After this latter animal had caught in his mouth several pieces of plain meat which were thrown to him, he readily took in the same manner a piece with the bitter *flavour* *; but he had scarcely got it *into his throat* before he was seized with vomiting, and he rejected this piece."—Page 885.

Objection. — The direct conclusion from this experiment is, that the animal had no taste in the fore part of his tongue [*the lingual branch of the fifth being divided*]; but that the soft palate tasted the bitter; and perhaps the root of the tongue, through the glosso-pharyngeal, although it did not taste the bitter, might vaguely *feel* its astringency.

5. Dr. G. Burrows continues—

"A most remarkable contrast between the dogs was now observed; for the animal whose glosso-pharyngeal nerves were divided, immediately ate this rejected piece of meat. Nevertheless, this latter animal still preserved his sense of feeling in the tongue; for immediately on its being pricked the dog howled, and attempted to run away."—P. 885.

Objection.—The natural conclusion from this experiment, taken alone, I admit to be, that the division of the glosso-pharyngeal nerves destroyed the sense of taste. Yet it appears equally certain to me that the dog, in this experiment, *may* have tasted the bitter, yet in spite of it, have been attracted by the flavour of the meat. I beg likewise to refer the reader to the *objection* subjoined to the third paragraph quoted.

II. The experiments of Panizza appearing to me thus to give plausibility to his opinions, but by no means to carry conviction, I proceed to recapitulate the evidence in favour of the opinions which I maintain upon this subject.

1. In the year 1821-2, I performed the experiment in different animals of dividing the ninth nerves and the lingual branches of the fifth. The result of trials made on these animals, which, however, I will not assert were made with as much care as Panizza's, led me to believe that the ninth is for motion, the lingual branches of the fifth for taste and touch jointly.

2. On Monday last I divided, in a dog, on one side the trunk of the glosso-pharyngeal nerve, and on the other, the lingual branch of the same nerve. Mr. Kiernan and Mr. Skey, with whose assistance I made this experiment, (and through whose obligingness I unexpectedly had the opportunity of making it), agreed with me in thinking that the gestures of the animal, when the extract of colocynth was applied to its tongue, evinced that it tasted it.

I must confess, however, that I am disposed to draw conclusions with great caution from experiments on the sensibility of animals after the division of nerves. When nature, in place of being interrogated, is put to the question, she often, like the tortured criminal, gives false answers. But setting my own experimental observations against Panizza's, as sufficient to throw some doubt upon his observations, I think the following argument, derived principally from anatomy, perfectly conclusive on the question.

The sense of taste is most acute at the tip and edges of the tongue. But anatomy shews that branches of the glosso-pharyngeal nerve do not reach the tip of the tongue. Branches of the fifth and of the ninth are distributed there. And as the latter are admitted at all hands to be voluntary only, or not to impart either taste or touch, it follows that the taste and touch of the tip of the tongue must depend upon the lingual branches of the fifth.

When I assert that the sides and tip of the tongue are the seat of the acutest degree of taste, I state a fact which I very carefully verified with Mr. Wheatstone, before the publication of the third edition of my "Outlines of Physiology." It surprises me, therefore, that so good

* The word flavour is here misapplied; *flavour* means a sensation excited in the nose; *taste* alone is excited in the fauces. A dose of salts and senna excites a saline joined with a sweetish taste in the fauces,—together with a peculiar *flavour* in the nostrils.—H. M.

an observer as Panizza should represent the *base* of the tongue as having the highest degree of taste.

Dr. G. Burrows gives the following as Panizza's:—

"In fact, when the glosso-pharyngeal nerve is diligently examined, either in man or animals, as in the dog upon which these experiments were principally performed, it appears that, without giving off a single filament to the muscles through which it passes, the nerve is entirely distributed to the mucous membrane of the tongue and to the surrounding parts, which, in common with the tongue, enjoy the sense of taste: this sense is most exquisite there, where the filaments of the nerve are most abundant — that is, towards the base of the tongue."—P. 885.

What appears to me the most interesting part of Dr. Burrows's communication, is the case which he quotes from the Medical Gazette, vol. xv. p. 120, reported by Mr. Noble, of Manchester.

The patient whose case is reported has want of feeling of one side of the face, from an affection of the fifth nerve of that side. The half of the tongue belonging to that side does not feel, but it *tastes*. It is evident, however, that this remarkable case does not prove the glosso-pharyngeal nerve to be the nerve of taste: it only proves that in hemiplegic anæsthesia of the face, taste may be unaffected, when touch is extinguished. It does not even prove (although, taken with other facts, it renders the conclusion highly probable), that there are different nervous filaments for touch and taste upon the tongue. These *different* nerves (which certainly I am disposed to believe exist) for touch and taste, I think we may fairly conclude to be different filaments of one nerve, and that nerve the lingual branch of the fifth.—I am, sir,

Your obedient servant,

HERBERT MAYO.

19, George-St., Hanover-Sq.
Sept. 30, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

The Anatomy of the Regions interested in the Surgical Operations performed upon the Human Body; with occasional Views of the Pathological Conditions which render the interference of the Surgeon necessary. In a Series of Plates, the size of Life. By J. LEBAUDY, M.D. with Additions. London, 1835. Baillière.

UNQUALIFIED praise implies all possible excellence: for this reason alone we withhold it here. We wish the author had given, with the other engravings, one of the anatomy of the perineum: the omission is the more to be regretted, as all the other views are admirable in design, execution, and (what of course constitutes their chief practical value) the instruction they afford.

For *merely* surgical purposes, these plates are incontestibly the best we have seen; and they moreover possess a claim to preference which many purchasers will be feelingly alive to. They are the *cheapest*. Until the public more highly appreciate and better reward medical men, they must not complain of a niggard economy in the investment of professional capital. The practitioner whose remoteness from cities denies him all the advantages of frequent intercourse, *ought* to be possessed of such a work as M. Lebaudy's; and although we could easily indicate more costly and expanded ones, it would embarrass to point out one more perfect in all the *essentials*. The provinces contain much "patient merit," often distrusting itself upon occasions where confidence would be justifiable; this confidence would be frequently furnished by such a work as the present; and sometimes the patient, spared the fatigue and expense of a journey to the metropolis, will remain to reward the skill and enterprise of the country surgeon.

It is not that we hold these helps to be indispensable, but we are sure their usefulness cannot be denied.—A well-instructed surgeon may have great general fitness, combined with *particular* inaptitude, arising from the *infrequency* of employing the acquisitions made at the schools—acquisitions which, from necessarily falling into desuetude, lose that freshness which constitutes half their value.

Works like the one before us tend to give a perennial character to that knowledge that for lack of use is in danger of *ærugo animi, rubigo ingenii*. We are concerned to be obliged to withhold approval of the view of the "six-month fœtus," which, although a plate of great

beauty and correctness, is wholly out of place and uncalled for, as it refers to no case or contingency involving an operation to which the engraving could contribute any advantage.

We are not satisfied of the necessity for exhibiting the cicatrices of burns, for they are never twice alike, though their effects be generally the same, viz. deformity and impairment of motion. It is not remarked, what will be too often found, that the mere *division* of the bands must be insufficient. Mr. Earle has succeeded in curing this painful deformity, by *dissecting off* the product of cicatrization; the new product by granulation of an incised wound not restraining motion like the tough condensed structure constituting the cicatrix from torrefaction.

We are on the whole highly pleased that this collection of M. Lebaudy's has been placed within the reach of British practitioners: it is entitled to the best encouragement, both as a work of art and utility.

MEDICAL GAZETTE.

Saturday, October 3, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

ILLUSTRATIONS OF IMPARTIALITY.

It has often been remarked, that when a habit of lying has once been formed, the practice gradually increases till the individual at length becomes almost insensible to the distinction between truth and falsehood, and thus makes such unconscionable drafts upon the credulity of his hearers, that even the simplest cease to be imposed upon. Such is the present predicament of a worthy hebdomadal contemporary of ours, who at this season of the year invariably puts forth a manifesto, addressed to pupils, in which he informs them, in the kindest and most disinterested manner, what schools they are to attend and what avoid; lauding some, and condemning others, in a manner which at first sight seems quite incomprehensible, but with as much appearance of candour as if he were quite uninfluenced by any private considerations or personal feelings. Does any one wish for a key to the mystery?—here it is:—let the teachers in a school send a good advertising order, and they will at least escape abuse; but if, in addition to this, they patronize the journal, and, most of all, if they countenance the man (as certain Professors in Gower-street do), they will then be sure to reap a rich reward, in the *honourable* notice taken of them in the Lancet. But alas for those who neglect these precautions!—and more especially if, like some rash and infatuated lecturers in the west, they openly discountenance his journal, and send not even one solitary advertisement to adorn its wrapper and mollify its Editor, then woe betide them!—the door of mercy is shut against them for ever. In short, we assert that schools and teachers are praised or dispraised exactly as suits the interests of our notorious contemporary;—that the very same things are lauded at one establishment which are condemned at another;—that facts as to the expense at different institutions are designedly misrepresented;—that those who have been the objects of ridicule or of invective obtain a truce, or even procure his patronage, as soon as they become opposed to others whom he hates still more;—in a word, that there is no insult which a journalist can offer to the public, which is not almost weekly repeated by our contemporary, who either assumes that his readers are aware of his falsehoods, and yet support his journal, which makes them out to be knaves,—or that they believe the mass of contradictions and absurdities which he indites, and consequently that he holds them to be fools.

If, in support of these assertions, we take up but a number or two of the Lancet, the proofs so crowd upon us

that the difficulty lies only in selecting. We shall at present illustrate one point—its gross, wilful, and interested partiality; and we choose this subject the rather because it is diametrically opposed to the principles on which the journal was founded, and on which it professes to be conducted.

It is well known to those who attend to such subjects, that ever since the North London Hospital was established it has been the leading object of our contemporary *to write it up*. For this purpose every thing connected with it has been systematically represented as affording a brilliant contrast to all similar institutions in this country; *there* we have no "jobbing," no "hole-and-corner work," no "corruption," no "abuses." Now the great complaint against the other hospitals—that which is said to be the root of all the evils which exist—is the mode of electing the officers. It is not our present purpose to inquire whether this be so or not; but as such is the recorded opinion of the Lancet — oft reiterated, and with many an invective — why, we ask, did not the same denunciations, and the same virtuous indignation, attend the election of officers to the Valetudinarium? Election! said we? there was nothing of the kind; the officers were installed as a matter of course, without even the form of an election. We do not object to this, under the circumstances; but then we do say that if the same thing had been done elsewhere, the Lancet would have been trumpet-tongued upon the subject.

It will be said, perhaps, that this was only at the first establishment of the hospital, and that the appointment of the professors in the *soi-disant University* was unavoidable. Be it so: but then comes the additional surgeoncy; how was that managed? Was the vacancy advertised? Were candidates invited? Was an election made? Was the most eligible chosen by the subscribers; or was it done by the *concours?* Will it be credited that the appointment was made by none of those modes! that it was matter of private arrangement! that it was a business of negociation! that it was done secretly and in silence! Certain parties cast about for a surgeon; they treated with Mr. Tyrrell; they treated with others; and, finally, they ended by coming to an agreement with Mr. Liston, who, it was arranged, was to be surgeon to the North London Hospital, while the importance attached to his acquisition was proved—by his being permitted to lecture in another school! Where, meanwhile, was he who professes to guard the profession and the public from all "hole and corner" proceedings, when this singular mode of *electing* an hospital surgeon was adopted? Where did the patron of the *Concours* hide his "manly" head*? We know not where,—perhaps at a *conversazione* in the London *University*, learning manners, against the conference with the Lords; perhaps at the House of Commons, trying how he might best oppose the bill against the pirating of lectures. But it matters not where; Cerberus was pacified with a sop; and means have been found to render the dog that bays so loud in other places muzzled and dumb in Gower-Street.

Again, we have been told, and it has been repeated *usque ad nauseam*, that the school in Gower-Street is cheaper than any other which claims to be respectable. This is not the fact, as we formerly demonstrated, by giving a tabular view of the fees at the various institutions in London, by which it was proved, beyond the possibility of contradiction, that the establishment in Gower-Street was very considerably more expensive than any other school

* See an extract in various London papers, from the Maidstone Gazette.

in the metropolis: since that time, and probably in consequence of our exposé, the fees have been somewhat reduced; but for unlimited attendance, still remain by much the highest in London. But it is said that the fees at the Valetudinarium are—as undoubtedly they ought to be—much less than at the great hospitals: let us see. The perpetual ticket is certainly less than at the large hospitals; but between the expense for the time and manner in which the immense majority of pupils attend, the difference is a mere trifle. Thus the common fee for attendance on the medical practice of the large hospitals during one year (lately increased to eighteen months on the same terms), is fifteen guineas. But hear what the prospectus of the North London Hospital says:—"For attendance during one year on the physicians' OR surgeons' practice, *separately*, 15*l.* 15*s.*"!! So that the medical practice there, is actually as expensive as at hospitals containing three or four times the number of patients. For attendance on the surgeons' practice at various other, and much larger, hospitals during one year, the usual fee is twenty guineas, or five guineas more than in Gower-Street; an excess, however, which has far more than an equivalent in the superiority of the opportunities of acquiring knowledge. It is not the mere difference of money asked or given that makes one thing cheap and another dear—something depends on the value of the articles; and estimated by this rule, the fee at the North London Hospital is preposterously high. How, then, is it that attendance at the Valetudinarium can be made to appear in any sense less expensive than elsewhere? Why, except as regards unlimited attendance, it is done by a cunning and unworthy manœuvre. The ticket, though nearly the same price as elsewhere, if the pupils profess to do no more than they profitably can, is yet much cheaper if they pretend to do, what we boldly assert no pupil can do, or even attempts, for any purpose but to obtain a certificate—namely, attend both the medical and surgical practice at once; that is, the practice of seven different persons at the same time: "attendance (says the prospectus, and echoes the Lancet) during one year upon the physicians' and surgeons' practice, 21*l.*" The plain English of all which is simply this:—The fee for the medical and surgical practice "separately,"—the mode in which alone it can be profitably attended, is very nearly the same at the North London as at the other metropolitan hospitals; while, viewed in relation to the variety of cases, the number of dresserships, and other advantages, it is infinitely higher;—but then, as aforesaid, the fee for a *certificate* of medical and surgical attendance is greatly less.

The mention of the dresserships reminds us of another point. The Editor of the Lancet has thought fit to institute a comparison between *his* hospital and St. George's, in reference to certain advantages enjoyed by pupils. "The dresserships (he informs us) at the North London Hospital are free to all perpetual pupils without the payment of any fee!" By which it appears that the dresserships *are confined to the perpetual pupils;* a limitation which becomes necessary in so small an hospital, and is no doubt judicious; but at St. George's, which our contemporary has himself selected for comparison, we find that "pupils entering for twelve months are allowed to dress the patients for three months without additional fee." This is rather a blunder on the part of our *honourable* opponent; but in truth the whole article alluded to is in a strain which admits of but one reasonable conjecture as to the condition of the writer when he penned it. Thus, among various other flights of fancy, he is pleased to assume that the

expenses of a pupil at St. George's, for board and lodging, would come to 120*l.* per annum, the gross amount of which, for three years, in defiance of all the rules of Cocker, he discovers to be 380*l.*!—and while the items, taking them at his own extravagant calculations, give a sum, the "tottle of the whole" of which even his friend Mr. Hume could only make 480*l.* 7*s.*, he, in adding up, finds to be 600*l.* 7*s.* * !!

But the inconsistencies are endless; for again, as we have seen above, in order to make up a fictitious amount, the expense of the pupil for board and lodging is calculated at 120*l.* per annum; and yet the amount charged by the Hospital for his board and lodging, namely, 50*l.*—(or 70*l.* less than the Lancet's estimate)—is printed in italics, and declared to be a "precious picture." What can the blockhead mean? Surely either the charge is moderate, or his own calculation is extravagant! But to reason upon such data is in vain; and unless the conjecture we have hazarded above afford a clue to the origin of this tissue of blundering balderdash, we must leave the cause of the mystification unexplained.

In every thing the same gross partiality and favoritism are manifested, and stand contrasted with corresponding demonstrations of jealousy and malevolence. At the Gower-Street School the system of giving prizes was very judiciously adopted, and the Lancet teemed with flaming accounts of the examinations, meetings, and distributions, therewith connected. The same plan was adopted at St. Bartholomew's, and it is now declared to be "all humbug and quackery"! Does any one for a moment doubt that this originates in his old grudge against Messrs. Stanley and Earle, and his newly-declared war against Mr. Lawrence?

Great was his indignation when it was insinuated that the Valetudinarium was not quite so large as could be wished:—now he ridicules and sneers at the Charing-Cross Hospital; yet there is little or no difference in the size of the two. How comes it that his kind anxiety to guard the pupils of King's College from the evils of insufficient clinical opportunities extends not to his young friends in Gower-Street?

There are two schools at Hyde Park Corner—one in the Hospital, and one adjoining; at neither is a course of chemistry given, an omission which is understood to be owing to the acknowledged excellence of the lectures of Messrs. Brande and Faraday, in Albemarle-Street. The Lancet draws up a spurious prospectus of the Hospital-School, and introduces the words "Chemistry—none given:" he also gives a prospectus of the other school, and announces "Chemistry—at the Royal Institution;" by which, of course, he means to imply that there are chemical lectures connected with one school, but not with the other—whereas the chemistry is equally in the vicinity of both, and equally independent of them. So also with regard to anatomy: at the Hospital-School there is said to be "none given," although the new theatre and whole establishment in Kinnerton-Street are inferior to none, and the dissecting-room superior to any in London. We observe, too, that the pupils are advised to attend the lectures which have been set up in opposition to those in the Hospital. Now whether this advice be good or otherwise, we cannot pretend to say; but we happen to remember that some of the principal teachers now patronised by the Lancet have heretofore been repeatedly attacked in it; and, indeed, it is but very lately that one of the chief of them was week after week held

* The sums (see Lancet, September 5, p. 745), are—

	£	s.	d.
	16	16	0
	1	1	0
	52	10	0
	50	0	0
	360	0	0
Total......	480	7	0
But, according to the Lancet	600	7	0 !!!

up to ridicule and scorn in its consistent pages! All this to the uninitiated might seem the very lunacy of malice, but yet it works by method, and proves what we said before, namely, that they who have been abused by the Editor of the Lancet may obtain his favour by becoming opposed to those whom he hates yet more. Who can fail to see, in these paltry manœuvres of our contemporary, the workings of that hostility to Sir Benjamin Brodie which betrays itself on all occasions—of that vexation with Dr. Seymour which his active opposition of the Lancet's protegés at the late discussion on the laws of the hospital engendered—and of that hatred of Dr. Macleod which has rankled for years in his bosom as—

"A worm that will not sleep, and never dies."

We undertook to prove the Lancet's partiality, and such are some of our illustrations; many remain behind, but our space shortens, and enough, we think, has been done to open the eyes of the most simple, and to convince the youngest tyro that none but a veritable *Johnny Newcome* would put himself in the leading-strings of such a guide.

EASTERN PROVINCIAL MEDICAL ASSOCIATION.

A meeting, which was very numerously attended, took place at the Guildhall, Bury St. Edmund's, on Friday, the 25th ult., for the purpose of forming a society, consisting of medical practitioners residing in the adjoining counties. The immediate objects in view are the collecting of information relative to medicine and the collateral sciences, and maintaining the respectability of the profession by promoting friendly intercourse between its members. Dr. Probart presided, and Mr. Crosse, of Norwich, acted as honorary secretary. The latter gentleman addressed the meeting at considerable length, and forcibly illustrated the advantages likely to accrue from such an institution. Many other gentlemen afterwards expressed their sentiments; and the establishment of an "Eastern Provincial Medical and Surgical Association" was unanimously agreed to. We regret that our space will not permit us to give insertion to the speeches made on the occasion; they will be found at length in the Bury Herald.

HOSPITALS AND THEIR REGULATIONS.

Our respected contemporary, the Medico-Chirurgical Review, after noticing a nonsensical tirade which lately appeared in a weekly journal under the above title, proceeds to make the following observations:—

The gist of all this lacrymation is, that hospital surgeons and physicians are paid. Really we see no abstract injustice in this; on the contrary, we think, and many others think so too, that the public services of medical men are not paid enough. A ruinous competition has ground down the profession into a starveling race—fellows who, like the opposition coachmen, not only take their passengers for nothing, but regale them with brandy and water on the road. Our dispensaries and many of our infirmaries have the services of physicians and surgeons for nothing—our hospitals have those services gratuitously also, so far as the hospital funds are concerned—and the remuneration, such as it is, is derived from the attendance of pupils. Now we do not see that Mr. A. or B. has any natural or indefeasible right to come from Yorkshire to London, to walk into the first hospital that presents itself, and accosting an *unpaid* physician or surgeon, to request him to show him his practice, explain his views, and exhibit his skill in the manner best adapted for his convenience. We find no such principle laid down even in 'The Rights of Man, by Tom Paine,' and we think it would be difficult for the most ingenious Owenite to prove it. But setting aside the claim to remuneration on the part of the medical officers of hospitals, and disregarding rights, let us look upon the question as one of expediency. In the first place, it may be stated as a general law that men work better when paid for their exertions than when unpaid. Perhaps editors are a class of men whose liberality is as great as can be expected from any members of a civilized community. None are so eloquent as they on the necessity and advantages of liberality and

disinterestedness — none can be more earnest in pointing out abuses, and jobs, and peculation. Yet in practice we find editors work nothing the worse for having a tolerable salary from a publisher, or a copious list of subscribers. The most liberal and patriotic writer never, so far as we can see, complains of finding his liberality and patriotism damped by their reward. Nay, singular as it may appear, the more he is encouraged, the more money he makes, the warmer is the complexion of his liberality. Were a new reforming journal to be established on the broad bottom of no pay for any person concerned in its writing, publishing, or printing, we are inclined to think there would not be a very lively contest for the situations of editor, publisher, or printer. In short, look where we will—at the bar—the pulpit -the judicial bench—the ministerial office—the ‘ gallant ship’—or the embattled field—we see that pay for services performed is admitted as a principle, and acted on in practice, with the best effects. Why, then, should the unhappy hospital functionary be singled out as the only man who is to work for nothing—why are the feelings of human nature to be disregarded in his case—and why is he to be supposed capable of doing what nobody else is asked to do, to work well and willingly without reward?

No doubt the principle of pay may be admitted, yet exception may be taken at its mode and its amount. In the case of the hospitals, we think that the method of remuneration is by no means bad. As the charitable institutions greatly depend upon private subscriptions, and as, whether that be so or not, the funds were intended, as exclusively as possible, for charity's sake, the less taken from those funds, for purposes not strictly charitable, the better.

If a man becomes surgeon to a hospital where he is not paid, he binds himself to perform certain duties to the patients; and if those duties are not performed, the persons who elected him have, or ought to have, the power of punishing him by suspension, or even by dismissal from his office. But pupils may be said to constitute an excrescence on a charitable institution—they are not essentially connected with it. Unless it be a surgeon's or physician's interest to pay the utmost attention to pupils, there is and there can be no efficient mode of preventing his neglect of them. The mere routine performance of specified duties is utterly inadequate to promote their advancement—there must be a disposition to direct, encourage, and assist. That disposition, we repeat, can be ensured by remuneration only. And from whom can remuneration more properly come than from those who are to derive advantage from it?

If it were otherwise an object of importance to protect the pupil from expense, if it were better that he should pay no fees, and that his entrance into the profession should be made as cheap as possible, we might feel inclined to look for some other mode of payment than such fees afford. But, so far from it being advisable to render admission into the profession very cheap, we think it would be well if it were more difficult than it is. Our ranks are at present more than sufficiently crowded, and the rate of profit is fearfully beaten down by the ruinous struggles of starving competitors for bread. Not a town, not a hamlet, but bears evidence to this—not a medical practitioner but directly or indirectly feels it. It is an evil that has increased, is increasing, and ought to be diminished. Let the common sense of our readers decide, whether that evil will be decreased by greatly cheapening medical education. For our own parts, we do not hesitate to express our opinion, popular or unpopular, liberal or illiberal as it may appear. We think the facilities for reception into the profession should not be too great; but we do not think that, to effect the desirable purpose of preventing an indiscriminate rush into our ranks, arbitrary or unreasonable tolls should be levied. Education should not be merely made dear—it should be dear only by being extensive. Whether our numbers be great or small, our real respectability can only be secured by securing the highest amount of attainments.

We agree with our contemporary, that the offices of house-surgeon, dresser, clinical clerk, and others of the same description, should not be obtained by the payment of money. They ought to be the reward of industry and merit; at some of the metropolitan hospitals they are so. At St. George's Hospital, for example, no fee is paid for any of the situations in question.

But those officers who reside in the hospital pay a small sum for their board in the institution, which is not unfair, for a young man must eat and drink, and probably it costs him less to live in the hospital than out of it.

LECTURES ON DISEASES OF THE RECTUM;

Delivered at St. George's Hospital,
By Sir B. C. Brodie, Bart.

[The preceding Lectures will be found in the volume of the Gazette just concluded.]

ABSCESSES CONNECTED WITH THE RECTUM—FISTULA IN ANO.

I have already mentioned two kinds of abscesses which occur in connexion with piles. First, an abscess forms in an external pile, and bursts externally. Sometimes it will heal and not form again; at other times it will heal, and then form a second time. I stated that the shortest way of curing this kind of abscess was to excise the pile in which it is situated. Secondly, an abscess forms in internal piles, which bursts into the cavity of the rectum; and this sometimes heals spontaneously, and sometimes not. When an internal pile, in which an abscess has formed, becomes protruded externally, you may remove it either by the scissars or by ligature; or where that cannot be done, the patient may be cured by a long-continued course of Ward's paste, or *confectio piperis composita*. Then I mentioned, in a former lecture, abscesses which occur in connexion with stricture of the rectum, or with malignant and other organic diseases of that organ. None of these, however, are the abscesses which terminate in the disease commonly called *fistula in ano*.

Abscesses which become fistulous generally occur in persons of costive bowels. Those who have been long subject to piles are more liable to them than others; I suppose in consequence of the inflammation originally occurring in the pile extending to the neighbouring textures. Sometimes this inflammation, which precedes the formation of such an abscess, occasions the patient so little pain that he is hardly conscious of any disease existing in the part. I remember a gentleman in whom an abscess formed by the side of the rectum: and who was not conscious of any local symptoms. But he had been for some time subject to headach, was languid, forced to go home and lie down in the middle of the day. One day, walking in the streets the abscess burst; and that was the first thing which gave him any notion of its existence. In general, however, the abscess forms, attended, in the first instance, by a moderate degree of pain, and some tumefaction and induration are perceptible in the neighbourhood of the anus. The inflammation goes on, the pain and tenderness become greater, the induration more extensive, and at last the parts are so painful and tender that the patient can scarcely walk. The system sympathizes with the local inflammation, and the pulse becomes frequent, the skin hot, and perhaps the patient has rigors. Thus the symptoms run on until the abscess bursts. Sometimes it bursts into the rectum, and not externally; more frequently it presents itself externally, and bursts at a little distance from the anus. In many cases the abscess communicates with the inside of the gut, and opens externally also. As soon as the abscess has burst, the symptoms are all relieved. The pus which is discharged is seldom healthy, like that formed in an abscess situated elsewhere. For the most part it is dark-coloured, putrid, and offensive. Whether these qualities are given to the pus in consequence of a small quantity of fæces penetrating through an aperture in the mucous membrane into the cavity of the abscess, or whether it is that there is any transudation of fæculent matter, or gas, through the mucous membrane, without there actually being an aperture in it, I am unable to determine. I think it very likely that in many cases, at least, the offensive quality of the pus is to be attributed merely to transudation. I have known many facts which prove that transudation, to a certain extent, takes place in living bodies as well as in the dead subject. For example, a gentleman had an encysted tumor in the neck, which contained cheesy matter, such as is found in the common subcutaneous encysted tumors. This cheesy matter had become rancid, and when the tumor was dissected out the smell was exceedingly disgusting. Before the operation, the effluvia penetrated not only through the coats of the cyst, but also through the skin of the neck; so that you could scarcely bear to stand near the patient: I mean that you could smell the rancid contents of the cyst, when the cyst itself and the skin over it were entire.

When the abscess has burst externally, it sometimes heals as any other abscess would do. This, however, is only an exception to a general rule. For the most part the orifice of it remains open, and it continues to discharge a little matter; or if the orifice closes, it is only for a time, the matter being again collected at the bottom of the orifice, and the skin once more ulcerating. In the course of time the margin of the orifice becomes hard and projecting, a sort of button or tubercle being formed round it, and it is while in this

state that the abscess is said to be converted into a *fistula in ano.*

I say the general rule is, that this kind of abscess does not heal. But abscesses in other situations do, unless there is some particular local cause operating, or the patient's general health be bad. An abscess may be prevented from healing by a portion of dead bone, by a diseased gland, or by a piece of dead tendon or ligament at the bottom of it. But what is it that prevents an abscess from healing that forms by the side of the rectum? Here is a preparation [presenting it] which I think will explain how this happens. Here has been a *fistula in ano;* the mucous membrane has been dissected off, and a piece of bougie has been passed into the *fistula.* The fibres of the sphincter muscle pass over the bougie, and they pass under it also, so that, as you perceive, the sinus in fact is in the substance of the sphincter muscle; and I am certain that that is generally the case. If you look at the position of the sinus in the living person, you will be satisfied that for the most part it runs through the sphincter muscle; and dissection, as you have seen, confirms the observation. The sphincter muscle is constantly in motion, contracting and dilating, and consequently there is not the repose which is necessary for the cure of the abscess. The action of the muscle, in fact, prevents the abscess from becoming consolidated. In illustration of the effect of muscular action in preventing the healing of an abscess, I may mention this case:—A man was in the hospital on account of an abscess which burrowed in the muscles of the thigh. There was no lodgment of matter, no dead bone, no diseased glands at the bottom; but the abscess penetrated in various directions among the muscles, and they were in constant action around it. As long as the patient was walking about and using the limb, the abscess never healed. I put him to bed, and kept the limb in a state of perfect repose, as if the bone had been broken. Under this treatment the abscess, which would not heal before, healed readily.

I have said that the abscess forms in the vicinity of the gut. But it may extend in any direction; so that there may be a very large abscess running all the way from the rectum to the *nates.* Such an extension of the abscess makes no alteration in its character: it is still a *fistula in ano.*

An abscess sometimes forms high up by the side of the rectum, above the sphincter muscle; but this is of comparatively rare occurrence. These abscesses attain a very large size before the patient suffers much pain or inconvenience. There is at first merely a sense of bearing-down of the rectum, occasioned by the pressure of the abscess on it; but as the abscess increases in size, the patient has violent painful spasms, and a constant feeling as if he wanted to pass a motion, while there are no fæces in the bowel. His sufferings are now excessive. You examine the external parts, and you see nothing. You introduce your finger into the rectum, and there you perceive the abscess pressing on one side of the gut, and very much diminishing its diameter. Having thus ascertained the position of the abscess, if you examine the external parts again, although you could discover nothing in the first instance, you will probably be able to detect it, deep-seated as it is, at the side of the anus. More resistance will be offered to the pressure of the finger here than elsewhere; or if you introduce two fingers of the left hand into the rectum, which you may easily do, and press on the abscess from within, you will make it actually bulge a little externally. These abscesses high up by the side of the rectum give the patient an extraordinary degree of suffering, and that for a great length of time. If left to themselves, they generally burst into the rectum. It is your business, as I shall explain to you hereafter, to give the abscess an opening externally, before it has made another opening for itself.

I have told you that abscesses of the rectum occur in persons who are costive, and have been the subjects of piles; but I have no doubt that sometimes they are produced by foreign bodies penetrating through the mucous membrane of the bowel and sphincter muscle into the cellular membrane. I was sent for to a gentleman who had a very uneasy feeling in the rectum. I thought he laboured under internal piles, and prescribed him something which, however, did not relieve him. The next day, as he was no better, I examined the rectum, and found a hard substance sticking in the mucous membrane. With some difficulty I extracted it, and found it to be part of the core of an apple, which the patient had eaten the day before. Suppose this had not been extracted, the apple core, having penetrated through the mucous membrane, would have produced an abscess by the side of the gut. I was sent for to another gentleman, a year or two ago, with a very large abscess formed by the side of the gut. He suffered a great deal of local pain; had a very frequent pulse, brown dry tongue, very hot skin, and typhoid symptoms. I opened the abscess, and let out a quantity of very putrid offensive matter, which sufficiently explained the typhoid symptoms under which the patient laboured. After I had opened the abscess, I introduced my finger into the cavity, and, sticking across it, I found a long fish bone, which I extracted. The fish bone had evidently penetrated through

the mucous membrane of the bowel, and in all probability some small portion of feculent matter had passed by the side of the fish bone, thus accounting for the remarkable putridity of the matter.

A *fistula in perineo* is a very different affection from a *fistula in ano*. It is an abscess communicating with the urethra, and the urine flows through it when the patient makes water.

It is generally caused by some obstruction in the urethra, and all that is necessary for its cure is to dilate the obstructed canal.

Sometimes, however, a *fistula in perineo* is complicated with the disease of which I am now treating. I have seen of this only a very few examples. One of these is a patient in the hospital at present. This man had, if I remember right, a severe attack of gonorrhœa. This was followed by an abscess communicating with the urethra, which, instead of making its way directly to the perineum, ran (I suppose) behind the triangular ligament of the perineum to the rectum, and then again burrowed in front of the latter, and at last burst by the side of the anus; so that it had all the characters of a *fistula in ano*, though it communicated at the same time with the perineum and urethra.

I have mentioned that abscesses connected with the rectum may occur in persons in the best state of health. But those who are in bad health are more liable to them than others. They occur frequently in connexion with disease of the lungs: patients labouring under tubercles and abscesses of the lungs being very liable to abscesses by the side of the rectum, which have all the characters of common *fistula in ano*. You should always take care, before you operate on a patient for fistula, to ascertain whether the lungs are sound. Persons with diseased liver and other visceral diseases, are also liable to the formation of these abscesses. The distinction of these cases from those which occur in otherwise healthy subjects, is very important, inasmuch as the practice which is proper in the one case is quite improper in the other.

I have said that for the most part an abscess by the side of the rectum has no disposition to heal. What, then, will happen if you do nothing in such a case? The fistula will remain open, sometimes discharging little and sometimes much; the orifice alterately closing and opening. I have known several cases in which a neglected fistula has continued in this state for years. This, however, is what ought not to be allowed, for sooner or later the day of reckoning comes, and the fistula which was a trifling disease originally, assumes a formidable character. A gentleman who had had an abscess by the side of the rectum for twenty years, and it went on as I have mentioned, sometimes discharging a little matter, then stopping, and then discharging again. Some three or four months before I was consulted the external orifice closed, and did not open again as usual. By and by he began to suffer a great deal of pain, became exceedingly ill, and I was sent for to see him in the country. He appeared to be all but dying; he was not in *articulo mortis*, but I do not think he would have lived twenty-four hours longer. I examined the rectum, and satisfied myself that there was one of those large deep abscesses which I have just now described. I introduced a lancet by the side of the gut, and thrust it in quite up to the handle before I could reach the matter. I was sure of the right direction of the lancet, and carried it onwards until I succeeded in my object. When a little matter had escaped, I introduced a probe-pointed bistoury, and dilated the orifice freely. Immediately there flowed a pint or more of matter so putrid as to cause the whole house to stink; so that you could smell it in every room. This operation saved the patient's life. It was necessary, on a future day, to enlarge the opening still further into the rectum, because the matter did not escape with sufficient freedom; but from that time the patient went on well. This monstrous and dangerous disease had been produced in consequence of the neglect of a trifling affection, which might easily have been cured in the beginning.

A *fistula in ano* is sometimes a simple fistula, such as I have before mentioned. It may communicate with the gut and not open externally; or *vice versâ*; or it may open externally, and communicate with the inside of the gut also. But sometimes the fistula is complicated, so that there is a sinus in one direction, and a second in another. There may be even three or four different sinuses produced, by the abscess burrowing first in one direction, and then being stopped and burrowing in another; thus pursuing a circuitous course until it finds its way to the surface. I have adverted to a case in which a common *fistula in ano* gave rise to the formation of a large deep-seated abscess, high up by the side of the rectum; and I have no doubt that many such deep-seated abscesses are formed in this manner. Sometimes, in attempting to cure what seems to be a common fistula, you find that it does not get well as usual, and then at last you discover a large abscess above, which prevented the closing of the smaller sinus below.

Treatment.—When inflammation takes place by the side of the gut, such as you might expect to terminate in abscess, it will do so in most cases in spite of all

your efforts to prevent it. There are, however, some exceptions to this rule; I have every now and then known a patient who has been thus threatened put on leeches and keep quiet, the inflammation subsiding afterwards without suppuration; but in nineteen cases out of twenty the abscess sooner or later forms. If you are called to the patient quite *early* in the disease, you may put on leeches, keep him quiet, and give him the small chance of preventing suppuration which this treatment will afford; but otherwise your best plan is to poultice the part, foment it, and encourage the formation of matter as much as possible. When you are satisfied that matter has formed, the sooner you open the abscess the better; for the longer you leave it the more mischief will the matter do, by burrowing in the neighbourhood of the gut. You need not wait till the matter can be felt externally; if the symptoms indicate that matter has formed, and you feel the hardness externally, you may venture to introduce your lancet. Bear in mind the anatomical position of the parts, and you will be able to introduce a lancet with perfect safety, until you reach the cavity of the abscess, even although it be very deep seated.

I have stated that the abscess sometimes heals, but that it generally does not, and that then it forms a fistula. What are you to do in this stage of the disease? how are you to heal the fistula? It was formerly the custom to dissect out all the hard callous parts around the abscess, and a most frightful operation it was. I believe it was Mr. Pott who made this great improvement in this department of surgery, by showing that such a dissection is quite unnecessary, and that all that is required is, to lay the abscess freely open into the bowel. If you look at this preparation [presenting it], I think you will see how it is that this simple operation is sufficient to produce a cure. The matter runs freely out of the abscess, and yet the patient does not get well, because, as I told you, there are the fibres of the sphincter muscle constantly acting on the abscess, and preventing the healing process. When the abscess is laid open, the fibres between it and the bowel must be divided, and the sphincter muscle being thus set at liberty, not only is there a free and immediate escape of the matter, but the action of the muscle, which prevents the healing of the abscess, is put an end to. This, then, is the mode of curing the abscess: lay it open into the bowel, dividing at the same time the fibres of the sphincter muscle which lie over it.

But before I proceed to describe the operation in detail, and to explain all the various circumstances which require to be attended to, as connected with it, I must speak to you concerning the cases in which the operation is, and those in which it is not, proper to be performed. It may be performed in healthy persons, but not in those who are in a state of disease. The operation is proper if the rectum be otherwise sound, if the lungs and the liver be sound, if there be no organic or visceral disease; but if the rectum be not otherwise in a healthy state, if there be stricture, carcinoma, or any other disease of this organ, the operation ought not to be performed; for under these circumstances the sinus will not heal, even though it be laid open. If the patient labour under visceral disease, it is seldom that the abscess will heal; but if it should, the visceral disease will make increased progress, and the patient will die sooner if the operation be performed than if it were let alone. If a person come to you with *fistula in ano*, and he looks as if he laboured under some other disease, always inquire into the state of his general health, and take care never to perform the operation except when you are satisfied that his health is good.

GENERAL LYING-IN HOSPITAL.

MIDWIFERY REPORTS.

By Edward Rigby, M.D. F.L.S. &c.

[Continued from preceding vol. page 896.]

Uterine Hæmorrhage.

February 24, 1834.—Susan Templeton, æt. 38, delivered of a boy; seventh child. Presentation and labour natural; child large, cord once round the neck. About ten minutes after its birth slight hæmorrhage made its appearance, and on feeling the uterus (which had been tolerably well contracted at first) it was found to have yielded considerably. After gentle friction over the uterine region, some very large clots of blood were expelled; the placenta still refused to quit the uterus, the neck of which was strongly contracted upon it. Profuse hæmorrhage then came on, and it was found necessary to introduce the hand into the womb for the purpose of extraction; this was accomplished with little difficulty. The patient was very faint, though not in a state of complete syncope. No pulse could be felt at the wrist; the extremities were cold, and she complained of numbness in them; voice feeble; face and lips blanched; eyes sunk. Sp. ammon. arom. was given, and she had some brandy in her gruel; friction to the extremities. The colour then began to return to the cheeks and lips, and the voice became stronger; but it was a considerable time before the pulse at the wrist returned, and

then but feebly. A firm bandage was put round the abdomen, with a compress over the uterus; she was removed to a bed in the ward, and a draught of tinct. hyosc. ammon. arom. and camphor mixture was given.

10 P.M. — Has slept a little, but the hæmorrhage returned, which caused her to faint, as nurse reports; pulse again could not be felt at the wrist, but gradually and feebly returned after taking some brandy. Cold cloths were applied to the external parts. She recovered without any untoward symptom.

It is difficult always to prove whether the non-expulsion of the placenta is owing to its not being yet completely separated, or to its being retained by contraction of the os uteri, or whether it depends partly on both causes. Professor Naegele rather deprecates the use of friction to the abdomen for the purpose of exciting the uterus to contract, as being liable to bring on irregular action of its fibres; and perhaps in this case the firmness with which the os uteri was contracted might be in part owing to the friction used. The hæmorrhage returning in about four hours afterwards, shews how important a part of a nurse's duty it is to watch her patient narrowly for several hours after labour. In no case are we secure against hæmorrhage. Thus in the following case hæmorrhage came on four hours after labour, although none had occurred immediately after the birth of the child to justify our expecting an attack.

September 18, 1835.—Jane Garwood, æt. 27, delivered of a boy; first child. Labour natural and easy; second position. A fold of the cord prolapsed between the neck and perineum, but as it was not so pressed as to endanger the child's safety, the expulsion of the body was allowed to take place by the natural efforts. Smart hæmorrhage occurred after she had been in bed four hours; it was easily repressed by cold applications and diminishing the quantity of bed clothes, and did not return (the night was sultry).

There can be little doubt but that this attack of hæmorrhage was in great measure produced by the heat of the weather. Nurses are too ready to "cover a patient up comfortable," as they call it, which is little else than to keep her in a reeking perspiration; and nothing is more likely than this to produce inertia uteri.

Hæmorrhage thirty-six hours for Labour.

The next case of hæmorrhage is still more remarkable, it having come on *thirty-six hours after labour*. It was a twin case, and in the debilitated constitution of an habitual gin drinker, proved fatal. My friend Dr. Barr has furnished me with the report.

"February 10, 1834.—Sarah Grieve, delivered of twins (girls); fifth labour. The first child presented with the head, second with feet; former rather larger of the two. Not much above ten minutes elapsed between the birth of the one and the other. The placentæ were not adherent throughout their contiguous edges, a portion in the centre being left unattached. But little hæmorrhage followed their expulsion, although there was a considerable discharge of liquor amnii. She is a pale unhealthy-looking woman, very fretful and spiritless, has been out of health for the last few months, and much troubled with cough and mucous expectoration. About thirty-six hours after delivery she was attacked with rather a profuse flooding, felt weak and faint; the nurse said that a minute before I saw her she had fainted away. Cold water was dashed on the face, and wet cloths applied to the hypogastric region. Cold vinegar and water was injected into the vagina after the removal of rather a large coagulum, and the abdomen was tightly bandaged. The hæmorrhage at length ceased, after returning somewhat at intervals, being especially excited by the cough. She had some refreshing sleep after taking 10 gr. of ext. hyosc. and 5 gr. of ext. conii.

"13th.—Feels very weak; dozes a good deal, but tosses about much; cough very troublesome; pulse 140, bounding, but compressible; skin hot and dry; tongue clean and moist; is thirsty; bowels moved twice during the day. Sound on percussion over the chest clear.

Mist. Salin. c. Vin. Antim. 4tis horis.

"14th.—Much the same; complains of no pain or tenderness; no delirium; pulse as frequent, but weaker; bowels relaxed, motions sometimes passed in bed. (Dr. Rigby saw her to-day).

> Ammon. Carb. gr. v.; Aquæ Menth. Pip. ℥iss. 4tis. horis. To add 10 drops of Tinct. Opii occasionally. To have some gin and water, to which she says she is daily accustomed.

"15th.—No improvement; respiration hurried, but less troubled with cough; has had slight moisture on the skin, which was only transient; eyes dull and heavy; complains of occasional dimness of sight and confusion of mind; pulse 130, soft and weak; bowels less relaxed, stools watery and curdy.

> Ammon. Carb. c. Confect. Arom. and Træ. Cinchon. Comp. ex Aq. Menth. Pip. 4tis vel 3tiis horis.

Dr. Locock saw her shortly after, and ordered, in addition to the mixture,

> Hyd. c. Cretæ, gr. v.; Pulv. Rhæi.; Pulv. Ipecac. aa. gr. iij. ft. Pulv. bis die sum.

"16th.—Weaker; very restless during

the night; complains of numbness of limbs, and general uneasiness. Early this morning had a catching pain of the side on coughing or full inspiration, which was relieved by mustard poultice. Pulse 141, feeble; skin dry; tongue moist and clean; bowels moved three times during the night, stools more natural.

Emulsion of egg and brandy.
Contin. Medicam. Capt. Liq. Opii sed. ℥ss. h. s.

" 17th.—Gradually sinking. Died at 2 P.M."

Remarks.—It is curious to see what a variety of anomalous symptoms will appear in patients of this sort, who have been deprived of their accustomed stimulus, and how really seriously ill they will become under such circumstances, even in spite of medical treatment. I have seen miserable pallid wretches, victims of spirit drinking, in whom, after being a few days in the hospital, there has been such debility, sleeplessness, and derangement of bowels, such irritability of stomach towards every species of food, and with such constant vomiting and anorexia, that considerable fears were excited for their life, and where the glass of gin daily has " done more good than all the physic in the doctor's shop." I have seen cases of habitual spirit drinkers where this was even necessary on the second day after labour. It is seldom that we are made aware of this fact by the patients themselves; and I have frequently known them even deny their being spirit-drinkers. But besides the above-mentioned symptoms, there is an irritable, peevish, discontented manner, which is very characteristic. The hæmorrhage in this case effectually accounted for the appearance of debility, and lulled any suspicions respecting her intemperate habits; but I am half inclined to suspect, that could we have been aware of it, we might by a moderate supply of the accustomed stimulus, even shortly after labour, have kept up that degree of tone and uterine contraction which would have effectually prevented the attack of hæmorrhage. It was the general atony of the system, in which the uterus partook, and which was doubtless increased by the demand upon its powers from her having had twins. A sectio cadaveris was not permitted by her friends.

No case of unusually precipitate labour has occurred during 1834. Two or three patients have put off coming to so late a moment that they have either been delivered in the vehicle which brought them, or on the floor before they could be put to bed; but no untoward result has happened, no rupture of the cord, inversion of the uterus, &c. I therefore pass on to

Prolapsus of the Cord.

The following case was reported by my friend Dr. Barr, who turned the child:—

March 31st, 1834.—Mary Lalley, æt. 26, delivered of a girl, still-born; third child. When she came in the os uteri was fully dilated; membranes projecting, and forming a tense bag during the pains. It was with difficulty the finger could reach the head, which was high up above the brim of the pelvis. When the liquor amnii came away, one or two coils of the funis were found presenting between the head and promontory of the sacrum. At all times the pulsation in the cord was but feeble, and during the pains, which were not strong, they ceased entirely. In order to give the child a chance of being born alive, Dr. Rigby thought it would be advisable to turn. Accordingly I introduced my hand slowly into the uterus. After passing the head I felt a hand, and then an elbow, and still higher up a foot, which I gently brought down. The patient suffered but little from this; but though I got the foot into the vagina, the head did not go up. Dr. Rigby then passed a fillet round the ankle, and pushed back the head, which required some force to make it retire. As the pains were inefficient, considerable extractive force was required to complete the birth. One arm came along with the head; the placenta quickly followed, with some large clots of blood, but the uterus soon contracted firmly.

April 1st.—In the early part of night had some difficulty in passing water; but after applying warm fomentations to the pubes, and steams of warm water to the pudenda, she passed it freely, and this morning feels easy and comfortable.

From the very feeble pulsations of the cord, even during the intervals of the pains, it was evident that as the head was so high, the only chance of saving the child's life was by turning. I rather suspect that I was in some measure the cause of the difficulty which Dr. Barr experienced in turning the child: having grasped one foot, and finding that the other was not so easily reached, I advised him to bring down the one which he had hold of; whereas, if the two feet had been brought down, the child would probably have been turned more readily.

Two cases of *placenta prævia* have occurred; in both the placenta was but partially attached, and in neither was it necessary to turn the child.

May 13th, 1834.—Mary Owen, æt. 30, out-patient, near her full reckoning, was seized with a considerable uterine hæmorrhage a week ago; it returned again the day before yesterday. Slight pains appeared this morning, and much flooding

followed. I saw her at 10 and 11 P.M.; os uteri dilated to about two inches; its anterior margin covered by the placenta; the head pressing on the os uteri. The discharge is now very slight; pulse full. I desired the midwife to keep her cool. The pains increased, and the labour followed naturally, without any further loss.

July 4th, 1834.—Anne Teeling, out-patient, reckons that she has still a month to go. Was surprised at 4 this morning by a profuse uterine hæmorrhage, which soaked through the bed; it continued for about half an hour, and made her feel very faint; it returned in an hour, but in less degree, and continued to dribble for some time. The midwife gave her an opium pill, and applied vinegar, &c. I saw her at 4 P.M.; os uteri nearly fully dilated; edge very uneven; margin of the placenta over its right and anterior edge; no discharge; head presenting in the third position; scarcely any pains; no liquor amnii; collected to distend the membranes. I gave her a dose of ergot of rye, with a little brandy, as she was extremely faint; the pains increased somewhat. Not having made water since 3 A.M., I drew off a small quantity of high-coloured urine, and gave her some ammonia, as the faintness continued. In the course of an hour a little liquor amnii began to distend the membranes. I ruptured them, and a considerable quantity escaped with every pain. The pains became stronger, the head descended, and a dead child about the eighth month was born. She recovered quickly.

In the first case nothing more was necessary than to let the labour go on, and not interfere; the head had effectually compressed the placenta, and the danger was passed. In the second the hæmorrhage had been so profuse, and the exhaustion was so great, that I feared she would not have sufficient strength to carry her through. It was an intensely hot day, and her relaxed half-fainting state added not a little to the disposition she had to perspiration. Besides the brandy and ammonia, I directed a little beef-tea to be made; and by giving her a tea-spoonful or two of it every few minutes, she revived considerably.

I well recollect thinking at the time of a passage in my father's work on Uterine Hæmorrhage, which applied remarkably to to this case. It is a part of the fourth case which he has recorded, and where the hæmorrhage and consequent exhaustion had been very alarming:—"By carefully attending (says he) to keep the room very cool, by preventing my patient from being the least stirred, and being myself her nurse, in giving her every few minutes small quantities of the coolest drinks, I prevented the discharge from increasing, and at the same time supplied as far as I could the waste of what she did lose, by the drinks she took being as nutritious as I could venture to give them without their being irritating."

[To be continued.]

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN SEPTEMBER, 1835.

James Plummer, Cirencester.
Edward Golding, Dublin.
William Tarleton, Hurley in Arden.
Wm. Alexander Gavin, Aberdeenshire.
James Williamson.
Wm. Barwick Clarke, Beckermont.
Arthur Benoni Evans, Hampstead.
Thomas Salmon, Barnstaple.
Frank Dobson Potter, Chipping Ongar.
Richard Goldstone, Bath.
Richard Beadon Ruddock, Stockland.
John Donald, A.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Sept.* 29, 1835.

Abscess	5	Hæmorrhage	1
Age and Debility	33	Heart, diseased	4
Apoplexy	5	Hooping Cough	2
Asthma	3	Inflammation	26
Cancer	1	Bowels & Stomach	7
Childbirth	2	Brain	3
Consumption	50	Lungs and Pleura	4
Constipation of the Bowels	2	Insanity	3
		Jaundice	2
Convulsions	35	Liver, diseased	5
Croup	3	Measles	11
Dentition or Teething	12	Mortification	5
Diarrhœa	4	Paralysis	5
Dropsy	18	Rheumatism	1
Dropsy on the Brain	15	Small-Pox	24
Epilepsy	1	Spasms	1
Erysipelas	1	Tumor	2
Fever	6	Unknown Causes	2
Fever, Scarlet	10		
Fever, Typhus	3	Stillborn	10

Decrease of Burials, as compared with the preceding week 9

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Sept. 1835.	THERMOMETER.		BAROMETER.	
Thursday . 17	from 40	to 60	29 68	to 29·73
Friday . . 18	37	62	29·73	29·64
Saturday . 19	49	64	29·50	29·60
Sunday . . 20	54	63	29 58	29 74
Monday . . 21	51	57	29·81	29·75
Tuesday . . 22	52	69	29 59	29·54
Wednesday 23	54	64	29 60	29·70
Thursday . 24	from 51	to 61	29·75	to 29·89
Friday. . . 25	44	62	29·91	29·81
Saturday . 26	44	61	29·65	29 61
Sunday . . 27	47	57	29·57	29 49
Monday . . 28	45	53	29 37	29·61
Tuesday . 29	43	62	29 55	29·47
Wednesday 30	51	65	29 32	29·19

Prevailing winds S.E. and S.W. Generally cloudy, with frequent showers of rain.
Rain fallen, 1 inch, and ·675 of an Inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 10, 1835.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE II.

ON THE ACTION OF MEDICINES.

GENTLEMEN,—I proceed in this lecture to examine the action of medicines on the animal system.

We have two distinct classes of phenomena, one caused by the other, both of which may be denominated the *effects* of medicines; but in order to distinguish their order of sequence, we may term the one class the *primary*, the other the *secondary* effects. Purgatives, for example, excite a certain degree of irritation in the intestinal tube, in consequence of which they are sometimes useful in relieving morbid states in distant organs. The irritation, then, is the primary effect; while the beneficial influence on the previous disease is the secondary effect: the first has by several modern authors been termed *physiological;* while the second has, in contradistinction, been denominated *therapeutical.* In conformity with the usage of some of our best writers I shall adopt these designations, without, however, being satisfied that they are the best that could be applied.

Physiological or primary effects.—The physiological or primary effects of medicines, for convenience, may be divided into such as are *local,* or those that occur in the part to which the agent is applied; and into those that take place in distant organs, and which, by way of distinction, we denominate *remote* effects.

1. *Local effects.*—If you apply strong sulphuric acid, or the potassa fusa, to a living tissue, you will observe that the death of the part soon takes place,—various chemical changes being effected in it,—while the surrounding parts become inflamed, and tender to the touch. In this case, then, there are simultaneous chemical, physical, and functional changes; and we can readily believe the primary influence of the caustic to reside in its chemical properties. This constitutes one kind of local action.

Observe the effect which arises from the application of any of the substances called *irritants* to the body—that is, of substances which excite pain, heat, soreness, and even inflammation; as, for example, cantharides, savine, gamboge, tartar emetic, or alcohol. These agents alter the vital actions of the part, and occasion a series of physical and functional changes; but we have no evidence of any chemical influence. This may therefore be regarded as a second kind of local effect, where most of the phenomena seem referrible to some unknown influence of the medicine over the vascular system of the part; and we may even suspect that the nervous system is only secondarily affected.

But there is a third kind of effect, which can be arranged under neither of the before-mentioned heads, and of which we have a good example in the numbness produced by chewing aconite. The most powerful effect of this kind that I ever experienced is in the case of the *Aconitum ferox,* a plant growing in the kingdom of Nepal, and which is used by the natives as a poison, under the name of *Bish* or *Bikh.* At the request of Dr. Wallich I undertook a series of experiments on it, which have been published by him in the *Plantæ Asiaticæ rariores.* While the spirituous infusion of the root of this plant was evaporating, I applied a drop of the liquid to my tongue. Within ten minutes an intense numbness came on in the tip of the tongue, and in the lips, and was

followed by an affection of the velum and uvula, giving rise to a sensation as if these parts were elongated, or paralysed, and rested on the tongue. This latter effect left me in about a quarter of an hour, but the numbness remained for eighteen hours. In this case, then, it is fair to infer that the primary operation of the poison was on the nerves of the part. Other instances of a like nature might easily be quoted: I shall only name two—the numbness produced on the finger by applying the vapour of hydrocyanic acid to it; and the paralysis of the muscular fibres of the intestines from the application of the ticunas poison to them. Now although all changes in vital action are fairly referrible to physical or material changes, yet in the examples here offered we have no visible alteration.

2. *Remote effects.*—One of the most striking phenomena produced by medicines is the influence which they exercise over distant organs. Certain substances taken into the stomach have the curious property of exciting an increased flow of urine; others act on the salivary glands, and promote the salivary secretion. How do these phenomena take place? Do the medicinal particles actually pass through the body, from the stomach to the kidneys, or to the salivary glands; and are we to consider these cases as, in point of fact, examples of local action, or should we refer the primary influence to a change in the condition of the stomach, or of some other organ, in consequence of which an alteration is effected in the distant organ, by some mysterious and unknown agency which physiological writers have named *sympathy?* The facts which we have at present accumulated certainly are in favour of the supposition, that sometimes one, sometimes both, of these modes of operation take place. Let us consider them separately; and first,

Of the operation of medicines by absorption.—Under this head we have to examine the following questions:—

1. Do medicines pass into the blood, and are they transmitted from one part of the body to another?

2. If they do, what are the organs that absorb them—the veins, or the vessels called absorbents?

3. If medicines are absorbed, is it probable that the remote effects take place in consequence of the absorption?

4. If a medicine produce a remote effect, apparently in consequence of its passage into the blood, in what way is this peculiar influence exercised?

Let us examine each of these questions separately.

1. That medicines do pass into the blood, and are subsequently transmitted to some part distant from the one to which they are applied, is undeniable in a great number of instances. The facts in proof are of two kinds: *first*, it has been established by the experiments of Drs. Christison and Coindet, that four ounces of a solution of oxalic acid disappeared from the peritoneal sac of a cat in fourteen minutes; and there is no mode of satisfactorily explaining the disappearance of the fluid except on the assumption of its passage into the blood. *Secondly*, the medicinal particles have been repeatedly found either in this fluid, or in the secretions, or in both.

It is not necessary for me here to enter into the difficulties which have been experienced in detecting medicines in the blood: it is very easy to establish the fact of their existence in this fluid beyond the possibility of doubt, by experiments. Camphor, animal empyreumatic oil, musk, oil of turpentine, asafœtida, indigo, rhubarb, madder, gamboge, copper, lead, cyanuret of potassium, sulpho-cyanuret of potassium, sulphate of iron, sulphate of potash, cyanuret of mercury, chloride of barium, sal ammoniac, iodine, hydrocyanic acid, and alcohol, have been detected in it; and I must refer you to the writings of Tiedemann and Gmelin, of Flandrin and Magendie, of Grognier, of Cantu, and of Krimer, for further information on this point.

But the question is easily decided by experiments, which may be made on ourselves. It is admitted that the secreted fluids are separated from the blood; hence, if we detect medicinal particles in those fluids, we may fairly infer their previous existence in it. Let me, therefore, notice some of the substances that have been found in certain secretions:—

First, in the cutaneous secretion, mercury, iodine, sulphur, the odorous matter of musk, of garlic, and of onions, and other substances, have been detected.

Secondly, in the breath, several substances have been detected by their odour; for example, camphor, alcohol, ether, phosphorus, asafœtida, sulphur, the odorous matter of garlic, and of onions, &c.

Thirdly, in the milk. The milk sometimes acquires purgative properties, in consequence of the employment of purgatives by the nurse. Tonics, indigo, iodine, and madder, have also been distinctly recognised in it.

Fourthly, in the urine so many substances have been discovered, that it will be most convenient to exhibit them in a tabular form. The following is taken principally from the experiments of Drs. Wöhler and Stehberger, as mentioned by the late Dr. Duncan:—

SUBSTANCES WHICH PASS OFF BY THE URINE.

(A) Unchanged, or nearly so.

Salts.

Carbonate of potash.
Nitrate of potash.
Chlorate of potash.
Sulpho-cyanate of potash.
Hydro-sulphuret of potash.
Ferro-cyanate of potash (in 66 minutes.)
Silicate of potash.
Tartrate of nickel and potash.
Borax.
Muriate of barytes.

Colouring Principles.

Indigo } (in 15 minutes.)
Madder }
Rhubarb (in 20 minutes.)
Gamboge.
Logwood (in 25 minutes.)
Red radishes.
Mulberry.
Black cherry (in 45 minutes.)
Cassia fistula (in 55 minutes.)
Elder rob (in 75 minutes.)

Odorous Principles, somewhat altered.

Oil of turpentine.
——— juniper.
Valerian.
Saffron.
Asafœtida.
Garlic.
Castoreum.
Opium.
Narcotic principle of Amanita muscaria.
Asparagus (*Cullen.*)

Other Matters.

Astringency of Uva ursi (in 45 minutes.)
Oil of almonds (*Bachetoni.*)

(B) In a state of Combination.

Sulphur, as sulphuric and sulphuretted hydrogen.
Iodine, as hydriodic acid.
Oxalic, Tartaric, Gallic (in 20 minutes), Succinic, Benzoic acid } Acids, appear in combination.

(C) In a Decomposed State.

Tartrate, Citrate, Malate, Acetate } of potash, or soda, are changed into the carbonate of the same alkali.
Hydro-sulphuret of potash changed, in a great measure, into the sulphate of potash.

I have only to remark respecting this table, that one of the easiest substances to experiment with is nitre, which may be readily recognised by dipping a piece of paper into the urine and drying it, when it will be found to deflagrate like touch-paper. If the accounts published respecting the *Amanita muscaria* (fig. 7) be correct, its effects are most extraordinary. A variety of this fungus has a powerful narcotic or rather inebriating effect; and that the active molecules do get into the blood is proved by the fact of the urinary secretion being impregnated with them, and thus possessing an intoxicating property; and we are told that the inhabitants of the north-eastern parts of Asia use it for this purpose. A man, for example, may have intoxicated himself to-day by eating some of the fungus; by the next morning he will have slept himself sober; but by drinking a tea-cupful of his urine he will become as powerfully intoxicated as on the preceding day. "Thus," says Dr. Greville, on the authority of Dr. Langsdorf, "with a very few *Amanitæ*, a party of drunkards may keep up their debauch

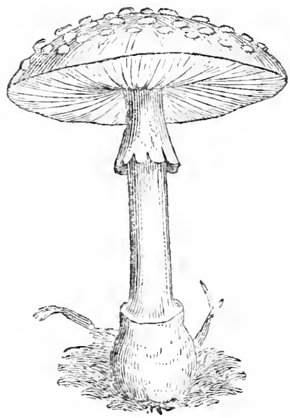


Fig. 7.—*Amanita muscaria.*

for a week;" and "by means of a second person taking the urine of the first, a third of the second, and so on, the intoxication may be propagated through five individuals."

To the facts now stated in proof of the absorption of medicinal agents, I may add the discovery of various substances in the solids of the body, such as the colouring matter of madder in the bones, copper and lead in the liver, mercury in various tissues of the body, &c. I cannot, however, help noticing here the hypothesis of Dr. Percival, more especially as it is adopted by Dr. Chapman, an American author on Materia Medica. He supposes that those medicines which, when administered by the stomach, can be detected in the secretions, do not exist in their original state in the blood, but are reproduced in the secreting organs. Without entering into all the inconsistencies of this hypothesis, I may mention that the subsequent discovery of so many substances in the blood is quite sufficient to overturn it.

2. Do the veins or the lacteals absorb medicinal particles? The ancients, unacquainted with the lacteal and lymphatic vessels, attributed to the veins the absorption of solids and liquids from the surfaces of the body. The truth of this doctrine, however, was called in question by Dr. William Hunter and Monro secundus; and their opinions were supported by the experiments of Mr. John Hunter. A change consequently took place in the opinions of physiologists, and absorption was presumed to be exclusively effected by the lacteal and lymphatic vessels. The subsequent experiments of Magendie, of Flandrin, and of Tiedemann and Gmelin, have, however, proved that the veins do absorb medicinal substances, and have rendered it highly probable that the lymphaties and lacteals do not.

Tiedemann and Gmelin administered a variety of colouring, odorous, and saline substances to animals, mixed with their food, and afterwards examined the state of the chyle, and of the blood of the veins of the alimentary canal. The *colouring* substances employed were—indigo, madder, rhubarb, cochineal, litmus, alkanet, gamboge, and sap-green; none of them could be detected in the chyle, but some were found in the blood and urine. The *odorous* substances used were—camphor, musk, spirits of wine, oil of turpentine, Dippel's oil, asafœtida, and garlic: they were for the most part detected in the blood and urine, but none were found in the chyle. The *saline* substances tried were—acetate of lead, acetate and cyanuret of mercury, chloride and sulphate of iron, chloride of barium, and ferro-cyanuret and sulpho-cyanuret of potassium. A few of these were detected in the chyle, and most of them in the venous blood and urine. From these experiments we may conclude, that although saline substances occasionally pass into the chyle, odorous and colouring matters do not; all, however, are found in the blood.

Magendie performed a striking experiment, with the view of settling, if possible, the question of the venous or lymphatic absorption of medicines and poisons. He divided all the parts of one of the posterior extremities of a dog, except the artery and vein, the former being left entire, for the purpose of preserving the life of the limb. A portion of the *Upas tieuté* was then applied to a wound in the foot: in the short space of four minutes the effects of the poison were evident, and in ten minutes death took place. To the inferences drawn from this experiment, however, several objections have been stated: first, the exhibition of opium, to diminish the pain of the operation, has been said to vitiate the whole of the experiment; secondly, the coats of the arteries and veins contain lymphatics, by which absorption might be carried on; and thirdly, as the poison was introduced into a wound, the poison might have combined with the blood, and have rendered it deleterious, without the process of absorption taking place. The first two of these objections have been obviated. Some years since I assisted my friend Mr. Lloyd, assistant-surgeon of St. Bartholomew's Hospital, in performing an analogous experiment, using *Strychnia* instead of *Upas tieuté*, and not administering opium: death took place in twelve minutes. In a second experiment, Magendie severed the artery and the vein, and reconnected them by quills, so as to preclude the possibility of absorption taking place by the lymphaties of these vessels: the effects were the same.

We may, then, I think, fairly conclude that the absorption of medicinal particles is effected by the veins. I do not think it necessary to enter into any discussion as to how these vessels carry on this function, and whether medicinal particles enter by what have been called absorbing mouths, or by imbibition through the coats. The hypothesis of the latter mode of transit has been ably supported by Magendie; and to his lectures, published in the Lancet of last year (1834), I beg to refer you for the numerous and striking facts which he adduces in favour of his own view. If his hypothesis of imbibition be correct, absorption must no longer be regarded as a vital, but merely a physical action. The principal objection to this opinion is the difficulty of accounting for the absorption of the particles of solid substances.

3. The next point to be examined is the probability or otherwise of the remote effects of medicines being produced by the absorption of the medicinal particles; for it does not necessarily follow that because certain active substances are found in the blood, their action on the system must be a consequence of their transit.

Mr. Travers, in his "*Further Inquiry concerning Constitutional Irritation,*" points out very forcibly the analogy to be observed between the effects of severe injuries and those of poisons operating rapidly upon the system (thus a punctured wound sometimes produces tetanus, so does strychnia); and he concludes that the *modus operandi* in the two cases is identical. As, in cases of injuries, there can be no absorption, so neither is there in cases of poisoning. But no injuries of remote parts that I ever heard of have produced the salivation which is caused by the use of mercury, or the affection of the urinary organs which cantharides are capable of exciting. It must, I think, be admitted that our opinion, whether for or against the operation from absorption, must be founded on probabilities, since the subject is incapable of actual demonstration or proof. I confess I cannot help thinking, that in a considerable number of cases the alteration of functions produced by poisons and medicines is the result of absorption. Let us consider *seriatim* the circumstances in favour of this opinion.

In the *first* place, we know that no organ in the body can perform its functions without a proper supply of blood; and we also know that alterations in the quality of this fluid are attended with corresponding alterations in the actions of the system; in proof of which I may refer to scurvy brought on by improper kinds of food (as the long-continued use of salt provisions.) We are, therefore, naturally led to suspect, *à priori*, that the passage of medicinal molecules (especially those of known activity) into the blood, and the circulation of them, would be attended with some functional disorder. This suspicion becomes stronger and more valid when we try the effect of injecting substances into the veins. We find tartar emetic excites vomiting; castor oil, purging; opium, narcotism.

Secondly, it seems that the effects of some medicines are modified by circumstances that influence absorption, and therefore we are naturally led to presume a mutual relationship. Thus the activity of a medicine is not uniform when applied to different parts of the body. Nux vomica, for example, is most energetic when applied to the pulmonary surface, less so when swallowed, and still less when applied to the skin; and the same remark applies to opium. Now the faculty of absorption, or of imbibition, as Magendie calls it, does not take place with equal intensity in all tissues. Certain physical conditions (viz. a fine and delicate structure, and great vascularity) enable the pulmonary surface to absorb or imbibe with extreme rapidity: in this respect, indeed, it is not equalled by any tissue of the body. Hence, then, if we assume that nux vomica and opium act by becoming absorbed, we can easily comprehend why these substances are so energetic when applied to this part. The membrane lining the alimentary canal absorbs with less facility than the pulmonary membrane,—which may be accounted for by the less extent of surface in contact with the medicine, by its less vascularity, and by its being covered, in some parts at least, by an epidermoid layer, and in all its parts by mucus, which to a certain extent checks absorption. We see, then, the reason of the less activity of the above-mentioned poisons, when applied to the alimentary membrane, if we admit their operation by absorption. The cutaneous surface, lastly, being covered by an inorganic membrane (the epidermis), does not possess the same physical faculties for absorption met with in either of the foregoing tissues; and hence the comparative inertness of medicines when applied to it. In fact, it is only by the long-continued application of these agents to the skin that we are enabled to affect the general system; and that the obstructing cause is the epidermis, is shown by the facility with which the system may be influenced when this layer is removed, as has been proposed and practised by Lembert and Lesieur, constituting what has been denominated the *endermic* or *emplastro-endermic* method of treating diseases: of this method I shall have occasion to speak hereafter.

Another circumstance tending to prove some connexion between the activity of a medicine and its absorption is, that the effect of many medicines is in proportion to their solubility. Arsenic and morphia are both more energetic in solution than in the solid state. Now liquids (particularly those miscible with the blood), are much more readily absorbed than solids, and therefore, by admitting the absorption of medicines, we have an easy explanation of the fact. In the treatment of many cases of poisoning we endeavour to take advantage of this principle, and by rendering substances insoluble, diminish their activity, or render them quite inert. Thus the antidote for the salts of lead, or of barytes, is a sulphate, the acid of which forms an insoluble salt with either of the bases (lead or barytes.). Gallic acid (or astringent infusions which contain it) is

for the same reason found useful in cases of poisoning by vegetable substances whose active principle is an alkaloid; and we employ carbonate of lime as an antidote for oxalic acid, to render this substance incapable of absorption.

We may add another argument to the above, and which, like them, is capable of practical application. Magendie asserts, as the result of experiments, that plethora uniformly retards, and depletion as constantly promotes, absorption. If, therefore, we wish to promote this function, we have a ready means of doing so in blood-letting. Thus, every surgeon knows that one powerful means of promoting the action of mercurials on the mouth is to abstract blood. And the same principle leads us to be very cautious in bleeding while a poisonous dose of opium or any other narcotic is in the stomach. Nay, in theory, the best means of preventing the operation of narcotic and other poisons that act by becoming absorbed, would be to throw a quantity of warm water into the veins. Magendie tried this on animals, and found it successful.

Thirdly, the experiment of Magendie, already related, of applying the *Upas tieuté* to the leg of a dog, connected to the body only by two quills, is another argument in favour of the operation of medicines by absorption.

The *last* argument which I shall adduce is, that some poisons, as prussic acid, are equally active when applied to the legs of an animal in whom the spinal marrow has been divided. In this case the effect of the poison could not be the result of its action on the nerves of sensation and voluntary motion. But it is said the division of the lumbar spine does not prevent the action of poisons, because it does not destroy the action of the sympathetic system, the nervous branches of which are distributed to the lining membrane of the blood-vessels. I am aware that it is an experiment liable to objection; but, on the whole, it is certainly favourable to the opinion of the operation of poisons by absorption; more particularly when we bear in mind that the motion of the blood is necessary to the action of the poison; for if the circulation of a part be obstructed, the poison will no longer act.

Taking into consideration, then, all these arguments, I can come to no other conclusion than that it is in the highest degree probable that the remote effects, of some substances, are produced in consequence of their absorption. I cannot admit this position to be at all invalidated by the experiments of Messrs. Morgan and Addison. Indeed, these physiologists do not deny the fact that poisons *may* enter the blood-vessels, and, in consequence, produce their poisonous effects on the system, but they deny the absolute necessity of this process. "It will be seen," say they, "that we are not opposed to the theory of venous absorption, but to the theory which would associate with it the *absolute necessity* for the admission of a poison into a vein, as a cause, and a sole cause, of its effects upon the body."

4. It being admitted, then, that certain medicinal or poisonous agents do get into the blood, and that when in this fluid they exercise a powerful influence over remote parts of the body, I proceed now to examine in what way this influence is communicated to distant organs. In the examination of this question, we meet with so many difficulties inseparably connected with the subject, that we cannot but acknowledge that any conclusion our reasoning may lead us to adopt, must be in the highest degree problematical. Viewing the question theoretically, we see three ways, by one or more of which a distant organ may become affected by the passage of medicinal molecules into the blood; namely—

(*a*). The medicinal particles circulating with the blood are brought into contact with all the tissues of the body, and thus an organ far distant from the stomach may be affected by local contact with the active molecules.

(*b*). Or we can conceive the possibility of the medicinal molecules making some impression on the lining membrane of the blood-vessels, and which, by sympathy, may be extended to remote parts.

(*c*). The properties of the blood may undergo some change by the contact of this fluid with the molecules of medicines, and the different organs of the body may have their actions or functions materially affected by receiving thus a vitiated blood.

Let us examine each of these supposed modes of operation separately, and endeavour to determine which has the greatest probability.

(*a*). It is the general opinion of physiologists, that after medicines or poisons have got into the blood, they are carried, in the ordinary course of circulation, to the heart, and from thence to the lungs. Here the blood undergoes certain chemical changes, and probably loses part of the foreign particles; at least this appears to be the case with respect to the molecules of certain odorous substances. The blood, still impregnated with medicinal particles, is returned to the heart, and from thence is transmitted to all parts of the system. In their passage through the tissues of the different organs, it is presumed that these particles act on one or more parts which are endowed with a peculiar susceptibility to their influence. Thus the opiate par-

ticles are supposed to exert a specific influence on the cerebral tissue; strychnia is thought to act on the tissue of the spinal marrow; mercury on the salivary glands, and so on. The molecules are supposed to be ultimately got rid of by the excretory organs. On this supposition, then, the blood is merely the "vehicle of introduction."

It must, however, be admitted, that this very plausible theory cannot be satisfactorily proved. We may adduce several arguments in favour of the opinion that medicines and poisons are conveyed to the parts on which their remote action is principally observed, but absolute proof or demonstration cannot be offered: our facts merely prove the passage of medicinal particles into the blood, and the affection of the remote organs; but the link which connects the two phenomena cannot be, or at least has not yet been, shewn. The strongest argument that, in my opinion, can be brought forward in favour of this theory, is, that the molecules of certain medicines may be detected in some one or more of the excreted fluids; and at the same time we find that the functions of the organs secreting or excreting these fluids have become influenced by the medicine. Now the simplest, and, therefore, the most plausible, explanation of these facts, is, that the molecules, in passing through the organ, acted on its tissue, and thus gave rise to the functional disorders. Let us select an example or two.

When nitre is exhibited, diuresis is frequently produced, while the salt may be recognized in the urine. The alkalies also usually have some diuretic effect, and at the same time alter the condition of the urine, either neutralizing the free acid, or even rendering this secretion alkaline. The oil of turpentine, in like manner, is supposed to produce its diuretic effect by local contact with the kidneys; for the urine acquires by its employment a violet odour. We are incapable of offering arguments of this kind in a large number of cases, either because the active principles of medicines are not easily detected, or because the part on which they act is not a secreting organ.

Thus, if it be true that strychnia acts on the spinal marrow by local contact, we cannot prove its presence in this part for the reasons just mentioned. We see, then, that this theory, though plausible, cannot from the nature of the evidence be easily proved. Is there any thing improbable in it? Can any arguments be adduced which are adverse to it? I am bound to acknowledge that there are. Many substances which have been swallowed may be detected in different secreted fluids, although they exercise no power or action over the secreting organs. Thus if a dose of rhubarb be swallowed, the colouring particles may be detected in the urine, although this secretion does not appear to be otherwise altered; and therefore it may be said that in those cases where the quantity of the secretion is affected, we have no right to infer that it depends on the passage of the medicinal particles through the secreting vessels. This objection, however, deserves but little attention, inasmuch as we know that the susceptibility of the same part is not the same to all medicines; for it is not every medicine which produces vomiting when applied to the stomach.

It has also been said that this theory of medicines "being conveyed by the circulation to particular parts, is utterly gratuitous, and no less improbable."—"What intelligence," says an American writer, "directs them, in this voyage of circumnavigation, to the port of destination; and how, on their arrival (admitting it to happen), are they separated from the great mass of fluids in which they are enveloped?" It is not supposed, on this theory, that medicines are *conveyed to particular parts;* we presume that the medicinal molecules are *conveyed to every part* of the body in which the blood circulates, but that certain parts only are susceptible to the influence of these particles. But, then, it may be replied, how is it that particular parts only are affected, since medicinal molecules are in contact with every part? We do not pretend to account for this circumstance. Every one is familiar with the fact that carbonic acid may be applied to the stomach in large quantity with impunity; whereas, if taken into the lungs, it acts as a narcotic poison. The urine has very little effect on the bladder, but if introduced into the cellular tissue, it gives rise to violent inflammation.

I have already alluded to another objection to this theory—namely, that injuries sometimes produce the same symptoms as poisons. But it must be recollected that in a large number of instances injuries do not produce the same symptoms; and in those cases where the effects of the two are analogous, I see no difficulty in assuming that there are two modes of affecting the nervous system: mechanical or other injury to the nervous fibrils; and the circulation of poisonous molecules in the cerebral vessels.

The most important objections that have been advanced against the operation of medicines through the circulation, by local contact with the tissues, are those founded on the experiments of Messrs. Morgan and Addison. Of all their expe-

riments, the following are, I conceive, the strongest against the theory under examination:—

The jugular vein of a full grown dog was secured by two temporary ligatures; one of which was tied round the upper, and the other round the lower part of the exposed vein. The vessel was then divided between these two ligatures, and the truncated extremities reconnected by means of a short brass cylinder or tube (fig. 9), within which was placed a portion

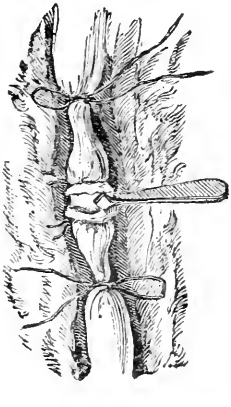


FIG. 8.

FIG. 9.

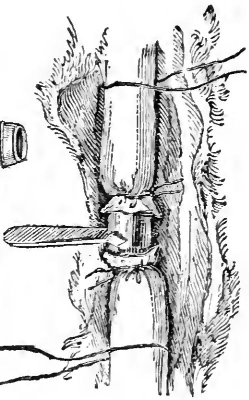


FIG. 10.

of woorara, of the size of a grain of canary-seed (fig. 8). Both the temporary ligatures were then removed (fig. 10), the accustomed circulation through the vessels was re-established, and in forty-five minutes the animal dropped on the ground, completely deprived of all power over the muscles of voluntary motion: in two minutes, convulsions and respiration had entirely ceased. This result was to be expected, whatever theory be adopted.

In another experiment, two temporary

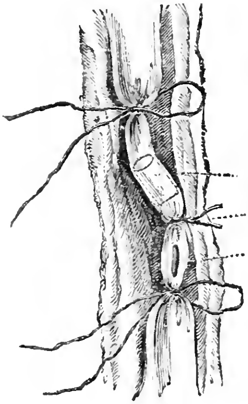


FIG. 11.

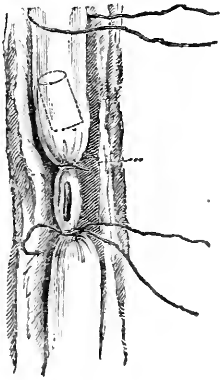


FIG. 12.

ligatures were applied to the jugular vein, as in the former case. A cylinder of quill containing a little woorara was then introduced into the vein between the two ligatures; another ligature was then applied (fig. 11), and the upper temporary ligature removed (fig. 12). In the space of 108 seconds after the removal of the ligature, the animal dropped in convulsions, as in the former case, and expired in 3¼ minutes. Now, in this experiment, the direct entrance of the poisoned blood into the heart, &c. was prevented by the lower ligature: hence, if this poison operated by contact with the brain, a greater length of time was necessary for its effects

to be produced; inasmuch as the circulation was no longer going on through the trunk of the jugular itself, and therefore if the poison acted by cerebral contact, it must have got into the system by the vessels of the vein.

The results of this experiment, it must be admitted, are somewhat difficult to account for, on the supposition that the woorara influences the brain by local contact, though they are not absolutely incompatible with it. Before, however, we can attach much weight to the experiment, it ought to be repeated several times, in order to determine whether the results obtained by Messrs. Morgan and Addison uniformly occur. It is necessary, as Dr. James Blundell has truly observed, "not to draw conclusions hastily from a few experiments, but, on the contrary, to multiply them as much as may be, since it is by performing the same experiment repeatedly that important exceptions are sometimes ascertained." But admitting that this experiment was performed with every attention to accuracy, and that the results obtained were correct, the most that can be inferred is that the woorara may disorder the cerebral functions without local contact with the brain On this supposition, then, woorara will not be peculiar in its operation; for concentrated hydrocyanic acid is presumed to act in a similar manner. We have no right to infer, because one poison (and that very imperfectly known) is presumed from one experiment not to act on the brain by local contact, that therefore no other poison can act thus. I cannot see the absurdity of admitting that the brain may be influenced in two ways—by local contact and by the medium of the nerves; I cannot admit that "all fair analogy forbids the conclusion." I cannot forbear here quoting an observation made by Mr. Travers on this point, more particularly as he is a supporter of the views of Messrs. Morgan and Addison. "But though it has been fairly argued that it is unphilosophical to admit two modes of explaining a phenomenon when one suffices, it cannot be denied that absorption by the blood may extend to the disintegration of it, and its incapacity of so acting on the nerves as to entertain life, any more than that it may be the vehicle of a subtle poison, which, upon reaching the sentient nerve, is instantly diffused over the nervous system, and puts a stop to its power."

The following experiment, related by Messrs. Morgan and Addison, appears to me to be of less weight than the one just mentioned. By the aid of a double brass tube (figs. 13 and 15) they established a complete double circulation between the carotids of a poisoned and of a sound dog, by connecting the lower and the upper ends of the divided arteries in both animals, so that each supplied the brain of the other with the portion of blood which had previously passed through the carotid artery to his own (fig. 14); and consequently the poisoned dog, in this case, received from the unpoisoned animal a supply of arterial blood equal to that with which he was parting. One of the dogs was then inoculated with a concentrated preparation of strychnia, which had been found, upon other occasions, to produce death in these animals in about three minutes and a half. In three

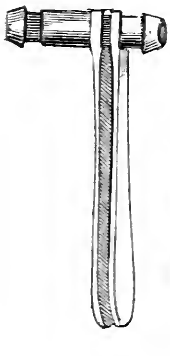


Fig. 13.

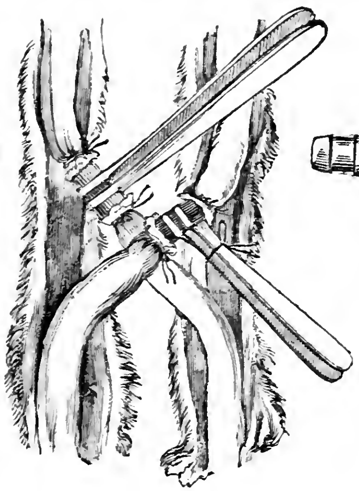


Fig. 14.

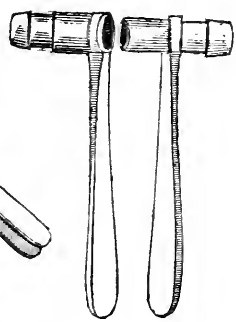


Fig. 15.

minutes and a half the inoculated animal exhibited the usual tetanic symptoms which result from the action of this poison, and died in a little less than four minutes afterwards—viz. about seven minutes from the time at which the poison was inserted; during the whole of which time a free and mutual interchange of blood between the two was clearly indicated, by the strong pulsation of the denuded vessels throughout their whole course. The arteries were next secured by ligature, and the living was separated from the dead animal; but neither during the operation nor at any subsequent period, did the survivor shew the slightest symptoms of the action of the poison upon the system.

The inference which has been drawn from this experiment is, that the arterial blood of an animal under the influence of poison is not poisonous. It appears to me, however, that this is not a necessary inference from their experiment; and as it is opposed to numerous facts, it requires careful examination ere we admit it.

Even if the blood of the inoculated animal had been really poisoned, the effect on the second animal would have been comparatively slight: for if the circulation of blood through the tube had been as free as through the artery, (which may be fairly doubted,) the second animal could receive little more than one-fourth only of the quantity sent to the brain alone, and, therefore, but a small part of the whole circulating blood in which was diffused the poison. Now as it is presumed that strychnia acts on the spinal marrow (not on the brain) by local contact, the blood thus sent to the brain would have to go through the usual route of the circulation before it could reach the medulla; and it is not too much to suppose, that during this transit some portion of the poison might be decomposed, or thrown out of the system, before it could reach the spinal marrow; and even if this were not the case, this organ could only receive a small portion (viz. that sent by the spinal arteries) of the poison contained in the system. This will, therefore, explain why a poison thrown into the arteries acts less powerfully than when thrown into the veins, unless it be the arteries supplying the parts on which the poison acts.

These reasons, then, lead me to doubt the correctness of the inferences which have been drawn from the ingenious experiments of Messrs. Morgan and Addison. When to these we add that Vernière found the blood of dogs killed by nux vomica was poisonous to leeches, and also to another dog when thrown into a vein, I think I am justified in stating that further experiments are required to disprove the theory of the operation of poisons by local contact.

Mr. Travers, in noticing the different results obtained by Vernière and Messrs. Morgan and Addison, says, that "if it be inquired why the poisoned blood concentrated below a ligature, and transferred into the vein of a healthy animal, proves destructive, while the blood of their common circulation affects only the one of the two animals which is the subject of the inoculation, the answer is obvious—that either the mechanical impulse fails, or the activity of the poison is exhausted before, in the latter case, it reaches the second animal." This admitted uncertainty, then, as to these experiments to which so much importance has been attached, proves, I think, that not one of them can be regarded as an *experimentum crucis.*

(*b.*)—It has been presumed by Messrs. Morgan and Addison, that when poisons pass into the circulation, and in this way affect the system, the effects result from the impression made upon "the sensible structure of the blood-vessels." And Mr. Travers says that the sentient properties of the lining membrane of the blood-vessels "is pre-eminently adapted to be impressed by the introduction of foreign substances or alterations in the condition of the blood:—their nerves are derived from the sympathetic or organic nervous system." Though I do not think it at all improbable that some medicines may act when they get into the circulation in the manner here assumed, yet further evidence is wanting on this point; for, as Bichât observes, sympathies in veins, like those in arteries, are very obscure, and we are but little acquainted with the influence which the tissues of these vessels may possess over other tissues.

(*c.*)—The operation of every medicine was formerly referred to the effects which it produced in altering the physical or chemical condition of the fluids, or, as they were termed, the *humors* of the body. To this doctrine of *humorism* succeeded that of *solidism*; in which the effects of all medicines were explained by alterations in the solids. These notions are now giving way to a kind of compound doctrine of *humoro-solidism*; and it is presumed that diseases are referrible sometimes to the blood, sometimes to the solids; and therefore that the operation of medicines may be either on the one or the other. It is possible that modifications in the qualities of the chyle, or of the lymph, may sometimes be the proximate cause of disease; but on this point we have no accurate information to guide us. No facts are known which demonstrate that the action of medicines and of poisons is primarily on the blood, although several circum-

stances favour the probability of such an opinion. We know that in many diseases the properties of the blood are altered; that in some cases the blood of a poisoned animal acquires poisonous qualities; and that by the use of poisons, medicines, or particular kinds of diet, we may alter the properties of the blood, and produce certain diseased states. It may, indeed, be said, that in these cases the alteration in the qualities of the blood is the consequence of the altered action of the solids; but when we bear in mind that the solids are nourished by the blood, and that they cannot support life without it, we cannot but admit that they must be influenced by the different conditions of this fluid.

Physiology, then, leads to the conclusion, that while alterations of the solids may be succeeded by alterations of the blood,—modifications of the blood may be succeeded by modifications of the solids; and, as an able continental writer has remarked, it is as hypothetical to assert that all medicines and poisons act primarily on the solids, as to assert that they all act on the blood: there is as much physiology in the one hypothesis as in the other.

ON THE

CHOLERA OF NORTH AMERICA.

To the Editor of the Medical Gazette.

SIR,

As the subject of Cholera still courts inquiry, I send the inclosed, if suitable, for insertion in your journal.—I am, sir,

Your obedient servant,

G. FARR,

M.R.C.S. & L.S.A.

Assist.-Surg. Ordn. Med. Depart.

Woolwich, Sept. 22, 1835.

In treating the subject of spasmodic cholera, more particularly as it appeared in the northernmost parts of America (where, however, its symptoms and appearances principally varied in the degree of their malignity, as compared with those exhibited in other parts of the world), my sole intention is to show the nature of its development, as influenced by the various modes of practice which were instituted to combat it; and to state what was finally deduced from these various means, by the majority of practitioners, as the most successful methods of treatment. In this attempt, I hope I may be excused in occupying the place of other matter, when it is considered that however desirable it may be to obtain a just estimate of the pathological nature of the disease, yet, as it has been of so fatal a character, it is the proper mode of treatment that we particularly want to see; and, therefore, he appears not to deserve ill who is willing to give an account of his own, especially when it has been more or less approved by other members of the profession with whom he has acted in concert. I may be permitted to presume, also, that the vastness of the field in which I was employed in those years, which will ever be memorable in North America—viz. 1832 and 1834, when the mortality from spasmodic cholera far exceeded that produced by the same disease in any other part of the world, is not the less a recommendation to my observations, having had the immediate charge of several hundreds of cases of more or less perfectly formed cholera, and having travelled upwards of a thousand miles through various districts where it either was, or had been raging; and, moreover, having personally witnessed and performed nearly every thing which I am about to state, which, it must be kept in mind, relates almost entirely to Canada, although there are few observations which will not apply, with nearly equal force, to the state of things at the same time in the adjoining republic.

Premonitory Symptoms.

I shall without farther remark commence with the disease itself, and leave any cursory observations till afterwards. There was, as in India and other places, a regular train of premonitory symptoms, more or less perfectly formed, in most cases; but in some it can hardly be said that there was any; for when a man who was about his ordinary business, apparently in tolerable health, suddenly fainted in the street, then vomited a little, turning cadaverous, and dying in half or three-quarters of an hour, without any purging at all, it cannot practically be said that he had any premonitory symptom; neither do any of these observations apply to such a case, as restorative means are here quite out of the question.

The person affected, then, with premonitory symptoms, felt an universal languor; impatience of interruption; pettishness, and dejection of spirits; loss of appetite; or, if the food was taken, it

remained like a heavy load at the pit of the stomach; some slight thirst, and other symptoms of indigestion, attended by giddiness of the head, and sometimes swimming of objects before the eyes; chills, and occasional numbness in different parts of the extremities; perhaps constipation of the bowels; in other cases something like the reverse; but not diarrhœa to an extent which I shall presently term the first stage of the disease.

I will not at present touch on treatment or remedies, but will go through with the progress of the disease, which, in a great majority of cases, is so systematic and methodical in its symptoms, as to allow of being divided into distinct stages, which, by the way, has been done by several writers, particularly Kennedy, although I shall take the liberty of observing a slight deviation or two from his rule. There will often, of course, be a difference in the real appearance of the disease and in its representation on paper. In the latter case we may, for the purposes of clearness and elucidation, have recourse to distinctive marks, and regular lines of difference in the symptoms, duration, and other circumstances of it, in order to see and understand its character better; but sometimes symptoms, and even stages, are blended together in the patient's case; and hence the value of experience, which, being attained, enables us still to treat it, in some measure, according to regular principles, although, at the time, we may be obliged to divest each division of its abstract grade.

Invasion of the Disease.

Under the head of the first stage of the disease it will be right to say a few words upon those articles of diet which, in all countries, act so perniciously upon the alimentary canal in hot weather, and which, owing to the excessive contrast of the summer and winter, together with the sudden cessation of the one season and the arrival of the other, act upon these organs in a most intemperate degree in America. Fish, aqueous fruits and vegetables, hard salted provisions, fermented liquors, ardent spirits, acids, and iced-water, are only a few of the articles which compose this almost indefinite tribe. These things in most countries, perhaps, act by producing diarrhœas and common cholera, both of which are produced in Canada, especially in the summer months, to a somewhat serious degree, from irregularities of diet, and also from the use of the water of the rivers, which is very strongly impregnated with lime and magnesia. Spasmodic cholera, then, happening to be epidemic, and diarrhœa being one of its common precursors, it is easy to imagine what facilities of dissemination it acquired: accordingly, the first stage of the disease commonly had marked symptoms of diarrhœa; these, however, were attended by some peculiarities. Upon loss of appetite, restlessness at night, slight thirst, heaviness and pain of the head, united with some other symptoms of the premonitory kind, would succeed an increased number of evacuations from the bowels, preceded by pain, and attended by tenesmus and tormina. These evacuations were characterised by nothing like bile; they were a little frothy and viscid, but nearly altogether composed of a disagreeable, clay-coloured or muddy water; at the same time very offensive. If this was allowed to go on for a few hours, numbness and tingling of the limbs supervened, attended by coldness, and in a short time by slight cramps. There is now a somewhat dark areola about the eyes and mouth; and sometimes at this early period the voice considerably alters, becoming both hoarser and weaker. The evacuations continue to be more watery, and prostration of strength rapidly ensues. Here, then, congestion has commenced, indicated by the more particular coldness of the limbs, the headache, and also by the state of the pulse, which is flagging.

The next, or second stage of the disease, is generally ushered in by the first, in a more or less developed state; sometimes, however, it advances immediately upon the patient, without any precursor of consequence. Here our attention is immediately awakened by symptoms of violent headache, great pain darting through the stomach and bowels, vomiting of a semi-transparent watery fluid, and constant stools of what has most aptly been called rice water. The fœtor of the stools is peculiar; and there is an offensive or deadly odour exhaling from the body, which odour is also peculiar and characteristic. If this state of things is allowed to proceed, the cramps which had commenced in the extremities increase in a short time to violent spasms, travelling on-

wards towards the central organs of life. The abdominal muscles and diaphragm become implicated, and a peculiar painful sensation, apparently sympathizing with these spasms, runs through the brain. The pulse soon becomes nearly imperceptible, the body cold, and the skin wrinkled about the hands and feet; the countenance sinks, and becomes cadaverous; the face wrinkled and shrivelled; the eyes sunk and cast upwards; the tongue becomes cold, and the thirst much more considerable; the voice hoarse, sunk, and guttural. Anxiety, great restlessness, fear, and occasional cries (sometimes vehement) characterize this state of things.

We find but too often that all our efforts to arrest the disease in this stage are futile, and with the most astonishing rapidity and vehemence this terrible malady advances steadily to the last stage. Now, then, the spasms put on the most violent form, particularly attacking the heart. The shrieks of the patient are sudden and piercing; the restlessness extreme; the pulse becomes quite imperceptible; the face and extremities much more discoloured, having a dingy blue hue, which sometimes increases to an alarming extent, appearing the colour of black-lead shaded with bronze. The countenance is horribly sunk; the eyes ghastly; the odour from the body very deadly; the feet and hands extremely soddened and shrivelled; the whole body is covered with a cold, clammy, watery fluid; the thirst is most intolerable, and the voice very much sunk, hoarse, and sepulchral. The dejections continue as frequent and copious for some time, and, in the majority of cases, a diminished vomiting continues. It is to be regretted that when the symptoms have gained this preeminence it almost invariably happens that the patient shortly sinks into perfect collapse: his appearance is then almost that of a dead person; he breathes in a hurried but feeble manner; his eyes are not quite closed; the cold sweat lies upon the surface of the body in large drops; the fæces pass involuntarily for a short time, and then cease; the vomiting ceases. The patient finally dies quietly, evidently possessing a correct knowledge of all that has taken place, and sensible of the presence of surrounding objects.

Sometimes the disease made its attack upon the patient in a different manner from that above recited (although not so suddenly as in those cases mentioned in the early part of this paper, where the patient might have been pursuing his ordinary occupation at the time of attack and almost immediate death); instead of coming on in a regular way, and proceeding through a regular series of symptoms and stages, its peculiar virulence was almost immediately developed amidst a confusion of symptoms of all kinds, which, after having proceeded with violence for a very limited time, were succeeded by perfect collapse. This is designated by Kennedy as the "rapid type;" and a more appropriate name could not have been given it.

There are a few other circumstances connected with this progress, and one or two anomalies. From about the time of the invasion of the second stage there is no secretion of urine; and I may add, that however violent the cries of the patient may be, whether male or female, you never observe tears, or any excretion from the nose. I do not think the secretion of saliva went on: although I never observed the tongue to be either remarkably white, or furred, yet I have seen both these symptoms in a moderate degree in different patients; but in the majority of them the tongue was nearly clean. It must be especially noted, too, that one of the earliest distressing symptoms is pain at the epigastrium, as noted by Mr. Annesley.

In patients who recovered, some trouble was occasioned in healing excoriations about the rectum, apparently arising from acridity in the matters passed from the intestines. A peculiar appearance sometimes takes place in the eye some hours before death; it consists in an extravasation, or ecchymosis, under the conjunctiva, upon the sclerotic coat, and, in general, has the appearance of a dark or black-coloured bar running transversely across the eye, below the cornea. I have witnessed it more particularly in cases where the rapid type has prevailed, and when the surface of the body has resumed its heat during collapse; for some remarkable exceptions to the common run of symptoms take place in the state of collapse. After a while the heat increases to an astonishing degree, so much so as to present a peculiar burning sensation to the touch; the perspiration becomes also heated, but with re-

spect to quantity remains the same; and this heat never diminishes until after death. This anomalous heat I have always observed to occur in cases where an extraordinary quantity of stimulus had been exhibited.

There was also, in many of the cases, after death, an extraordinary motion and confusion amongst the muscles in different parts of the body, and an irregular contraction going on to an astonishing extent. This may be seen in many parts of the body in a given number of cases; but, in the majority, happens most in the muscles of the thigh and abdomen. I have, indeed, seen the lower jaw separate itself completely from the upper, as though the patient opened his mouth wide to get breath, at a considerable period after death. The contraction of the muscles of the abdomen is often so perfect as to imitate the breathing of the patient, and to excite doubts whether he is not, in a most remarkable manner, reviving.

Treatment.

As a great deal of the practice instituted in Canada was unsuccessful, the bare mention of those means (which, indeed, were principally selected from the proposals of practitioners in other countries) will be sufficient, previous to reciting that which did appear, in some cases, to produce or facilitate recovery. Hot or warm water baths invariably appeared to injure the patients by exhaustion. Tinctures of a stimulating and heating kind never produced any decided benefit. Saline medicines, champaign, and cold water, seemed worse than useless, by very much augmenting the secretion of the alimentary canal, and so exhausting the powers of life. Nitric acid never acted well topically. Large opiates were not considered good in themselves, although opiates were used beneficially to a certain extent,—of which more anon. Acetate of lead and hydrocyanic acid did not produce much evident effect any way; and injections into the veins, although stated to be attended with successful results in other places, were followed either by apoplexy or delirium in all the cases in which I saw it tried: perhaps, however, there might have existed some untoward circumstances to account for this, either in the time, quantity, or method of administration; and the number of cases operated on were not numerous. I will, therefore, now briefly state that practice which, with a little modification, was most successfully pursued.

When a patient was attacked with premonitory symptoms, we administered an emetic of ipecacuanha, conjoined with a little carbonate of soda, a pediluvium, and an immediate immersion of the body between warm blankets: after a while, ten grains of calomel, with one of opium, were given: and the same medicines were repeated several times, in the proportions of two or three grains of calomel, with an eighth of a grain of opium, at intervals of three hours. One or two moderate doses of castor oil, to render the bowels sufficiently open after the secretions were healthy, generally effected the cure.

But in the first stage of the real disease we have to use both caution and expedition, whilst we employ the most energetic treatment which the patient is able to bear. Bleeding, in moderate quantity, was a very successful mode of practice, and was effected with all the speed possible; this process often relieved the urgent symptoms immediately. With respect to what kind of artificial heat it was best to employ at this stage of the disease, it became necessary to consider that prostration of strength was fast approaching; that the disease was then peculiarly violent, and of hasty progress; that the slightest exertion exhausted the remaining strength of the patient, which is the only foundation we have for our efforts; together with the general opinion of the medical men there, that, from what they had witnessed, the exhausting influence of warm water was of itself sufficient to destroy the remaining vitality of the patient. All these things, therefore, combined, made the application of *dry heat* I may say universal. Tin vessels of different sizes and shapes, made to fit the several parts of the patient's body, were filled with hot water, and applied as universally as possible; hot bran, or wheat, or barley, in bags of flannel, or hot sand in the same, together with a sufficient covering of flannel and blankets, were the means resorted to for applying it. As soon as possible, likewise, an emetic, consisting of forty or sixty grains of ipecacuanha, and half a drachm of carbonate of soda, combined, was administered, and rather a liberal quantity of warm water given, to excite vomiting: the result obtained

here by a pretty smart vomiting was generally a good one. After the commotion of the stomach had for a short time subsided, twenty (and sometimes thirty) grains of calomel, with half a grain or a grain of opium, was given; care, however, being taken during the whole treatment not to administer too much opium; the principal object in its exhibition being to make the calomel rest quietly on the stomach. If this first dose of calomel was rejected, it was repeated after a short interval, when it generally remained, unless the disease was inclined towards the next and more dangerous stage. Afterwards three grains of calomel, with an eighth of opium, were given every two or three hours, care being taken at the same time to keep up the heat of the body. The patient was not allowed much drink, a very small quantity of gruel or tea only. In this stage the cramps are often considerable, but are often relieved by heat; or if that does not succeed, by rubbing. I have, however, seen, even at this period, cramps so severe, as to leave behind them large extravasated blotches when the patient was recovering, as high up as the middle of the femur. The mercury was now carried on sufficiently far to produce ptyalism before the patient was considered safe. The subsequent treatment was commonly effected by castor oil, and a cautious introduction of diet. Port wine in small quantities, when sufficient recovery had taken place and sufficient time elapsed, was decidedly beneficial.

If the case had advanced to the second stage before the patient was seen (which not unfrequently occurred), the utmost care and expedition were required, in order to sustain the heat of the body, and also as much as possible to increase it; which was attempted by the means above mentioned. It was now a question of great importance to decide, and to decide expeditiously, whether or not venesection might be resorted to. For the most part, if the constitution of the person suffering seemed to be unimpaired by dissipation or infirmity, and the pulse retained a moderate volume, the result of a small abstraction of blood was beneficial; but if the constitution had suffered much from debauch, or sickness, or variations of climate, or if it was naturally weak, especially in advanced life, then, on the contrary, bleeding was invariably followed by quick collapse. An emetic was, however, given, but not so large a one as that in the former stage; for if great care was not taken to husband the strength of our patient, he sank almost without passing through the subsequent stage. But if the patient had been seen from the first, and, notwithstanding our efforts, the case had thus far advanced, then a domestic enema, with plenty of salt, was administered; and this usually contained an ounce of castor oil, and one or two ounces of spirit of turpentine; but sometimes, instead of these last, if the patient was of a very robust habit, from thirty to sixty grains of emetic tartar; and these injections were sometimes repeated with evident advantage. A rag dipped in spirits of turpentine, or a mustard plaister, was at the same time placed upon the region of the stomach, and retained there as long as it could be borne. The best stimulant wherewith to rub the external surface of the body, and to allay the cramps and spasms, was spirit of turpentine: but for this labour a considerable number of persons were required to be in attendance; and it was necessary, for the benefit of the patient, that they should be indefatigable. If possible calomel and opium were continued, in the proportion of a grain or two of the former, with an eighth of a grain of the latter, every quarter of an hour: occasionally twenty grains of carbonate of soda; a tea-spoonful or two of brandy whenever the pulse became very small; and to prevent large potations, a piece of ice now and then put into the mouth, which the patient always sucked with the utmost greediness. I have seen patients so employed almost in the act of dying.

But too often all these efforts are unavailing, and, with the utmost rapidity, this terrible disease advances onwards to the last stage. Little is then to be expected from our remedies; but yet every moment must be made the most of, and we must be as unremitting as possible in our exertions. And here I would say, that it requires no common share of courage to be enabled to act at all. The dreadful state in which the whole apartment is at this time excites so much horror, as well nigh to overcome the strongest mind. Emetics at

this crisis (supposing no previous treatment) for the most part were instrumental in destroying life, by excitement. Clysters were used, more as experiments than any thing else, composed of gruel mixed with two or three drachms of tincture of opium, and one, two, or three ounces of brandy, or from four to six of port wine. Some question was raised whether, at this period of the disorder, the clyster should be large or small in quantity; both were tried, and I should say that a decided preference was given to small ones; for although I am not prepared to say that the difference ever proved of sufficient importance to attribute recovery or death to it, yet, as in all the preceding stages, it had been considered best to allow but little fluid to be introduced into the alimentary canal, and as it never appeared that a large clyster produced benefit from its mere bulk, but perhaps served in part to exhaust the remaining powers of life, by causing so much to repass the anus (for these injections, however anodyne they were made, were speedily returned), operated in persuading us that the less the clysters the better the effect.

Very large doses of morphia were now given; ice was still used sparingly; and wine or brandy (which last is best) in exceedingly liberal doses, every three or five minutes. A drachm of rectified ether, with twenty grains of camphor, every five or ten minutes. These doses may appear large, but it is merely trifling with a patient to make them smaller at this most critical juncture; added to which, the system does not in any way appear to be acted upon by doses of an ordinary kind. Carbonate of ammonia, and, indeed, all the stimulants, were here resorted to; the indication being to prevent the collapse, which most imminently threatens every moment.

It is necessary that blistering should not be overlooked: if the symptoms were very urgent in the second stage, blistering became necessary, in order to have the advantage of time; but if we did not find ourselves called upon to apply this remedy so early, it always became necessary when the last stage had set in; and I venture to assert, that the more the patient's body was covered by blisters, the more chance he had of recovering. Blistering the whole spine, from the head to the coccyx; blistering the regions of the stomach and abdomen; applying blisters round the arms, legs, and even over the whole scalp, is what in some cases we successfully did. In one particular case, in 1832, this treatment was fully acted up to, in a second attack of cholera, when the patient was perfectly collapsed, saving a little warmth on the body, for nearly fifty hours; after which he perfectly recovered, and was, even very lately, officiating as a surgeryman in the Garrison Dispensary of Quebec. In a few other cases, the natural constitution seemed to assist in passing through this deadly trial, and to rally,—burnt brandy being principally administered by the mouth. In the rapid type the indication was altogether to keep up the heat of the body, and get stimulants to act in such a way as to produce revival. I need not, however, say, that our efforts were generally in vain.

The subsequent fever which occurred in those cases where recovery was not complete, possessed much of the typhoid type, and sometimes ran a most tedious course, yet subsiding at last. There appeared always to be gastro-enteritis; but I suppose rather as a consequence than a cause of the original disease. It is needless in this place to repeat the variable treatment which was resorted to; I shall only observe, that when these patients were convalescent, bottled porter seemed frequently to restore them to their former vigour sooner than any thing else.

Morbid Anatomy.

The principal post-mortem appearances which I have observed are the following:—Congestion of the vessels of the brain; ecchymoses on the pia mater at its base, particularly under the medulla oblongata; an absence of water in the ventricles; patches of ecchymoses immediately upon the trunk of the par vagum (these last above, but near the division of the carotid and subclavian); patches of small size upon the artery itself. Lungs extremely engorged; pericardium nearly empty. Heart covered with remarkably distinct, and somewhat large ecchymoses; serum and black blood coagulated in the auricles; extreme injection of the outer coat of the aorta; inner coat of the aorta having a

livid shade. Omentum considerably engorged. Stomach partly distended with a whitish semi-mucous liquor; internal surface flocculent. Duodenum two-thirds filled with biliary secretion, rather dark and inspissated; small intestines generally containing a bilious secretion for a short distance, sometimes rather fæcal; otherwise nearly empty; internal membrane flocculent; small intestines, particularly towards their termination, injected with bright-coloured blood. Colon partially filled with a whitish semi-transparent fluid, something like the dejections. Bladder empty, exceedingly collapsed, and drawn to the bottom of the pelvis. Kidneys gorged; liver gorged in its nervous system, and its substance altered in colour to an ash, or livid yellow. Gall-bladder nearly full, and its contents dark.

I have thus hastily drawn up this fragment, amidst a multiplicity of duties and a great variety of occupation. My desire is, that if it contains any thing worth knowing it may be made known; and for this reason I have not pretended to argue any points connected with the mysterious pathology of this fatal epidemic, however interesting in themselves, or showy in their announcement; but what I have written I have written with confidence, as of matters which I have myself seen and handled; and, moreover, have, in some measure, sadly experienced in my own person.

MUSCULAR IRRITABILITY.

To the Editor of the Medical Gazette.

SIR,

I FIND by Dr. Alison's reply of the 14th ult., that the objection of Dr. Allen Thomson was not exactly that which suggested itself to my mind; for I never doubted the accuracy of Dr. Reid's statement. I suspected the experiment to be an imperfect one, from which, consequently, no sound conclusion of any kind could be drawn: this suspicion is now confirmed by Dr. Alison himself, when he says that the irritability of the muscles was only *diminished.* There is, therefore, no reason to doubt Dr. Reid's accuracy, since, like those experiments of Mr. Hunter on the question of the impossibility of absorption by veins, it is clear that it may be perfectly true, and equally evident that it does not bear at all on the real point where the proof was necessary. Dr. Alison ought to bear in mind, that no evidence can be admitted which falls short of proving the existence of muscular action after the *complete destruction*, or *exhaustion*, of the nervous influence. If he is able to bring forward positive proof to this extent, I shall be ready to acknowledge that I have been in error; but if he cannot do this, which he will recollect has never yet been done, I should hope that he on his part will not hesitate to make a similar admission.

I may observe, in conclusion, that the grand point wherein the generality of physiologists have erred, is in conceiving that the nervous influence is destroyed by the division of a nerve, or of the spinal marrow, as when the head of an animal is cut off; whereas the nervous influence is, in fact, no more destroyed by such an injury, than the blood is destroyed in an artery when a ligature is thrown around it.

I remain, sir,
Your most obedient servant,
J. W. EARLE.

64, Welbeck-Street,
Oct. 5, 1835.

OBSERVATIONS

ON THE

CHANGES PRODUCED IN THE BLOOD IN THE COURSE OF ITS CIRCULATION:

WITH EXPERIMENTS.

Read to the Royal Medical Society of Edinburgh in 1823.*

WITH ADDITIONS AND REMARKS ON DISCOVERIES AND OPINIONS SUBSEQUENTLY PUBLISHED.

BY CHAS. J. B. WILLIAMS, M.D. F.R.S. &c.

[Concluded from preceding vol. page 876.]

On the Origin of Animal Heat—continued.

IF we compare with the general results of the experiments which have been

* This essay formed the subject of a thesis published in Latin in 1824; and in 1826 an abstract of it was published in the Transactions of the Medico-Chirurgical Society of Edinburgh. The additions are enclosed in brackets.

described or noticed in the preceding observations, the views which I have advanced respecting the changes of the blood and animal heat, we shall find them to be in perfect consistency with the phenomena, and explanatory of them.

It is not impossible to reconcile these views even with the extreme results of Mr. Brodie's experiments. According to our theory, the carbonic acid exhaled by respiration is what has previously existed ready formed in the blood. The usual quantity of this gas might therefore be *emitted* for the first half hour, without a sufficient quantity being actually *formed* to preserve the heat of the body. That little carbonic acid is formed where the nervous influence has been destroyed, appears further probable, from the fact observed by Le Gallois, that the blood does not in this case assume the proper venous hue; and this is, most likely, because the secretions, which we have shown to supply carbon for this purpose, are suppressed. This suppression also removes the other process which we have supposed to be a direct source of heat—namely, the formation of the principles of the secretions.

If it is possible to explain these extreme results, there can be no difficulty in accounting for the phenomena observed by other physiologists, and by myself, in which for a short time animal heat *was* kept up by artificial respiration. Hence we must either ascribe to the functions of respiration and circulation the power of generating heat (although to small extent, unless supported by other functions), or, with those who attribute to the nervous energy a power almost omnipotent—assume that the heat generated after the removal of the brain is produced by the nervous organs still remaining in the body. This latter alternative is so entirely destitute of support, that it is unnecessary to discuss it. I will, however, remark of the only author who, to my knowledge, has advocated it, that he had a particular hypothesis involved in it. I allude to Dr. Wilson Philip, who considers animal heat to be a *secretion*, a *tertium quid*, resulting from the operation of the nervous energy, or electric fluid (for he holds them to be the same), on arterial blood*. If, by this expression, he implies that the evolution of heat is the only consequence of the supposed action, I affirm that his proposition is not in accordance with the known properties of heat, nor with the generally received opinions of its nature; but if he admits that the supposed action is attended with other changes, besides the mere evolution of heat, his term *secretion* is logically inaccurate; for the heat must be the result of these other changes (whether in composition or in condition) of the principles of the blood, and not the direct effect of the supposed nervous power on this fluid. That such was the case in Dr. W. Philip's experiments with galvanism and arterial blood, a perusal of his account of them will sufficiently prove; for the rise in temperature which took place on passing the electric current through florid blood was attended with a blackening and obvious change in the chemical condition of this fluid.

[Since the foregoing remarks were written, the opinions of philosophers respecting heat have undergone some change, the notion having gained ground that it is the undulation of an elastic medium rather than a distinct matter. This will but little affect our subject; for we have less to do with the nature of heat than with the laws of its development. We find heat continually evolved in the animal body; and if we can point out according to what physical or chemical law it is there produced, we explain its cause as far as is necessary in the science of physiology. Any notion which falls short of this is not an explanation. Such appears to me to be the opinion of Dr. Wilson Philip, above quoted. To call heat a secretion, in spite of its want of parallelism with any other secretion, is to substitute an hypothetical, and probably erroneous, term for an explanation. With more plausibility animal heat might be referred to the calorific power of a current of electricity passing through an insufficient conductor. When a large quantity of electricity is sent through very fine metallic wires, it will heat them even to incandescence and fusion. The heat is here truly a *tertium quid* (a *secretion*, if you will), resulting directly from the action of electricity on the metal; there being no chemical or physical change to account for it. But if we admit (as I think we must) that there are electric currents continually present in the animal frame, there is no trace of the condi-

* Experimental Inquiry, &c.

tions under which they can produce heat, as in the case just described. There is no where in the body a sign of so strong a current, or of insufficient conductors. Besides, nearly all the solids and fluids of the body are electrolytes, and chemical changes would accompany the passage of electricity through them, as in the case of Dr. Philip's experiment, which destroys the analogy with the case of conduction by a metal. If, then, electricity be a direct cause of animal heat, it must be through some property not at present known; and to assume gratuitously the existence of such a property, is contrary to all rules of philosophy.

The hypothesis that electricity is the direct cause of animal heat being unsupported by analogy or experiment, we have again to inquire whether the position which I have advanced, that the chemical changes continually going on in the system will explain it, is still tenable. This view requires further consideration than I had given it in the preceding essay, and it may be conveniently examined under the following propositions:—

1. Certain kinds of chemical change are attended with an evolution of heat.

2. Such kinds of change do take place in the living body.

3. Relations are observed to subsist between these changes and the production of heat in the living body.

1. It is well known that when bodies pass from a gaseous to a liquid state, or from a liquid to a solid state, they give out heat. The same phenomenon accompanies any change in a body from a rarer to a denser condition. The most numerous and remarkable sources of heat are, however, the changes accompanying chemical action; and at the head of these stands the familiar one of combustion.

The phenomena of combustion depend on a certain intensity and rapidity of chemical action, which, when moderate and slow, does not give out heat sufficient to be luminous. Thus a mixture of hydrogen and oxygen in the proportions to form water, when heated sufficiently, unite instantaneously, and by the heat, light, and explosion, exhibit the intensity of their mutual action; but if heated to a lower degree, or if much diluted with some neutral gas, and exposed to spongy platinum, they then unite slowly and silently, still giving out heat, but not with the rapidity and quantity necessary to constitute combustion and explosion. This example, although elementary, illustrates generally the principles of calorific union. We are apt to associate the generation of heat especially with combustion, forgetting that this phenomenon exhibits only the higher degrees of what occurs also extensively when similar chemical changes take place more slowly, or when circumstances prevent the accumulation of heat. Water has this latter effect in a signal degree; and as this property is materially concerned in the calorific processes which are to come under our notice, I will illustrate it by a simple example. We know how sudden and vivid is the combustion of dry gunpowder. When the mutual action of its component parts is developed, nothing interferes with the extrication of heat, which is accordingly intense and instantaneous; but if the powder be moistened, the evaporation of the water impedes the heating process, and the combustion proceeds through the mass more slowly, and less vividly. Again, expose the chief components of gunpowder to each other's action in a watery medium, by warming sulphur and charcoal in nitric acid; the chemical changes are much the same as in the former case, and heat is evolved; but the liquid impedes its extrication and the rapidity of the action, which therefore go on in a lower degree, and for a longer time. This instance of gunpowder is given, because its constituents and their mutual action are well known; but it would be easy to shew that many other chemical actions, which, at their acmé, exhibit the phenomena of combustion, are, in their lower degrees, and when retarded by the presence of a cooling mass of water, still attended with the gradual extrication of minor degrees of heat.

In a somewhat similar light may be viewed the processes of fermentation and putrefaction, which develope low degrees of heat; and their chemical nature, although different from that of common combustion, may be proved to pass into it by successive gradations. It is thus, as I have elsewhere shown, that many cases of spontaneous combustion originate. Take, for example, the

spontaneous combustion of damp, or newly-made hay. This arises from a fermentation produced by the presence of moisture; but the heat thereby generated gradually dissipates the moisture, and developes the other ultimate affinities between the vegetable matter and the air. Hence the hay emits at first a fragrant steam, then an empyreumatic smoke, whilst its interior becomes charred by the increasing heat, which, under favourable circumstances, at last breaks out into open flame.

It would lead us beyond the immediate purport of this essay to pursue this part of the subject further here: the examples adduced will suffice to show how generally heat, in various degrees, accompanies certain chemical actions. *What are these actions which produce heat?* is a question still requiring a brief notice. It may be answered, *those kinds which tend to the formation of simple and permanent products.* The most efficient cause of heat by chemical change is the union of two contrary elements, with no opposing affinities to restrain their combination. Such is the union of oxygen with hydrogen, with carbon, &c. The next in calorific effect may be the open combustion of various animal and vegetable matters: the simplest products result; but the development of heat is somewhat impaired by the pre-existing slight affinities, and the presence of some oxygen, already in the combustibles. A lower degree of heat, but yet luminous, is produced in the low combustion which I have shown to affect most combustible bodies, both simple and compound, at temperatures considerably below red heat*. The products of this combustion in vegetable and animal matters are, according to the kind, chiefly water, empyreumatic acetic acid, oxalic acid, carbonic acid, and ammonia; all more simple in ultimate composition than the combustible. Lastly, the lowest cases of calorific change are those of fermentation and putrefaction. In one sugar is converted into alcohol and carbonic acid, or alcohol into acetic acid; in the other, animal and vegetable compounds are resolved into ammonia, carbonic and hydrocyanic acids, carburetted hydrogen, and the like: in both these cases the change being from the more complex organic principles to combinations simpler, and more resembling the permanent products of combustion. We may therefore reduce all the above instances to this general law—*the evolution of heat during chemical action is, cæteris paribus, in proportion to the change from isolation, or weak combination, to firm and simple union.*

2. Our next position to establish, is, that such chemical changes as have been just shown to evolve heat, do take place in the living body. We know of no processes in which the condensation of gases, or solidification of liquids, can become a source of animal heat. It is true that, in nutrition or reparation, the solids are formed of the liquid blood; but this is balanced by the opposite process of decay, in which the solid structures are again removed in a fluid state by the various absorbent vessels; and by the operation of chylification, in which a liquid is extracted from a more or less solid mass of aliment.

But the several chemical changes in the blood which we have been considering in the preceding pages, I apprehend we shall find included in the definition above given of the production of heat by chemical action. That the union of carbon and oxygen must prove in the body as elsewhere a source of some heat, is too obvious to need further argument. I shall here only consider the changes effected in the formation of the secretions. Here, from the highly perfect animal principles, albumen and fibrine, which originate only in living bodies, are susceptible of organization, and become the medium of some of the most remarkable properties of life,—we find produced, urea, uric acid, ammonia, picromel, resin of bile, lactic acid, and the like; matters not only totally insusceptible of organization, but obviously approaching in nature to inorganic substances, and capable of being formed by the decomposition of other organic matter, and in some instances even by the synthesis of inorganic compounds. Thus urea consists of precisely the same elements as the hydrated cyanate of ammonia; it may be formed artificially by the action of ammonia on cyanogen; and as it exists in urine, a boiling heat is sufficient to resolve it into the still simpler carbonate of ammonia.

* Transactions of the British Association for the Advancement of Science: vol. iii., Chemical Section.

The uric and lactic acids also approach to ultimate compounds; the former differing but little from some of the combinations of cyanogen, and the latter being the product of fermentation of milk and other animal fluids. Of picromel and the resin of bile we can speak with less certainty; but their ultimate composition, resembling oils, varieties of hydro-carbon, bespeaks the simplicity of their nature.

As we thus find the principle of calorific change fulfilled in the formation of the excretions of the animal body, so we may have experimental illustrations of it in the further history of the excreted matter. Thus in the septic properties and progressive decomposition of the urine and dung of animals we see a continuance of the simplifying process which had begun in the body; whilst the attendant heat, so familiar in the steaming dunghill, and usefully applied in hot-beds for forcing plants, exhibits the constancy of the phenomenon, which in its earlier periods had assisted to sustain the temperature of the living animal.

3. The last position to be examined, is, that relations are actually observed to subsist between the chemical changes going on in the body, and the animal temperature.

I need not dwell on the general relation observed between the heat and the perfection of the respiratory process in healthy animals. This has been sufficiently pointed out by the various writers on the subject, from the time of Dr. Black and Lavoisier. The experiments of Edwards have added a new proof, in the fact that young animals gain the power of preserving their own temperature in proportion as respiration becomes necessary to them. Animals born blind, or without covering, partake somewhat of the fœtal state, and will live for a short time without air; in this condition they have very little power of generating heat, and depend in great measure on the warmth of the mother: but as the respiration and connected functions become more perfect and indispensable, they gain the power of sustaining their own temperature. The exceptions to this law of relation in the experiments of Sir B. Brodie, and of those who have followed him, do not disprove it; they only shew that a certain integrity of the nervous function as well as of the respiratory is necessary for the preservation of animal heat. But, as it has been already observed in this essay, these experiments afford a direct illustration of the relation which we seek to establish between certain chemical changes and animal heat. A quotation already given from the accurate Chossat comprehends the result of his and all other experiments on this point. "*All the lesions of the nervous system which diminish the production of animal heat, act in a similar way on the secretions.*" Here, then, we find the exception proving the rule. A certain integrity of the nervous function is necessary to maintain animal heat, because it is necessary to the continuance of those chemical processes on which animal heat depends. It is directly necessary to the formation of the secretions, (one cause of heat) and as these supply carbon to maintain the production of the carbonic acid of respiration, (the other cause) the nervous energy is also necessary to the continued perfection of this function. Some production, as well as exhalation, of carbonic acid, may take place after the destruction of the nervous influence; and my own and other experiments shew that some heat is at the same time evolved. But this portion of calorific power is insufficient to preserve the heat of the blood: hence, besides the absence of the agency which forms the secretions, the increasing depression of temperature diminishes even the changes which the oxygen absorbed can *chemically* effect; the cooling is therefore progressively rapid, and when it has reached a certain degree, artificial respiration accelerates instead of retarding it.

I have before had occasion to cite from Dr. Hodgkin's notes to Edwards, some experiments of Sir A. Cooper which illustrate the effect of intense cold in arresting the changes of the blood in a sound animal, that in the veins retaining the arterial hue. It at once destroys that nervous influence, and impairs those chemical affinities, which jointly work the calorific changes. Such a moderate degree of cold as the vital powers can react upon has an opposite effect; more counteracting heat is generated, the blood becomes darker, and more carbonic acid is given out from the lungs;*

* Crawford, Lavoisier, &c.

and it is this exhibition of a property peculiarly vital, adapting the laboratory of the body to a variety of states, that distinguishes living from dead chemistry, and makes the very laws of decomposition subserve to maintain the heat and health of the body. We are familiar with the effect of external cold in increasing the secretion of the kidneys, and are used to ascribe it to the mechanical change of the circulation of blood thrown inwardly from the constricted surface; but nature's beneficial purposes are not limited to this, for in this inward determination of blood we also see a beautiful provision for the maintenance of animal temperature by an increase of those changes in which the internal viscera are materially concerned.

The relation of the heat and respiration of very young animals has already been noticed; but we have to add the correspondent relation of the state of the secretions. In the fœtus the secretions are scanty, and devoid of distinctive character, the power of producing heat being at the lowest; but as soon as the animal has breathed, the vital energies are excited, the chemical changes are promoted, the glands yield their peculiar secretions, and the power of maintaining heat is proportionately acquired.

These last considerations deter me from reckoning *nutrition* among the chief calorific processes of the animal body. The only way in which I see that it can contribute to produce heat is in the trifling extent to which it may (as before noticed) supply carbon for the respiratory function. But the opposite process, *decay*, which is supposed by physiologists to be continually affecting the tissues of the body, is essentially one of those simplifying changes which are always attended with an evolution of heat, and we thus find that this process, which has hitherto been deemed a result of defective composition, whether we view it as a distinct operation or as a part of the function of excretion, answers the useful and important purpose of contributing to sustain the heat of the body.

The necessity of a controling power over all the chemical changes which thus sustain animal heat is as obvious as is the need of a similar influence to predominate over the various physical properties of the animal machine; and if in the agent by which that control is exercised we recognise some characters which approximate it to electricity, we step but little higher in the mysteries of vitality. What directs this agent, and what are its relations to the physical and chemical laws which act upon it, and which it so powerfully and beneficially controls, are matters which according to our present knowledge are entirely beyond our comprehension. We have been studying the chemical changes as they do occur under this unknown influence, and find in them a sufficient cause of the heat of the animal frame. This inquiry, far from degrading our view of the animal economy into a mere application of chemical or mechanical laws, exalts and enlarges it, in the further proofs which it has afforded, that these laws, which in dead matter tend to decompose and to destroy, are made under the influence of vitality to warm, sustain, and purify the living body. In the variations of disease, which are too numerous to admit of present notice, we may find proofs of insubordination in the chemical agencies which sometimes seem even to predominate for a while; and in the balance of these against the opposite ones of vital reaction, are comprehended many of the phenomena of morbid action. The physical condition of temperature is, however, still amenable to the laws of its production; and in the highly charged excretions and increased carbonic acid expired in inflammatory fevers, we see the causes of augmented heat; whilst the lower temperature in typhoid, cachectic, and dropsical states, is accompanied by a diminished excretion from all the important organs.* Other causes may doubtless be assigned for these variations of functions and phenomena; nor do I deny their reality: and in adducing these new relations of acknowledged facts, I do but shew further instances of the admirable economy of nature, by which the chain of causation, although consisting of many links, is at once simple, comprehensive, and harmoniously adapted to a variety of ends.

Half-moon Street, Piccadilly,
Oct. 5, 1835.

* Nysten, Recherches, &c. p. 202. Apjohn, Dublin Hospital Reports, Vol. V.

COLLEGE OF PHYSICIANS.

DR. PHILIP IN REPLY TO DR. JOHNSON.

To the Editor of the Medical Gazette.

SIR,

IT has pleased the Editor of the Medico-Chirurgical Review, in the number just published, to make, under the title of a review of my Gulstonian Lectures, an attack on me in consequence of my having been elected a fellow of the College of Physicians.

The degree of credit due to his observations will appear when I state that I attended but one meeting of the College between that at which I obtained my license, about fifteen years ago, and that at which I was received as a fellow; and I never either directly or indirectly intimated to the President or any other Fellow any wish to obtain a fellowship, or took any step of any kind whatever with such a view: so far from it, indeed, that, although gratified by the flattering offer made to me, there was more than one reason which for several days caused me to hesitate whether I should avail myself of it: a fact well known to one of the most respectable members of our profession, whom I consulted on the occasion, and to whom I stated the reasons here referred to.

Such being the case, to what am I to ascribe a *tirade* written in absolute ignorance of the facts, and which sets all good taste and delicacy at defiance? Is it possible that it could have originated in any feelings which a man who has the least regard for his respectability would venture to acknowledge?

The truth is, that with regard to the by-laws of the College, which excluded from the fellowship, except under peculiar circumstances, the graduates of other Universities than those of Oxford and Cambridge, but which have since been repealed, having never experienced any inconvenience from an exclusion which implied nothing personal respecting the individual, I had never particularly directed my attention to the subject; and at the time I declined signing the petition of the Licentiates, I certainly had no reason to suppose that a fellowship would have been offered to me. My only reasons for refusing my signature were that just mentioned, my not having sufficiently attended to the subject to see all its bearings, and my dislike to come forward on such occasions.

Why is Dr. James Johnson so much offended by what I say of the medical scribblers of the day? It cannot apply to all medical reviewers, many of whom are among the best informed of our profession.

The review which follows, both with respect to the knowledge and feeling it displays, is such as might be expected after such a preface.

In all my late publications—a Treatise on the Preservation of Health, that on the Effects of Minute Doses of Mercury in gradually restoring the Vital Functions, the republication from the Philosophical Transactions under the title of A Treatise on the Nature of Sleep and Death, my Gulstonian Lectures on the more obscure affections of the brain, as well as my Treatise on Indigestion—I have had the same object in view; the application of the long series of experiments which have for the greater part of life occupied the time I could spare from the more active duties of the profession, to improve our knowledge of the nature and treatment of diseases; and it is gratifying to me that I am entitled to state, from the testimony of many members of our profession, that my objects have not been altogether unattained. But those who either will not be at the trouble, or are incapable, of entering with any accuracy into such inquiries, only see in the foregoing publications a reference to the same train of experiments, ; and here, as on other similar occasions, the most ignorant are the most confident. They neither see the difficulties, nor can judge of the means by which an attempt is made to remove them. What I have done will remain as far as it is consistent with truth. I wish nothing more.

I am, sir,
Your obedient servant,
A. P. W. PHILIP.

Cavendish Square,
Oct. 7, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Practical Observations on the Nature and Treatment of Nervous Diseases: with Remarks on the Efficacy of

Strychnine in the more obstinate Cases. By George Russell Mart, Member of the Royal College of Surgeons in London, &c.

Mr. Mart's preface sets forth—

"The following pages contain what may be considered new information respecting the treatment of such chronic nervous diseases as are too often pronounced incurable. The virtues of *strychnine*—the remedy chiefly used—are imperfectly known, and it has not received that degree of attention which its merits demand. Secondly, to prove that various complaints, if they do not admit of relief at one period, are capable of alleviation at another;—and to impress upon patients who have long been afflicted with these distressing maladies, the necessity of persevering in the method recommended in these pages; more especially, when it will be found that in the majority of cases which appeared hopeless, the method has been efficacious."

A word on this. We have in vain sought in the book for the "new matter," or information; the matter is very familiar—an old friend with a new face. Moreover, we deny that the "virtues of strychnine" are imperfectly known; nor are we strangers to its vices, any more than the dogs who have suffered in the cause. Assuredly, if Launce's dog were to look out of his grave, there would be much wringing of hands for those of his species who have become acquainted with the "virtues of strychnine."

The utter hopelessness of the public ever being able to appreciate or apply medical knowledge, diagnostically or curatively, must always expose the practitioner who tenders it, to the suspicion of having an *aim* less direct than the *sole* benefit of the laity. That this work is addressed rather to the public than the profession, the above quotation is an unfortunate proof. Apart from every other consideration, such as the danger of entrusting patients with so active and expeditious a poison as strychnia—the moral effect of attempting, or professing, to popularize physic, is exceedingly pernicious; for it brings into ill odour and contempt the whole body for the sins of the few, whose bad taste or cupidity have incited them to the medical misdemeanor of mantling empiricism by *book* advertisements,—a practice no less degrading and dishonest than Goss's or Eady's, and, unhappily, becoming nearly as frequent. To complete the misfortune, when the meanness meets with castigation and exposure, the cry of "jealousy" is raised by the populace, and joined in by the delinquent, who of course encourages the vulgar belief that the legitimate is envious of the wisdom and good fortune of the quack: in truth, the "public" is an exceeding fool, somewhat ungrateful withal, and far more likely to rend the medical watchman than the charlatan thief.

Mr. Mart adopts a notion with respect to nervous affections, strongly savouring of one of the rejected articles in the creed of humoral pathology, *viz.* that a "morbid state of the blood" must stand accountant for the sin of their production.

Few doubt the fact of a changed condition of the blood being competent to cause diseases of varying kinds and severity; but such affections are for the most part *constitutional*, not *local*, (if we except some cutaneous affections.) Few maladies are more decidedly *local* than the "nervous affections," for which strychnine has been so abundantly employed, and, (if we take the evidence of the impartial and disinterested,) with any thing but encouraging success.

Spasmodic, painful, and paralytic affections, depending upon lesions of the cerebral or spinal organs, are necessarily beyond the possibility of *cure*. Time may abate, remedies alleviate, but cure never. The same affections depending upon those intangible causes, called functional derangements, are often medicable, chiefly when treated on *general* principles, and sometimes by means which operate *sui generis*, as galvanism, or electricity, and magnetism, perhaps (?) Nevertheless, failure is sufficiently frequent to forbid the indulgence of a reasonable hope that "the more obstinate cases of nervous affections" may be tractable to any known remedies, not even excepting the virtues of strychnine. It were useless to deny that good results have been *apparent* from its use; but have these been the *effects* of the remedy? No cases have ever convinced us; yet writers of such books as Mr. Mart's would have us adopt the advice of Friar Michael,—to take the implication for absolute; and, without looking into the fact *whether*, seek only the reason *why?* And by consequence they are never deficient in hypotheses, "physical-natural, physical-historical, or natural-superinductive." Our belief is, that a willing self-delusion is practised when the expectation is indulged, and

talked of as *just not* inconsistent with possibility; that "specifics" may some day be found to add to the surprisingly long catalogue "bark, brimstone, and mercury:" and then we are favoured with the shrewd inquiry—Who would have thought sixty years since that so many apparent impossibilities would have been surmounted by modern science and ingenuity? Then follows a cloud of examples in point, such as steam-boats, gas-lights, and the musical glasses! Reasoners like these are ignorant of an impossibility *in limine*, or of an antagonist and incremental difficulty in many of their fine speculations; such are the persons who would hope to *double* the speed by merely *doubling* the power; they are of that school of philosophy, whose founder—we are told by Lear's fool—out of sheer kindness to his horse, buttered his hay!

To return to the author. His pathology is vague and indefinite; and his generalization appears to be of the kind that founds a general conclusion upon a single fact. He is unmindful of the powerful aid derived from the auxiliaries which were employed conjunctively with the "virtues of strychnine;" or, if cognizant, somewhat ungratefully omits to acknowledge the obligation. Sir Abel Handy says—none of his specifics being at hand,—"Well, perhaps the fire may go out of itself." Certain is it that many disorders realise the baronet's expectation, *quoad* the fire: they exhaust themselves; nervous and hysterical affections one of them; yet magnets, metallic tractors, with other equally questionable agents, appropriate the merit. There is not a class of disorders more numerous, more embarrassing, than that whose characteristics are referable to some permanently or periodically deranged condition of the nervous system: the fact of their being too often regarded as the results of organic disease, and subjecting the unhappy patient to active treatment, adds to their consequence; but we contend that they will never be better understood, nor their treatment improved upon, by regarding them as depending upon a general or common cause, which is certainly inferred by the general recommendation of a common remedy.

Disquisitiones de Animalium vertebris carentium in Ovo formatione: De Generatione Insectorum in Ovo. Auctore Maur. Heroldio, Jenensi, M.D. &c. Frankf. am Main, 1835. —Schloss.

We learn from the prefatory notice attached to this splendid work, that the author, Professor Herold, of Marpurg, has spent eighteen years in its preparation. We are not surprised at such an announcement when we see what has been done: the plates in the present fasciculus representing the development of the eggs of insects, as observed with the microscope, indicate the most unwearied diligence and perseverance. As a work of art we never saw any thing to equal the representation of the *Bombyx quercus*, just issued from the egg, given magnified to about 600 diameters: of the faithfulness of the figure we entertain no doubt, seeing the wonderful minuteness with which all the details are executed. Some interesting remarks on the comparative value of simple and compound microscopes are prefixed to the explanations of the plates: and the author candidly asserts that diligence in observing is a better auxiliary than any optical contrivance even of the highest powers.

There are to be four fasciculi altogether to complete the work. The present, which is the first, contains both outline and finished coloured copperplate views, 1, of miscellaneous ova and their elements; 2, the ovum of the *Bombyx quercus* at different intervals after birth; 3, that of the *Sphinx ocellata;* 4, the grub of the *B. quercus*, already alluded to; and 5, successive views of the development of the egg of the Blowfly.

The form of the work is in folio; and the letterpress is given in Latin and German.

De Pulsu, Resorptione, Auditu, et Tactu: Annotationes anatomicæ et physiologicæ. Auctore E. H. Weber. Lipsiæ: 4to.—Schloss.

Some of the most curious questions in physiology are here treated by the distinguished professor of anatomy at Leipsic. We could wish the first of these subjects had been allowed a larger space and more ample discussion; but the tract on the sense of Touch

compensates for the scant measure given to Hearing, Pulse, and Absorption.

On the subject of *touch* we have here an account of the numerous experiments instituted by the author regarding the subtlety of that sense, and a summary of all that is authentic concerning its peculiarities. On our perception of the part touched; on our estimation of the weight of bodies; our sensations of heat and cold; and on the delicacy of the sense of touch as compared with that of our other senses, we have many admirable remarks made by the learned professor. Among the facts mentioned under the last head, there are one or two with which our readers may be glad to be acquainted.

The author considers it as a point worthy of special notice that the sense of hearing has in some respects a marked superiority over sight and touch in regard to acuteness and sensibility. Although the sight has such advantages in the clearness and subtlety of its perceptions, it appears that in observing minute differences it falls short of hearing. For example: M. Weber shows that by *touch* we cannot distinguish between the respective gravities of ponderable bodies unless they differ by $\frac{1}{15}$th, or at the very least, by $\frac{1}{30}$th of their weight. By *sight*, in order to distinguish a difference between two lines, one must exceed the other by at least $\frac{1}{100}$th part of its length. Whereas by the sense of *hearing* we can distinguish sounds from one another when they merely differ by the $\frac{1}{322}$d part of the number of vibrations.

The volume is dedicated to the sacred shade of Scarpa: "*piis manibus Antonii Scarpæ*"!

Theoretisch-practische Geburtskunde durch Abbildungen erläutert.—Midwifery, Theoretical and Practical: illustrated by Plates. By W. H. Busch, Professor of Midwifery in the University of Berlin.

The plates, which constitute the staple of this work, form an excellent collection: they are gathered from all quarters—Hunter, Maygrier, &c. as well as from original sources. The style of lithography in which they are executed is of the best sort; and we have no doubt the whole will form a very complete and valuable production.

Nosologie und Therapie der Chirurgischen Krankheiten in verbindung mit der Beschreibung der Chirurgischen Operationem. Von C. J. M. Langenbeck, &c. &c. Fünfter Band. Erste Abtheilung.

The excellence of Professor Langenbeck's work on Practical Surgery is too well known to need description. We merely notice it here to announce the recent publication of the first part of the fifth volume.

Lehrbuch der Chemie. Von E. Mitscherlich. Zweiter Band. Erste Abth. Berlin, 1835.—Schloss.

This is another work which is famous on the continent, and especially among chemists. The admirably simple and popular manner in which all the processes are detailed and illustrated with engravings, renders the Elements of Professor Mitscherlich the best production of the kind in any language.

MEDICAL GAZETTE.

Saturday, October 10, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE LANCET AND THE APPRENTICESHIP SYSTEM.

That the labourer is worthy of his hire is a sacred as well as a familiar truth. That the demagogue who lives on popularity, and sucks through the crooked quill of imposture the uncertain tribute of credulity and ignorance, earns hardly, and enjoys precariously, *his* reward, is a fact that history sufficiently attests. The mountebank must be ever on the move: when *known* in a neighbourhood, his occupation is quickly gone. Falsehood, in order to captivate, must surprise; and to maintain its empire, must be ever new. It is like those fireworks which derive their brilliancy from the rapidity of their revolutions; stop

them, and they are seen to be but sorry squibs.

Reflections like these must suggest themselves to the mind of any one who contemplates, even for a moment, the rise, the progress, and the present situation of the Lancet. The offspring of a lawless disregard for property, it has pursued from the instant of its birth one undeviating course of profligate indecency. Reckless of the truth, regardless of aught save the private interests and private passions of its owners and conductors, its honesty has been accident, its knavery calculation. Circumstances have made it liberal. As the many are those who can best pay their supporters, the many are its clients. They who openly laugh at even a decent regard for truth, can scarcely be supposed to entertain a very active perception of principle. Principle, indeed, and the Lancet, are twain—the moral antipodes each of the other. The monotony of its malignity was relieved at first, by some of those charms with which ability has the power of adorning vice. With much ribaldry and billingsgate, there was often humour, and sometimes wit. But the volatile spirit has evaporated altogether, and the *caput mortuum* that remains is unmitigated trash. Talent, at least literary talent, has departed; and the man of education must turn with disgust from the stupid vulgarity of its pages. Yet the same sordid thirst for personal gain—the same indulgence of vindictive passions, are as constant in their exhibition, and as palpable in their workings amid decay, as in the palmy days of the supremacy of that journal. It is with it as with the bawd that Horace loathed—unquenchable venom

"Sævit circa jecur ulcerosum."

That the circumstance which has more immediately called forth these remarks may be appreciated, it is proper to inform our readers, that, on the Saturday preceding the first of October, it is the custom of our contemporary to publish what he calls a "Student's Number." The name is probably, and properly, given on the *lucus à non lucendo* principle, because it is a number which students ought not to read. And yet it is a curious number, for on these occasions the worthy Editor is usually in the habit of "going the whole hog," if we may use that phrase, and outdoing even himself. It is really astonishing to see how many lies, and what gross absurdities, are packed in this cargo of calumny. First there is a preface—a summary of fabrications, and then there is a leading article, especially addressed to students, and, of course, especially false. But we leave the actual falsehoods for the present, in order to direct attention to one or two of the sentiments and exhortations, with which our contemporary insults the understandings of the students he affects to patronize. His pages on these occasions remind us of Richardson's booth at a country-fair. The senseless clap-traps strut up and down in the presence of the gaping multitude, and the prestige is crowned by a jig before the mob, in which Hercules and Pantaloon, the heroine in blue satin and she who pockets the cash, "go in and out and round about," to the delight and bamboozlement of the crowd below.

All who have thought much, we might almost say who have thought at all, upon the subject of medical education, have arrived at the conclusion that the present system of apprenticeship is decidedly pernicious. There has latterly arisen a disposition to abate that nuisance, and to do away with the palpable absurdity of confining a young gentleman for five good years

behind a counter, where he learns very little unless it be to label a bottle or a pill-box, and to make himself agreeable to his master's maid. But observe the light that has dawned upon the mind of our honest contemporary. He has suddenly discovered that the apprenticeship system is the beau ideal of human wisdom. He has discovered that it is not lectures—no, nor hospital practice—that can teach a student the principles of his profession; but this same calumniated system of apprenticeship, this it is, which constitutes the veritable Amphitryon; this it is which, under the new light, will rear the future Crichtons in medicine and surgery. To be sure, the *honourable* advocate of this antiquated abuse has at present the majority of thinking persons opposed to him, and is much in the situation of the bedlamite, who accounted for his chains and straw by the circumstance of the world and himself entertaining different opinions, and the world being able to outvote him. But it is not those who think, that the Lancet aims at pleasing, for they are the minority. Those who "take apprentices" are the mass of the profession, and, by flattery, they may be also got *to take the Lancet*. Look to this, parents and guardians! students, look to this! You whom the Lancet addresses and cajoles, you whom it advises—and advises, to betray—see your real interests offered up, without one feeling of compunction, to the Moloch of its cupidity! By us, and such as think and act with us, you have been rescued from the parchment bonds with which the ignorance of barbarous ages had bound you; will you suffer yourselves to be fettered again, that the miserable scribblers who subsist by that journal—the

"Heaven's Swiss who fight for any God or man—

may enjoy a little longer the wages of their literary prostitution?

It may be thought that we wrong our respectable contemporary—that it is a libel upon his unspotted character to attribute to him the preposterous idea of advocating the system of apprenticeships. We present his own words; and, unlike himself, we do not garble nor misquote.

"Why is the knowledge which the country surgeon communicates to his pupil treated as nought by the extortioners of our colleges and companies? It is one of the grossest insults that was ever offered to a body of educated men. By the existing regulations, all the medical students in the kingdom, unless they have paid certain sums of money to other schools where the system of folly and of falsehood is carried partially into operation,---all the students, without exception, we repeat, are treated as so many dunces,---as mere beginners in the rudiments of medical education. What doctrine does this inculcate to the community? Why, that those surgeons who take apprentices are incapable of instructing them even in the very elements of professional knowledge. Thus, in the course of years, they are deprived of those fees which they are entitled to receive from their apprentices, because parents and guardians, naturally enough, contend, that if the knowledge which the student derives during his apprenticeship does not *lessen* the expense of a medical education in London, the money paid for the pupilage in the country is, virtually, thrown away. Thus a flagrant robbery is committed on the great body of surgeons and apothecaries who are engaged in private practice, and a most shameful injustice is inflicted on the student, who, during his apprenticeship, and under the able and generous guidance and instruction of his master, has succeeded in obtaining a practical and scientific knowledge of his profession."

How absurd it is to suppose that lectures and observation at the bedside and in the deadhouse of a hospital, can teach a young gentleman what he learns "under the able and generous guidance and instruction of his master" in the country. Observe the peculiar advantages enjoyed by the apprentice. Anatomy is the foundation of surgery and medicine. What are our schools, with the amount of ca-

pital expended for the comfort and instruction of the pupils? what are *they*, we say, to some snug little corner in North Wales, where bodies must be plenty, and the facilities for dissection incomparably great? What are your hired lecturers and demonstrators, who devote themselves to teaching, if not to learning, the science they profess, to some generous master, the cares of whose family and fatigues of whose practice qualify him most admirably for affording anatomical instruction to his pupils? Can the paid attendance of a teacher in a dissecting-room be placed for an instant in comparison with the easy and familiar style in which a country surgeon, having finished his day's work, initiates the neophyte in the mysteries of the human frame? Oh, no! the student who would learn anatomy should go where there are no bodies, no dissecting-room, no museum, and no teacher. He would not acquire that vulgar information which would tell him the course and the relations of arteries, and nerves, and muscles; but he probably would acquire a much more subtle and refined sort of knowledge—such knowledge as we may suppose the *honourable* propounder of this new doctrine to possess in his own person.

We have instanced anatomy, but the parallel will hold equally with the other divisions of medical study. In all, the Lancet's reasoning will enable us to lay down this important axiom, that the best way to qualify a student is to strip him of every advantage. The less he sees and hears, the more he is to learn—a conclusion extremely reasonable in itself, drawn, we cannot doubt, from our contemporary's personal case and experience. He, we believe, enjoyed the advantages of an apprenticeship, and great were the fruits, in leading to a professional career, which may have been short, indeed, but which was certainly brilliant, as was convincingly proved a few years ago in a Court of justice*. Success equally signal will probably be the lot of those young gentlemen who are wise enough to follow the advice of so eminent an example, and so trustworthy a guide.

THE GOWER STREET PUFFERS.

We thought we had done with the wily trickery of the Gower Street stock-jobbers: we flattered ourselves that we had abated some of their overweening pretensions: but they have returned, we find, to their old habits again. No gentry calculate better than do the worthy proprietors on the value of advertising themselves in every possible shape, particularly at certain seasons: and we should not quarrel with them for this, if they only did it fairly and with good faith. Mark, however, what they have been doing this week, in the way of blowing trumpets on the opening of the session. Poor Mr. Tooke is, as usual, converted into a stalking horse or a cat's paw in the service. He hawks his "statement" into the market, complaining bitterly of ministers for not granting a charter to the self-styled university, to grant degrees—which, he says, they promised to do. Now this has evidently been contrived to lead the public to suppose that they, the Gower Street joint-stock company, still have a chance of such a charter, when they know in their hearts that such a pretence is a gross deception. Next comes *Professor* Malden, with a letter in the *Times*, to inform the public that, though the "University" may not have the power of *giving* degrees, it, along with King's College, will have the power of *taking* them; thereby insinuating that there was no chance for students getting degrees in London otherwise than by entering the respective establishments in Gower Street or the Strand. We are glad to find that the barefaced manœuvre of the "Greek" professor has not been suffered to go unreproved: the *græculus esuriens* will be more cautious for the

* In the trial which took place on the subject of the calamitous fire in Mr. Wakley's dwelling-house, a medical gentleman, who acted as his deputy during an absence of "about ten days," deposed that he had been called upon to attend but one patient, "a decent woman lodging in Avery Row!"

future. But the party know they have not much longer to carry on the game of deception, and are accordingly very desperate. The charter, which they know is destined for them, and which will take them down to their proper dimensions, changing their jackdaw title of "University of London" into "London College," will shortly be before the public, when it will be seen that they must stand henceforth on their merits alone, and on a precisely similar footing with the other schools of the metropolis.

OPPOSITIONISTS OF ST. GEORGE'S.

EPISTLE FROM DR. WILSON.

We cannot present the following letter to our readers without premising one remark, on the ridiculous *touchiness* of the writer; it can only be compared to the sensibility of a skinned eel. But let Dr. Wilson speak for himself: we shall append a few notes to his "*grandis epistola*," though we confess there are parts of it at the meaning of which we can only guess.

To the Editor of the Medical Gazette.

Sir,

It is with extreme unwillingness that I address any communication to a journal which, while encouraging attacks on personal character, withholds the name of its Editor, and thus renders itself irresponsible to complaint.

In justice, however, to others, I am compelled to correct certain statements relating to the School of St. George's Hospital, which appears in the "leading article" of the last number of the Medical Gazette. It is there assumed that a school of anatomy, designated as that of Kinnerton-Street, is especially connected with the school of medicine and surgery established in St. George's Hospital, of which hospital school your readers are led to believe that the Kinnerton-Street school forms a part. As a governor of the hospital, as one of its physicians, and as a teacher in its medical school, I beg to inform you that such connexion does not, and cannot, exist.

It is further stated, that the lectures lately established at the school immediately adjoining St. George's Hospital, in Grosvenor Place, have been "set up in opposition to those in the hospital." This your assertion, sir, in the spirit of the term "opposition," I utterly deny. The lectures on physic, on surgery, and on other branches of medical education, now given at the school in Grosvenor Place, were established, not in opposition to the lectures delivered at the adjoining hospital, but in defence of the anatomical school, threatened in its very existence by the concerted, yet active, hostility of one of the surgeons of the hospital, against which no security could be obtained from the majority of the medical officers acting with the individual to whom I have alluded, and myself, in committee on the affairs of the hospital school.

On the system of disparagement by implication, long pursued by you with respect to the anatomical school under my direction and that of Mr. Lane, I shall offer no remarks. Within the last few days you have been joined by an associate lecturer, in every way worthy of the same high mission.

The incorrect and injurious statements to which I have directed your attention, have already received a contradiction the most public and direct.—I am, sir,

Your obedient servant,
James Arthur Wilson.

St. George's Hospital,
Oct. 6, 1835.

Upon the intelligible parts of this communication we have only to observe—

1. That the charge of personality in the outset is wholly gratuitous; that there was any thing of a personal nature in the article which seems to have roused the chivalry of the Doctor, we totally deny. If, however, his gallantry extends to the protection of the *honourable* member for Finsbury, we must only cry him mercy.

2. As to the Doctor's fidgettiness to find out the name of the Editor of this journal, it only makes us smile at his simplicity. We long ago made up our mind that a public journal, in order to be conducted with impartiality, and to deal out even-handed justice to all parties, ought not to have an editor's name fixed on the wrapper; and we are not going to change our principles at the bidding of the valorous Doctor. If he thinks any of our statements slanderous, false, or libellous, he knows our publishers, and our printer: let him be content with that. Truth is independent of personality: though it may be convenient for Dr. Wilson and his friends

to turn aside from facts, to find out and fasten upon, if possible, a writer's name.

3. With regard to Dr. Wilson's assertion, that such a connexion as is represented in the Gazette does not exist between the hospital school and the school of anatomy, we have only to repeat our former statement (to which we refer), and to add, that what the Doctor says only proves that he has been misinformed on the subject.

4. The intimation that the lectures at Mr. Lane's school are not in *opposition* to those given at the hospital, is amusing. The matter stands thus: lectures on medicine, surgery, materia medica, &c., it is well known, have for several years been delivered in the theatre of St. George's Hospital (No. 1, Grosvenor Place); Dr. Wilson and others have been procured to lecture on the same subjects at Mr. Lane's house (No. 2, Grosvenor Place). This we were simple enough to think constituted what is commonly understood by an "opposition;" but Dr. Wilson says it does not: of course we must bow to his decision.

The rest of the letter we do not understand, and therefore cannot pretend to answer. Dr. Wilson, however, appears to charge us with having in some manner disparaged his school,—which we directly and positively contradict: we merely spoke highly of the excellent, and, we are happy to learn, most flourishing establishment in Kinnerton-Street,—if, indeed, that can be construed into disparagement of the "opposition" school!

A word at parting. As we happen to know from experience that the worthy Dr. Wilson and his friends rejoice in a prodigious *copia verborum*, and that discussions on this subject cannot be very interesting to the general reader, we beg to decline further contributions of this nature; but venture to suggest instead, that *the long-promised* PAMPHLET (see the letters of Messrs. Lane and Co., published some months back in this journal) may serve the purpose of the whole party, in opening up all their grievances to a sympathizing world.—Ed. Gaz.

MUSEUMS AND HOSPITALS OF PARIS.

M. Orfila, as Dean of the Faculty of Paris, has just visited Holland. He went through the museums of Utrecht and Leyden, in the latter of which he had an opportunity of viewing the combined collections of Albinus, Raw, Bonn, Bruckmann, and other eminent anatomists. The object of the Dean has been to gather information on the modes of preparing practised in the Dutch cabinets, as well as to ascertain what may be wanting for the new "Museum Dupuytren," which it is intended shall be the richest and grandest in Europe. In visiting the hospitals of Rotterdam, the Hague, Amsterdam, Breda, and the towns already mentioned, M. Orfila is said to have gathered much useful information respecting the best mode of pursuing medical studies, which no doubt he will turn to account, as the project of law of the French government regarding medical education is still on the tapis.—*Journ. Hebdom.*

GUACO A REMEDY FOR GOUT AND ASTHMA.

In the year 1788, Mutis, a Spanish botanist, learned from an Indian the preservative properties of the guaco against the bites of serpents. He and his friends tried it, by inoculating some of the Indians with the saliva of a venomous serpent, in which the presence of the poisoned fangs was ascertained. One of the party was also bit in the hand. The application of the leaves of the guaco arrested the occurrence of consequences which would otherwise have been fatal. The Indians regularly inoculate themselves with juice of guaco to protect the system from serpent bites; the juice is also taken internally, by way of reinforcing the inoculation. The guaco is said also to be efficacious where hydrophobia is apprehended. In arthritic pains, gout, convulsions, tetanus, and other disorders, the guaco is said to act as a specific; but the list of affections in which it is boasted to be beneficial is so heterogeneous, that little confidence can be reposed in it.—*Casper's Wochenschrift.*

NEW DIVISION OF THE ANIMAL KINGDOM.

M. Ehrenberg has lately presented to the Academy of Sciences, Paris, a new plan of dividing the animal kingdom into twenty-eight classes, founded on organization. M. de Humboldt, who explained M. Ehrenberg's system to the Academy, stated that twenty-two of the twenty-eight classes were of the invertebrate animals,

which are divided into *cordata* and *vasculosa*, according as they are possessed or are destitute of a heart. In the latter the vessels offer no pulsations, and the rapid movement of the fluid within them is promoted by the oscillation of the internal walls. The digestive organ is either simple, as in the *tubulata*, or multiform, as in the *racemifera*, the lowest classes of which having ciliæ and hermaphrodite organs, constitute the *infusoria* or *polygastrica*.

In the course of two expeditions which he made (one in Syria, Nubia, Dongola, and along the shores of the Red Sea; the other in the north of Asia, and about the Caspian Sea), M. Ehrenberg had an opportunity of observing a vast number of these organizations. But the plan of dividing them now suggested he by no means considers as perfect; he merely proposes it as an essay susceptible of development and *perfectionnement*.—*Gaz. Médicale.*

HAHNEMANN AND HIS DISCIPLES.

The French government has licensed Dr. Hahnemann, the founder of homœopathy, to practise his profession in France. It is doubtful whether his disciples in the capital will retain the due respect for their master, who is now become their rival.—*Gaz. des Hôpitaux.*

ASPHYXIA OF INFANTS IN THE BIRTH.

M. Baudelocque announces that he has saved the lives of two children, footlings, whose heads remained long engaged in the pelvis. The plan adopted was to introduce into the mouth a silver sound of sufficient capacity to permit the entrance and exit of air in the act of respiration.—*Gaz. Médicale.*

LONGEVITY OF BIRDS.

A wild goose was lately shot at Schievenhorst, near Dantzic, having round its neck a metallic collar, on which was inscribed the date 1800.—*Gaz. des Hôp.*

LITERARY INTELLIGENCE.

A Compendium of the Ligaments, illustrated by Woodcuts; with the Dislocations, and an Epitome of the Physiology, Pathology, and Fractures of the Joints. By A. M'Nab, jun., M.R.C.S.

NEW MEDICAL WORKS.

Observations on the Action of the Broom Seed in Dropsical Affections. By R. Pearson, M.D. 8vo. 2s. 6d. cloth.

Dr. J. Lebaudy's Anatomy of the Regions interested in Surgical Operations. Royal 4to. 24s. bds.

Practical Anatomy of the Nerves, &c. supplying the Head, Neck, &c. By E. Cock. 12mo. 7s. cloth.

Mayo's Outlines of Human Pathology. Part I. 8vo. 8s.

Phelan's Inquiry into the Medical Charities of Ireland. 8vo. 10s. 6d.

Introduction to Hospital Practice. By C. J. B. Aldis, M.B.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

October 3d and 8th, 1835.

John Moyle, Cornwall.
William Ellison, Liverpool.
Charles Thomason Thompson, Brightwell, Berks.
Magnus Francis Lynch Andrews, London.
Henry Wooldridge, Winchester.
William Lees Underhill, Tipton.
Charles Arnold, Grantham.
Donald Dalrymple, Norwich.
Dennis Rock, Birmingham.
Alfred Henry Wagstaff, Leighton Buzzard.
Samuel Legh, Hordley, Salop.
George Whitmarsh, Downton, Wilts.
William Croser, Newcastle-upon-Tyne.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Oct.* 6, 1835.

Abscess	2	Hooping Cough	3
Age and Debility	31	Inflammation	16
Apoplexy	9	Bowels & Stomach	3
Asthma	3	Brain	1
Childbirth	4	Lungs and Pleura	4
Consumption	44	Liver, diseased	1
Convulsions	29	Locked Jaw	1
Croup	2	Measles	7
Dentition or Teething	6	Mortification	6
Diarrhœa	2	Rheumatism	1
Dropsy	10	Small-Pox	15
Dropsy on the Brain	15	Sore Throat and Quinsey	2
Dropsy on the Chest	1	Spasms	1
Fever	3	Unknown Causes	3
Fever, Scarlet	6		
Gout	1		
Heart, diseased	1	Stillborn	13

Decrease of Burials, as compared with the preceding week . . . 84

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Oct. 1835.	Thermometer.		Barometer.	
Thursday . 1	from 50	to 58	29·19	to 29·20
Friday . . 2	52	58	29·20	29·28
Saturday . 3	38	56	29 31	29·25
Sunday . . 4	48	55	29 29	29 47
Monday . . 5	40	57	29·58	29·73
Tuesday . . 6	39	61	29·80	29 87
Wednesday 7	40	57	29·93	30·02

Prevailing wind, S.W.

Except the 5th, generally cloudy, with frequent showers of rain.

Rain fallen, 1 inch, and ·75 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 17, 1835.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE III.

OPERATION OF MEDICINES BY SYMPATHY.

IN my last lecture I mentioned two ways by which it may be presumed the remote effects of medicines are produced—namely, absorption and sympathy. Having fully discussed the first mode of operation, I proceed in this lecture to examine the second.

Some medicinal agents affect remote parts of the body, under circumstances which do not authorize us to conclude that they act by absorption; such substances are said to operate *by sympathy*. Let us inquire into the meaning attached to this expression.

All the different parts of an organized being have certain relations or connexions with each other; which are divided into three kinds or varieties,—the mechanical, the functional, and the sympathetic. It frequently happens that one organ in the performance of its functions exercises a mechanical influence over another. Thus the motions of the respiratory muscles have an important influence over the circulation of blood within the chest. By the contraction of the muscles of the fore-arm pressure is made on the deep-seated veins, and the passage of blood through them thereby obstructed. These, then, are evident and clear cases of a *mechanical relation* between certain organs. We have also numerous instances of what I have called *functional relations*; but one example will suffice: the liver cannot secrete bile if the supply of arterial blood, or of nervous energy, be cut off: hence it is evident that the function of the liver depends for its performance on the proper execution of the functions of the arterial and nervous systems.

But there is a class of relations which cannot be referred to either of the preceding heads, and which are called the *true sympathies*, or the *sympathetic relations*. Thus if we titilate the mucous membrane of the nose, sneezing is produced; if the soft palate, vomiting. These are but two out of many instances in which relations exist between organs connected neither mechanically nor functionally. We know not how sympathies are maintained; the membranes, the cellular tissue, the blood-vessels, and the nerves, have each in turn been charged with this office; but none of these modes of explanation are free from objection. The most plausible hypothesis is, that the nervous system is the seat of sympathies; but the difficulty in admitting this supposition arises from the evident proofs of sympathy which exist in vegetables, beings not known to possess nerves, or a nervous system. Dutrochet, indeed, asserts that the small points, or spots, observed on the cells and vessels of plants (figs. 16 and 17), are analogous to the nervous globules of animals; he calls them *nervous corpuscules*, and regards them as the scattered elements of a diffused nervous system. That globules are found in vegetables in the situation described by Dutrochet no one can deny; but the grounds on

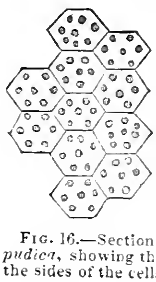
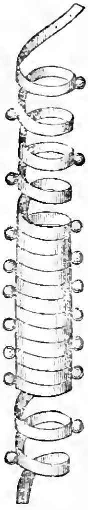


FIG. 16.—Section of the medulla of the *Mimosa pudica*, showing the globular bodies adhering to the sides of the cells.



which he asserts they are nervous, are slender indeed. The researches of Leeuwenhoek, Prochaska, Fontana, Sir Everard Home, Bauer, the brothers Wenzel, and Dr. Milne Edwards, have shown that the nervous system of animals is composed essentially of agglomerated globular corpuscules. Here is a drawing (fig. 18) representing the globular cells of the pulpy matter forming the brain of *Limax rufus* (red slug), and of *Helix pomatia* (the great or vineyard snail), molluscous animals of the class *Gasteropoda*. Observe the corpuscules on the sides of the cells. Here, says Dutrochet, is a structure evidently similar to the medullary tissue of vegetables; and the analogy extends beyond the mere physical characters, for we find the two structures agree in their

FIG. 17.—Spiral vessel found in the stems of *Solanum tuberosum* and *Cucurbita pepo*, with the adhering globules.

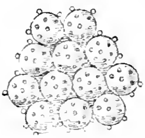


FIG. 18.

chemical properties: the globular corpuscules both of animals and vegetables are rendered opaque by boiling in nitric acid, while a solution of potash restores their transparency. These corpuscules, then, are regarded, by the before-mentioned celebrated physiologist, as the organs producing the nervous power.

In the lower classes of animals which have no nerves—namely, the zoophytes—we meet with a kind of diffused nervous system. Here is a magnified representation of one of the arms of a *Hydra* (fig. 19), showing the distribution of the nervous corpuscules. In this animal, the *Vorticella convallaria* (fig. 20), these corpuscules are much less numerous, but are still observed; they occupy the central part of the branches.

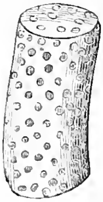


FIG. 19.

On these grounds, then, Dutrochet assumes that the little globules sticking to the cells and vessels of plants, and which are nothing but particles of amylaceous or resinous matter, are to be regarded as the scattered elements of a diffused nervous system. Two facts seem to disprove his hypothesis: first, plants that do not offer the least trace of animal sensibility have globules in as great abundance as those vegetables (*Mimosa pudica*, for example) that really appear to possess it; and secondly, those plants which offer the strongest symptoms of sensation are not those that

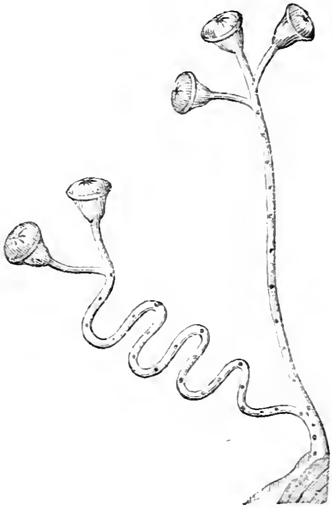


FIG. 20.

approach the nearest to zoophytes in structure. I think, then, we may say with Bichat that the causes of sympathies are completely unknown: a dark veil still conceals from us the agents of communication which connect the organs from whence the sympathetic influence is conveyed, to the part which receives it.

Four circumstances lead us to infer that particular substances produce certain remote effects on the system, independently of absorption; these are as follows:—

1. The instantaneous operation of some medicines.

2. The remote effects not being in proportion to the absorbing power of the part.

3. Dilution diminishing the remote action.

4. Mechanical injury affecting remote organs.

1. Observe the instantaneous effect of strong hydrocyanic acid. I apply the nose of a rabbit to the mouth of a receiver, filled with the vapour of this acid, and you see the animal is instantly killed. In this and other cases the operation is, I think, too quick to admit of the supposition that absorption was the cause of the phenomenon. Sir Benjamin Brodie has remarked, that he once happened to touch his tongue with the end of a glass rod, which had been dipped in the essential oil of bitter almonds; and he says that he had scarcely done so, before he felt an uneasy indescribable sensation in the pit of the stomach, great feebleness of limbs, and loss of power to direct the muscles, so that he could hardly keep himself from falling.

2. Orfila found that alcohol acted with

much less energy when injected into the cellular texture than when taken into the stomach; and as the power of absorption is greater in the former than in the latter part, he concludes that the remote action of alcohol must be produced by the agency of the nerves. Opium, on the contrary, is supposed to operate by absorption, because it is more active when injected into the cellular texture of the thigh than when taken into the stomach.

3. The effect of dilution on the action of medicinal and poisonous agents oftentimes assists us in determining the mode by which the remote effects take place. Thus if we apply a strong mineral acid to the stomach, great disorder of the general system is produced; but if we dilute the acid previous to its exhibition, little disturbance in the system is observed. Now as dilution facilitates absorption, it is improbable that the constitutional disorder caused by swallowing strong mineral acids depends on their absorption. On the other hand, Dr. Christison has shown that oxalic acid, considerably diluted, quickly enters the blood, and causes speedy death; hence it is presumed to kill in consequence of absorption.

4. Mechanical injuries sometimes give rise to effects on remote parts similar to those caused by the action of certain medicines or poisons. Laceration of the stomach produces similar constitutional disorder to that occasioned by the strong acids.

The action of medicines by what authors call *continuity* and *contiguity* of organs is only an example of sympathetic operation. By irritating the mouth of the excretory duct of a gland we promote excretion; and by applying clysters or suppositories to the rectum, we excite secretion and contraction in the intestinal tube above the rectum: these are examples of the action of medicines by *continuity* of organs. When we give aloes, hellebore, or savine (substances which irritate strongly the lower portion of the large intestines) to affect the uterus, or when we employ demulcents to lubricate the pharynx and œsophagus, and thereby to alleviate cough, we are acting on the principle of *contiguous* sympathy.

Peculiar Effects of Medicines.

Having now discussed the question as to how the remote parts of the system become affected, I proceed to examine the *locality* and *quality* of the effects of medicines; and

First, of the *locality* of the operation. Most medicines have some one or more organs on which they exert their especial influence; and in consequence of the mechanical, functional, and sympathetic relation which this organ has with the other organs of the body, the whole system becomes, in a secondary way, more or less disordered. These are the *Specifica topica* of Hufeland. Dr. Christison has justly remarked, that there are few substances which affect primarily and simultaneously many parts of the body: arsenic and mercury, however, do appear to have this effect. The following are instances of parts specifically affected by medicines:—

1. The brain, by the substances commonly called narcotics and inebrients.
2. The spinal marrow, by the substances containing strychnia and brucia, to which we may probably add mercury and lead.
3. The circulatory organs, by stimulants, tobacco, arsenic, digitalis, &c.
4. The lungs, by tartar emetic, emetia, corrosive sublimate, &c.
5. The alimentary tube, by some emetics and purgatives.
6. The urinary apparatus, by diuretics, cantharides, &c.
7. The skin, by some diaphoretics.
8. The salivary glands, by mercury, and also sometimes by hydrocyanic and nitric acids, opium, arsenic, fox-glove, &c.
9. The mucous surfaces, by mercury, turpentine, balsam of copaiba.
10. The lymphatic system, perhaps by mercury and iodine.
11. The organs of reproduction, by ergot of rye, cantharides, phosphorus (?), and some of the substances denominated emmenagogues.
12. The liver, probably by mercury.
13. The throat, by belladonna.

This list might be much extended, but it is difficult to satisfy one's mind in many cases; and, indeed, in this list I have inserted several substances whose operation on the parts mentioned, though generally believed, has been less satisfactorily proved. Many remedies disorder several of the functions of the body, but it is sometimes not easy to determine which was the first effected.

Secondly, of the quality, or nature, of the effects produced by medicines.—Theoretically we can easily conceive that the functions of the system may be influenced in two ways—namely, *quantitatively*, or in respect to the *degree* of their action; and *qualitatively*, or in reference to the *nature* of the action induced. It is, however, difficult to select pure and unequivocal instances of either mode of operation solely, since most medicines give rise to simultaneous quantitative and qualitative changes.

Dr. John Brown, the ingenious author of the *Elementa Medicinæ*, assumes that all medicines are stimulants, differing only in

the degree of their operation. His views may be thus explained: all living beings possess a peculiar principle, termed *excitability*, and which distinguishes them from inanimate bodies. Certain agents, called by him *exciting powers*, act on the *excitability*, and thus maintain life, or, in the language of Brunonism, produce *excitement*. Whatever can modify the excitability, and produce a greater or less degree of excitement, are termed *stimulant powers*. Hence, therefore, medicines, like other external agents, are stimulants; some of them causing excitement of the whole system, others of a part of it.

The author of this doctrine also assumes that medicines differ from each other in little more than the degree in which they exert their stimulant power;—so that digitalis, hydrocyanic acid, cinchona, mercury, opium, alcohol, cold, and even bloodletting, are all stimulants, differing only in degree. But what right have we to assume all medicines or other agents, stimulants? Where is the evidence of this stimulant effect when we apply strong hydrocyanic acid to the tongue of an animal, and thereby produce instantaneous death? Is it not absurd to suppose that alcohol, mercury, cinchona, and opium, differ in the *degree* of their operation only? The apparent effects produced are so dissimilar, that we are compelled to suppose a difference in the *nature* or quality of their operation.

These difficulties in the way of the Brunonian doctrine led some physicians of Italy to modify it; and thus arose the *theory of contra-stimulus* of Rasori, Borda, and Tommasini. Some medicines, say these writers, stimulate, others have a contrary power: the first they call stimulants, the second contra-stimulants. Is this term contra-stimulant synonymous with sedative? Certainly not; for sedatives, properly so called, are those substances which produce a depressing action upon the system in a state of health. Now a contra-stimulant is a substance which merely counteracts a state of excitement, and the term, therefore, becomes applicable to many bodies whose action is unequivocally stimulant. Perhaps our word antiphlogistic is most analogous in its meaning to that of contra-stimulant; but by the Italian physicians would not be regarded as synonymous, for according to their notions contra-stimulants diminish excitement, by depressing the excitability of the fibre by a kind of specific property, which no one thinks of assigning to our antiphlogistics. The difference between the two, however, is after all only theoretical. The test of the contra stimulant power of medicines is their obviating or counteracting the effects of some well-known and well-characterised stimulant. Thus alcohol is put down as a stimulant, and hence any agent which relieves the inebriation produced by it, is denominated a contra-stimulant. Proceeding in this way, then, we may form the following classification:

Stimulants.	*Contra-stimulants.*
Caloric.	Cold.
Opium.	Bloodletting.
Musk.	Metallic medicines.
Camphor.	Emollients.
Phosphorus.	Tonics.
Æther.	Ipecacuanha.
Ammonia.	Purgatives.
Wine.	Hydrocyanic acid.
Alcohol.	Digitalis, belladonna, stramonium, &c.
Carbonic acid.	
The electric fluid.	Nux vomica.
Aromatics.	Valerian.
Cinchona (by some this is regarded as contra-stimulant.)	Coffee.
	Mustard and pepper.
	Cantharides.
	Turpentine.
Red particles of the blood.	Squills.
	Nitre.
Animal food.	Acids and oxygen.

A slight view of this table will convince you that many of the substances put down as contra-stimulant are, unequivocally, stimulant; for example, cantharides, turpentine, &c. In fact, the founders of this doctrine have assembled under the same head, bodies of the most opposite effects; and have separated others, whose general operation is very analogous. They appear to me to have totally disregarded the physiological or immediate, and to have directed their whole attention to the therapeutical or secondary effects, which, as I shall hereafter have to show, are accidental and uncertain. They assume the existence of certain diseases, which are called *sthenic*, because they are produced by too much stimulus, and suppose the existence of *contra stimulants*, because certain agents sometimes relieve this state. In other words, these physicians judge of the nature of a disease by the effect of the curative means, and of the virtues of medicines by the nature of diseases. So that if a disease now supposed to be *sthenic* should hereafter prove to be *asthenic* (that is, produced by too little stimulus), the medicines used would then immediately pass from the class of contra-stimulants to that of stimulants!

The hypothetical notions of the Italian physicians have, however, led to some practical advantages in the treatment of diseases, and taught us that the system will oftentimes bear much larger quantities of medicines than had been supposed. According to their views the doses ought to be proportioned to the degree of excitement; for when inflammatory

action runs high, the patient will bear very large doses of contra-stimulants without any obvious evacuation from the skin, stomach, or bowels, and the disease will be subdued wholly by the contra-stimulant effect upon the fibres, and other solids of the body. This capability of bearing increased doses has been called *tolerance* of medicines, and has led to their exhibition in much larger quantities, and at shorter intervals, than had previously been practised, and which in several cases, (especially that of tartar emetic in peripneumonia) has been attended with great success. Thus, instead of giving one-eighth of a grain to three grains of this antimonial salt, they have administered it to the extent of a scruple.

I now proceed to offer a few remarks *on the circumstances which modify the effects of medicines;* a subject which is of considerable importance, and which would become of great practical use, were we better acquainted with it. Frequently, however, the causes of the modifications observed are enveloped in the deepest obscurity; and in other cases, though we may by experience refer certain results to particular causes, yet we cannot in the least explain the mode of agency of the latter. The following are some of the leading circumstances that influence the operation of medicines:—

1st. *Variations in the doses of medicines* are sometimes attended with variations in the effects produced. Thus, in small repeated doses, oil of turpentine acts as a stimulant or excitant to the general system, and in a large dose, as a purgative.

2nd. *Variations in the mechanical and chemical properties of medicines* also modify their effects. Thus, sulphuric acid in its concentrated form acts as a caustic, but not so when largely diluted. Morphia is more active in solution than in substance.

3rd. *Habit.*— Experience has proved that by repetition metallic substances seldom lose, but, on the contrary, seem rather to gain an increase of power by repetition. Habit or custom will not, I believe, enable a person to increase to any great extent the dose of arsenic or of mercurials; yet the very reverse of this is found to hold good with respect to many medicines obtained from the organic kingdom, especially narcotic and inebriating substances; of the diminished effects of which we observe good examples in opium-eaters and drunkards. Habit enables the Turk to swallow two drachms of solid opium at a dose; the *bon vivant* to take his two or three bottles of wine at a sitting; and the sailor to chew large quantities of tobacco, which in the unaccustomed would be attended with powerful effects. Dr. Chapman tells us that he knew a wine-glassful of laudanum to be given several times in the twenty-four hours. "But what is still more extraordinary," says this author, "in a case of cancer of the uterus which was under the care of two highly respectable physicians (Drs. Monges and La Roche) of Philadelphia, the quantity of laudanum was gradually increased to three pints, besides a considerable quantity of solid opium in the same period." Pinel mentions a lady who required 120 grains of opium to give her ease in cancer of the uterus.

In what way are we to explain these phenomena? Some think that the effect of habit is only an increased power of decomposition, on the same principle that, by repetition, the stomach often acquires an increased facility in digesting substances which had at first resisted its assimilative powers. If this explanation were correct, we ought to observe the effect of habit principally when substances are swallowed, and little, or not at all, when they are applied to a wound, to the cutis vera or other parts not possessing a digestive or decomposing power;—and the common dose of opium ought to have the usual effect, when applied to any part of the body of an opium eater, except the alimentary tube. I am not prepared to say positively whether this would or would not take place, but I suspect the result of the experiment would be a disproval of the hypothesis. The ultimate fact, then, to which we can reduce our inquiries as to the effect of habit, appears to be this, that by repetition, the body (or certain parts of it) becomes less susceptible to the influence of certain organic agents.

We must not pass over a curious fact sometimes observed by the repeated use of certain substances, namely, that effects are obtained different in their nature from those caused by only one or two applications. For example, no one could by observation of the effect of one or even two doses of opium have anticipated that great emaciation, debility, and even premature old age, could be brought on by the long-continued use of large doses of this substance.

4th. *Climate.*—That climate has an important influence over the functions and even physical characters of the body, every one will admit; and we presume that the same agent may influence the action of medicines, by rendering the system more or less susceptible of their influence. It is, however, difficult to select pure and unequivocal instances of this. Calomel has been given in much larger quantities in India than it is customary to exhibit it in Europe, and as the effect was not proportioned to the dose, it has been presumed that the climate of India diminishes the susceptibility of the system to the influence of this agent; an inference,

however, which, although it may be true, can hardly be said to have been proved.

5th. *Diseased conditions of the body.*—Diseases of various kinds sometimes have a remarkable influence in modifying the effects of medicines; and hence it is of considerable importance in practice to be acquainted with these facts. One of the most striking instances is that of the diminished influence of opium in tetanus. A scruple of this substance has been given at one dose, and repeated every two or three hours for several days, without any remarkable effects being produced. To what cause we are to attribute this I know not. The late Mr. Abernethy mentions in his lectures, a patient who had tetanus from a wound which he received at the time of the riots in the year 1780, to whom a scruple of opium was given every day, besides a dose of a drachm at night: when his body was opened, 30 drachms of opium were found undissolved in his stomach. You might perhaps infer from this case that the diminished effect arose from the want of solution of the medicine; and that this was Mr. Abernethy's opinion seems presumable from his advice as to the mode of using it in this disease. "Give it," says he, "repeatedly in small doses, so that it may liquefy." However, that it was not the want of liquefaction or solution seems proved from the fact that laudanum is also less effective in tetanus than in health.

Begin, in his *Traité de Therapeutique*, relates that M. Blaise, in a case of tetanus, administered in ten days, four pounds, seven ounces, and six drachms of laudanum, and six ounces, four drachms, and forty-five grains of solid opium! Begin endeavours to explain these facts by assuming that the stomach acquires an increase of assimilating power, so that it is capable of digesting these enormous quantities of opium, in consequence of which their usual narcotic effects do not take place. He supports this hypothesis by stating, that if, during tetanus, opium be injected into the veins in much smaller quantities, it produces its usual effects. If, however, this latter assertion be correct, it does not at all warrant Begin's assumption; and bearing in mind that opium administered by clysters during tetanus is less powerful than usual, and also taking into consideration the case related by Mr. Abernethy, I think we have evidence sufficient to warrant our non-admission of this hypothesis. All, therefore, apparently that can be said in the way of explanation is, that in tetanus, the nervous system has undergone some change by which its susceptibility to the influence of opium is considerably diminished.

Another example of the influence of disease in modifying the effects of medicines is seen in the diminished power of mercury during fever. I have repeatedly seen large quantities of mercurials exhibited internally, and in some cases accompanied with mercurial frictions, without exciting salivation; and in general such cases terminated fatally. I never saw a case of fever in which salivation was established (and I have seen several) that died; but whether the recovery was the consequence of the mercurial action, or the salivation of the recovery, I will not pretend to say, though I must confess I have formed a favourable opinion of the efficacy of mercury in some forms of fever, especially those complicated with local congestion or inflammation.

6th. *Mental influence.*—The effects of medicinal agents on the body are very much influenced by the state of mind. Hufeland tells us that he knew a lady who, having conceived a violent aversion to clysters, was thrown into convulsions by the injection of a mixture of oil and milk. I have several times seen the most violent effects attributed to bread pills, which pills the patients had been previously told exercised a powerful influence over the system.

7th. *Age.*—Living beings are incessantly undergoing changes; so that at different epochs of their existence their organization and actions are somewhat different. Thus they increase, develope themselves, arrive at maturity, and lastly decrease. We may conveniently admit four ages of man,—infancy, youth, the adult age, and old age,—each of which is marked by particular states of the functions, by a tendency to certain diseases, and by a different susceptibility to the influence of medicines. As a general rule, infants require much smaller doses than youth, and the latter than adults. This is particularly remarkable in narcotics, which require to be exhibited with the greatest caution to infants: while the more active metallic preparations, as the salts of lead, nitrate of silver, arsenic, &c. ought perhaps never to be given to them. However, infants appear to be less (or certainly not more) susceptible to the influence of calomel than adults; constituting here a remarkable exception to the general rule.

Attempts have been made to lay down a general rule for determining the doses of medicines for children, but none is applicable in all cases. Dr. Young says, that for children under twelve years of age, the doses of most medicines must be diminished in the proportion of the age to the age increased by twelve. Thus, at twenty-one, the full dose may be given, but at two years only $\frac{1}{7}$.

$$\frac{2}{2+12}=\frac{1}{7}$$

Hufeland gives the following scheme:— At the end of the first year suppose the dose to be 1; then at the fifth it will be 2; at the fifteenth 3; and at the twenty-fifth 4. Suppose the dose for an adult to be 40 grains, for other ages it will vary thus—

Years.	25	20	15	14	13	12	11	10	9	8	7	6	5	4	3	2	1
Doses.	40	35	30	29	28	27	26	25	24	23	22	21	20	18	16	13	10

Months.	11	10	9	8	7	6	5	4	3	2	1	½
Doses.	9		8		7		6		5	4	2	1

8th. *Sex* has also an influence on the operation of medicines, females being much more susceptible than males.

9th. The *nature of the part* to which the medicine is applied has an important influence over the effect produced. The stomach, for example, is much more susceptible than the skin. Opium acts more powerfully on the serous than on the mucous tissues. Carbonic acid acts as a positive poison when taken into the lungs, but as a grateful stimulant when applied to the stomach.

10th. *Individual peculiarities.*—Under this head we may examine three kinds of peculiarities; 1st, those that affect whole nations or communities, and which constitute the differences between the human races: if I might be allowed to coin a new term, I would call these *racic peculiarities;* 2ndly, those peculiarities called the *temperaments*, which affect certain individuals without reference to the races, and which consist in disproportions in the development or activity of certain organs by which the whole animal economy is influenced; 3rdly, *Idiosyncracies*, or those peculiarities which affect the functions only of organs, and which are not common to a number of individuals.

In the first place let us notice the influence of *peculiarities of race or of nations.* It is not necessary for me to enter into an examination of the peculiarities (structural and functional) of the human races or of individual nations. It is quite sufficient for me that such do exist, and are obvious to the most careless observer. *A priori* there would be little difficulty in admitting that these may be accompanied with modifications of the susceptibility to the influence of exterior agents, such as medicines. As, however, we meet with differences of customs, manners, diet, climate, &c. among different races, or even nations of the same race, so it is difficult to determine what we ought to refer to the influence of these, and what to the peculiarities in organization. Hence we are not in a condition to offer pure and unequivocal instances of the peculiarities of race affecting the operation of medicines.

Differences of *temperament* are said to affect the influence of medicinal effects. But what is temperament? This term is derived from the Latin verb *tempero*, to mix together, or to temper, and is applied to certain conditions of the body formerly supposed to arise from variations in the proportions of the fluids of the body. Thus, when the fluids were in proper relative proportions, they were said to temper each other, and by so doing produce a perfect temperament. When the yellow bile was supposed to be in excess it produced the choleric or bilious temperament; when black bile, the atrabilious or melancholic; when blood, the sanguineous; and lastly, when pituita or phlegm, the pituitous or phlegmatic. Although in modern times physiologists do not admit this theory of the ancients respecting temperaments, yet we cannot help acknowledging that certain individuals offer physical and functional peculiarities of the kind described by the ancients under the above mentioned heads: and thus the existence of temperaments has been generally admitted, while the theory or explanation of them has varied with the prevailing medical doctrines of the day.

The number of temperaments has not been agreed on; Hippocrates admitted four, Boerhaave eight, others five. Under five heads I think we may include the leading varieties, which will then stand as follows:—

1st. The *nervous* temperament, characterized by great susceptibility of the nervous system, and comparative little muscular power.

2d. The *sanguine* temperament, known by great development of the vascular system. The functions are performed with considerable activity, but the strength is soon exhausted.

3d. The *muscular* temperament is characterised by great development of the locomotive organs (bones and muscles); but accompanied by diminished nervous energy.

4th. The *relaxed* temperament, marked by deficiency of power and imperfect performance of all the functions, with a soft and flabby condition of the solids.

5th. The most *perfect* temperament is that in which all the organs and functions are properly balanced, and in which we have the greatest strength.

Each of these temperaments varies in regard to its susceptibility to the influence of medicinal agents. In the sanguine temperament stimulants must be employed

very cautiously: in the nervous and relaxed temperaments, evacuants can only be used with great attention and care.

The effect of *idiosyncracy* (a term derived from ἴδιος *proper*, σὺν *with*, and κρᾶσις *mixture*) in modifying the effects of medicines and poisons is in general to increase their activity. Thus, some individuals are peculiarly susceptible of the action of opium, some of mercury, and others of alcohol. The odour of ipecacuanha will in certain persons produced short and difficult respiration, approaching almost to a paroxysm of asthma. The late Mr. Haden has related a case in which two drachms and a half of tincture of colchicum produced death: the mother of the patient was also exceedingly susceptible of the action of colchicum even in very small doses. There are, however, instances in which the effect of idiosyncracy is to diminish the activity of medicines; some persons, for example, are exceedingly insusceptible of the action of mercury.

A PAPER

ON THE

APPLICATION OF COMPARATIVE ANATOMY TO HUMAN EMBRYOLOGY,

Read before the Junior Physical Society, Guy's Hospital, May 2d, 1835 *.

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Some apology is necessary for introducing a subject which may appear at first sight foreign to the usual discussions in this society; but still more will an apology be necessary for the very imperfect manner in which that interesting subject will be submitted to its notice, and the presumption (I may almost say) on my part, in at all venturing upon so diversified, so intricate, an investigation.

It is scarcely necessary to observe that the objects and meaning of comparative anatomy are the anatomical and physiological investigation of the lower animals, and a comparison of their structures with the same structures in man,—the results of such investigation and comparison forming an unerring foundation for the systematic zoologist, and marks of identity for the inquiring geologist, exposing and unfolding numberless physiological facts to the natural historian, and enriching and perfecting by comparison our knowledge of human anatomy, physiology, and pathology, the ground-work and basis on which the superstructures of medicine and surgery are built up and supported, the links by which they are indissolubly compacted and united. These subjects are, however, so diversified and extensive, that it behoves me to confine myself entirely to a comparative consideration of the essentially important organs and systems (those of generation purposely excepted), and to adhere rigidly to some fixed plan. In accordance, then, with the one which, as possessing the greatest general interest, I have thought it most expedient to pursue, a two-fold object has been attempted. I propose, in taking a review of the whole animal kingdom, to illustrate, as they present themselves, the analogies and progressive phases of development of the human embryo, premising these by a momentary elucidation of systematic technicalities and classification, and a few remarks on the advantages to be derived from the study and pursuit of comparative anatomy. To do justice to these propositions, I must trespass on the time, and entreat the patient hearing, of the society.

Many illustrious names, from the earliest ages, may be ranked amongst the promoters and followers of comparative anatomy; many highly-talented names of recent and present date may be mentioned as its enthusiastic advocates, contributing, by their unwearied and patient investigations, to improve a science which, until lately, has advanced with but reluctant progress. Will it be out of place here to mention the splendid monograph of Mr. Bell on the

* I feel it necessary (to my more immediate friends especially) to explain the motives that have induced me, after so long a delay, to publish this paper, which is word for word the same as that read before the Society in May last. From a variety of circumstances, which it is unnecessary here to notice, I was induced, after reading that paper, to extend it, and alter its form, before publication in the Medical Gazette; but in prosecuting the investigations, and making the necessary dissections for that purpose, time has rapidly passed away, and I have been so led on by the extreme interest and variety of the subject, as to render the mass of facts even now accumulated much too large for insertion in any periodical; and thus I have been induced to abandon the idea of adding to the original paper; with the view, however, of publishing those facts, and the results of my recent observations, in a different form and distinct title of "Researches in Comparative Anatomy and Human Embryology." Under these circumstances, therefore, I have considered it better not in any way to disturb the original arrangement; and will conclude these remarks by requesting the indulgence of my readers in perusing this very imperfect and superficial paper, assuring them, at the same time, of the substantiation of many assertions here made, in the work in which I am at present engaged, together with many new and I trust some important facts collected from actual dissections, in a series of which I am still occupied, and to some of which I shall take the opportunity of referring in this communication.

testudinata, the accurate investigations of Mr. Morgan on the generative system of the kangaroo, and the invaluable lectures of Professor Grant on comparative anatomy and physiology? Sir Charles Bell observes *, "Comparative anatomy is but the means of preparing us for other branches of the profession," and it is in this light, and not as an abstract science, that we, as students of medicine, are to consider and pursue it; and of this we may be assured, that when engaged in these attractive investigations, there will be unfolded to our view many beautiful phenomena, many otherwise hidden truths, and a multitude of facts with which to store and invigorate the mind; to impress it, as Cuvier observes †, with a habit of order and precision, to accustom our powers of observation to vigorous exercise on all occasions, to lay up a rich and solid foundation-store of human anatomy and physiology. How necessary are all these acquirements for us in our professional career! With how many advantages, then, may we pursue that science which I now venture to introduce to the notice of the society. But again: where is the link whereby the fundamental connexion of comparative anatomy with the sciences just mentioned is formed?—it is in its application to human embryology; it is this that constitutes the essence of its import, and the value of its researches to the student of human anatomy.

In endeavouring, however, to demonstrate the facts just enumerated, I regret my necessity to make use of certain terms that to some may not be familiar. With a view, then, to their elucidation, and a hope of increasing the general interest of my subject, I will detail a hasty and general survey of the animal kingdom, referring to the denominations of the leading classes, and the characters by which those denominations are supported. In taking this survey, the most superficial observer would find many general points of interest, many beautiful illustrations of the relation of organization and instinct, many physiological analogies, to arrest his attention; he would be struck with a certain degree of similarity pervading the construction of animals, and the end and aim of their existence; he would see, indeed, that the whole animal kingdom was an extensive chain of beings, every link being perfect, every gradation being strictly, though almost insensibly, apparent: so that from man, "the type of animal perfection," a general and gradual decrease of organization, of perfection, of development, takes place—chasms that formerly existed being now filled up by geological discoveries; breaks that still remain affording matter of stimulating interest to the industrious naturalist and scientific geologist.

Again, on taking only a general survey of the organization and nature of animals, we find four principal forms, four general plans, on which they are constructed. The first form is that where a bony case or shell protects and encloses the brain; a chain of bones proceeding thence protects the spinal marrow; and from these, ribs, varying in number, protect the viscera. Added to this, a mouth, with two jaws, senses distinct, sexes separate, red blood and a muscular heart, characterize a large group of animals, with man at their head, to which the name of Vertebrata has been applied. They are subdivided into *mammalia*, viviparous animals, suckling their young, and *aves*, or *birds*, which are oviparous; both these classes have a heart, with four cavities, and hot red blood. *Reptilia*, which are oviparous, have a heart with only three cavities, and cold blood. These three classes breathe by means of lungs. *Amphibia*, animals undergoing a peculiar metamorphosis, having a heart with only two cavities, and breathing at first by means of gills. Lastly, *pisces*, or *fishes*, undergoing no metamorphosis, having a heart of two cavities, and breathing by means of branchiæ. These classes are subdivided into orders, founded principally on the form and position of the teeth and the structure of the feet. The second form comprises animals with no skeleton, but of a soft muscular texture, in which occasionally are inclosed plates or shells; the digestive apparatus is tolerably complex; two organs of sense only are discoverable: they are called Mollusca. Amongst them we distinguish the *cephalopoda*, so named from the feet surrounding the head, as the common *sepia*. The *pteropoda*, having wing-shaped membranes for swimming, as the *clio borealis*. The *gasteropoda* inhabit univalve shells, and have a sub-abdominal muscular disk, upon which they creep, as the *snail*. The *conchifera* inhabit bivalve shells, and are always fixed to them. The *tunicata* have their internal organs tolerably developed, and are surrounded externally with a soft transparent tunic. Most of these animals are inhabitants of the sea. The third group, familiar to us in the forms of insects and worms, comprises animals with articulated membranes, and breathing by lateral tracheæ; their external covering is hard and horny, or soft and

* Lectures on the Hunterian Preparations, delivered at the College in 1833; published in the Lancet, vol. i. 1833.—Vide Lecture I., p. 285; the concuding paragraph of which contains strong proofs of the advantages to be derived from comparative anatomy.

† Preface to the first edition of his Règne Animal.—See also a letter addressed by Cuvier to Mertrud, in his Leçons d'Anatomie Comparée.

annulated; their jaws move laterally. From their form they are called Articulata, and are divided into *crustacea*, having a solid calcareous covering, compound eyes, and two pair of antennæ. *Arachnida*, or *spiders*, with no antennæ. *Insecta*, a magnificent tribe of animals, the study of which alone has engaged the attention of the most profound talents, the most illustrious characters. I could wish that time would permit me to dwell on the beauty, the infinite variety, the instinct, the external appearance, the internal conformation, of this splendid, this numerous tribe. They are distinguished by undergoing an interesting metamorphosis, having six legs, antennæ and spiracula for respiration; they are found in every corner of the globe. The *myriapoda* are characterized by a number of feet, as the *centipede*. The *annelida* are surrounded by rings, as the *leech*. The *cirrhopoda* inhabit multivalve shells, and have feet of a curled form, as the common *barnacle*. The minute *rotifera* are distinguished by their circles of ciliæ and wheel-like form; and the *entozoa* are animals found in the interior of the body, and familiar to us under the forms of hydatids and intestinal worms. The fourth and last form comprises animals whose organs of sense and motion are arranged in a radiated manner: they are called Radiata. Amongst them are the *echinodermata*, animals having a spiny skin, and a complex internal organization, as the *echinus*, or *sea-egg*. The *acalepha*, which are of a gelatinous form, and excite inflammation of the skin when touched, as the various forms of *medusæ*. The *polypiphera*, or *zoophytes*, have digestive cavities in a soft fleshy substance, and a calcareous concretion either externally or internally; they have a great analogy to, and some were indeed formerly figured as, plants. The *porifera*, or *sponges*, have an external porous surface, leading to internal ramifying canals. And lastly, the *polygastrica*, minute microscopic animals, having an amazing number of stomachs. Many of these classes are inhabitants of the sea.

Having taken this hasty survey, I pass more immediately to the object of my paper. The comparative development and perfection of the organs of support and of digestion, the nervous, circulatory, and respiratory systems, will come successively under consideration; and I set out with presupposing a complete anatomical knowledge of these parts in their perfect and complicated state. I suppose also that we are familiar with the varied external appearance and configuration of the human embryo, according to its age, and the peculiarities of the full-grown perfect fœtus.

It is a singular but beautiful physiological fact, that organic matter generally should be developed and called into being, should obey the same laws and rules of development, should become more complex and perfect in the same gradual manner, in which man individually, from the first germ of his existence to his perfect form, exhibits;—that the animal kingdom, from its minutest microscopic point of vital endowment, should pursue an analogous perfection of development of different organs, should exhibit the same systems, mutually dependent or rising into or out of each other, that are called into operation in rapid succession during the short nine months of human embryotic and fœtal existence;—that we ourselves, in embryo, should lead a similar aquatic life to many of these animals. I repeat it,—it is a stupendously beautiful fact of analogy, that in every living animal, from the lowest zoophyte to the highest quadruped, a certain degree of organization, or development of every structure, whether taken individually or collectively, is found, that will correspond with the development and temporary perfection of the same organ, or organs, in man, in all his different stages, from the embryo upwards. Thus, when we reflect upon our own complex organization, we are struck with the fact, that in the development of our organs, the evolution of our systems, the formation of our structures and tissues, we demonstrate a rapid and magnificent succession of objects illustrative of the whole animal kingdom. To prove this will now be my humble endeavour.

The impregnated human ovum, when first detected, consists of a thin membrane inclosing a quantity of transparent limpid fluid. The time at which it enters the uterine cavity is variously stated by various authors, some mentioning eight days*, others sixteen†, others twenty‡. The embryo is first visible in about three weeks, but according to Meckel in five days §. Now the lowest speck of animal life with which we are acquainted, the *monas termo*, one of the Polygastrica, seems to furnish us with a similarity in form and structure to the human ovum, it also being in the form of a minute vesicle, consisting of a delicate membrane inclosing a transparent fluid. Thus, then, in the very onset, the two extremes of animal organization present a similarity in external configuration and internal structure ‖. These Polygas-

* Home, Phil. Trans. for 1817, Part 2.

† Haller, Velpeau's Embryologie.

‡ Dr. Ramsbotham's Lectures on Midwifery, Med. Gaz.

§ Dr. Blundell's Midwifery, by Castles.

‖ Concerning the interesting subjects mentioned so cursorily in this paragraph, but which time then prevented me from further noticing, I have collected some valuable information from the most eminent authors; and as regards the

trica are minute animals of a soft texture, inhabiting fluids of various kinds: this corresponds with the gelatinous aquatic condition of the very early human embryo, when no trace of earthy matter is perceptible, and when imbedded and surrounded by the water of the amnion. Their substance is made up, as it were, of an infinite number of appendices, cavities, or stomachs, the structure of which the recent investigations of Professor Ehrenberg have more clearly demonstrated. Some of them have only one aperture for the reception of food and excretion; others have an oral and an anal aperture.

The organs of support in the Porifera, or sponges, present us with a pleasing analogy; they consist of minute spiculæ of various forms imbedded in a texture of some firmness, and composed either of silica, of carbonate of lime, or of a peculiar horny matter. In the human embryo we find nuclei of ossific matter deposited in various parts, in a similar manner, and each point of calcareous matter following a uniform and constant methodical arrangement of deposition. Again, in the very early human embryo, no internal cavity I believe exists; and this is the case with these and the preceding class of animals in their early and gemmule state; but as the former proceed in development, minute openings and internal ramifying canals are formed, through which currents of water are constantly circulating. The same takes place in the latter class, only that the ramifying vessels are more superficial, and the currents of water are excited by the rapid vibration of ciliæ placed on the surface of the animal; thus, then, is nourishment afforded by the animalculæ which the water contains, are oxygenation and renovation of the vital properties effected, by the air which is in this simple manner circulated. This uncomplicated state of things will at once bring us to the earliest embryo condition of the human body, where there are found only similar anastomosing ramifying vessels (derived from a simple one proceeding from the amnion), simply circulating fluids, without any contraction or peculiar action of the vessels themselves, or without any development of a central cavity. No nervous filaments have as yet been detected in either of these classes of animals, though they certainly appear to be under the well-directed guidance of nervous sensibility; nervous globules, however, are everywhere diffused through the cellular tissue of their body*. Precisely the same occurs, I presume, in the earliest condition of the human embryo.

Proceeding with the Polypiphera, the next class: in some the skeleton consists of a horny substance, situated either external or internal to the secreting fleshy part, which is itself permeated by calcareous spiculæ, reminding us again of the formation of bone in the early human embryo, and especially the white calcareous points in the young cranium. In others again, as in the *corals*, the skeleton is composed of an earthy substance—either silica, carbonate, or phosphate of lime.

The digestive apparatus presents us with a greater complexity of development, and diversity of form, than in the preceding classes. Some, however, as the *hydra*, or common fresh-water polypus, have only the simple form in them described; others have the mouth prolonged, or developed into a number of tentacula; and the ciliæ with which they are fringed having a vibratile motion, create a current of water, and thus attract such a prey of animalcules within its vortex as is necessary for food and subsistence, and for the proper aeration of the cellular texture of their body. Some again, as the *flustra*, have a distinct cæcum appended to the alimentary canal, this forming the first rudiment, the first approach, to a chylopoietic gland; for Dr. Grant supposes that this cæcum may be the origin of the liver†. Now this gland I have found well developed in the early fœtus of the *mus musculus* (common mouse), *erinaceus europæus* (hedgehog), and *felis catus* (domestic cat), of immense size in the human fœtus at the fifth month, the tenth and seventh week; and as most physiologists consider it to be in some way or other instrumental in the development of the fœtal body, we may infer that it is of exceedingly early formation: allowing this, therefore, and judging from analogy, may not its rudimentary condition and primary development be even the same as in these Polypiphera? Again, the forms of stomach in the classes of animals just related have an analogy to what is considered the embryo development of the human stomach, which probably is at first little else than a fold inwards of the external tunic; or, reasoning from Andral's descrip-

primary form of organized matter, the analogies of its development, and the manner in which the progressive advances of this latter phenomenon take place, according to geometrical principles, I hope to bring forward some of the highly important, and to this country somewhat novel, opinions advanced by Carus in his Anatomie Philosophique, a work abounding in elaborate and scientific research.

* On this subject Dr. Roget, in his Bridgewater Treatise, makes some valuable remarks.

† Lectures on Comparative Anatomy and Animal Physiology. In this first quotation from Dr. Grant's Lectures, I will take the opportunity of stating how much valuable assistance I have derived from their perusal and study; in fact, they have been my guide throughout.

tion *, a pouch formed by the continuation inwards of the vesicula umbilicalis; and this pouch or cavity, equally in the early human embryo as in these low zoophytes, may be considered as the only well developed organ; but, in the more complicated forms of the Polypi just described, we find the alimentary canal composed of a distinct membranous tube, open at both extremities; so, in a little more advanced state of the human embryo, do we find a similar tubular cavity existing. Andral observes †, " the intestinal canal is one of the first organs of which any trace is perceptible; at first it is only a continuation of the vesicula umbilicalis, which gradually elongates itself into two tubes,—one inferior, constituting the large intestine; the other superior, forming the small intestine and stomach." Others, however ‡, assign a different situation (the cæcum, for instance) to the origin of the intestinal canal §. Sanguiferous vessels, and circulatory currents of fluids and globules, have been observed in many of these Polypi; but their nature does not seem well determined. Nervous globules are abundantly distributed, and nervous filaments are supposed here to exist, but only by analogy.

Proceeding with the Echinodermata, some are covered with a dense integument, of a leathery consistence, and have a power of forming for themselves adventitious coverings of shells or sand. Others, as the *asterias*, or star-fish, have the rays, or framework of their body, composed of small osseous pieces; others, as the *echinus*, have the body invested by a shell, or calcareous crust, armed with spines, and a mouth with five teeth. The genera of this class present numerous forms and transitions of the digestive apparatus, and in them its increased complexity is well demonstrated. In the star-fish there are radiating ramifying canals, or cæca, extending into each division of the body, terminating in little sacs, and having their origin in a central stomach, which alimentary canal, again, has but one opening for the reception of food and the discharge of excrement, though in the higher classes it terminates by a distinct anal orifice. In the common *echinus esculentus* we find an œsophagus, developing a stomach, and an intestinal canal having no ramifying cæca, but supported by a mesenteric membrane, and terminating in a cloaca, or general cavity for reception. Here an analogy may be noticed: the cloaca (according to Andral) exists in the human fœtus a few months before birth; and I have myself observed it at the fifth month, and tenth week. Again, in the early human embryo, at first but one opening to the alimentary canal can be supposed to exist, and that the tubular communication with the vesicula umbilicalis, which, according to Velpeau, contains a nutritive fluid contributing to the development of the embryo; but as this development progresses this communication ceases, the alimentary canal becomes more extended and perfected, though straight, and, it is scarcely necessary to observe, oral and anal orifices are formed. In examining a fœtus of about nine weeks, I distinctly traced a cord, independent of the intestine, running from the vesicula umbilicalis to the under part of the stomach.

As regards the nervous system, the entrance of the alimentary canal is surrounded by a circular nervous cord, giving off filaments in various directions, each of which has its origin marked, as in the Acalepha, by a minute ganglionic enlargement. In the mesenteric membrane before noticed is a distinct vascular system, comprised of numerous ramifying minute capillaries; these anastomosing form a larger circular trunk, which surrounds the stomach; in some this trunk is longitudinal, which then gives off lateral branches. The same occurs in the human embryo: the ramifying vessels before described, by anastomosing, enlarge into a trunk, a simple canal, which gradually dilating, is the first rudiment of the heart.

The respiratory organs are of a tolerably complex structure; for, in the Holothuria, the water for the aeration of the blood enters by the cloaca, or anal orifice, passes through two large vessels, and then circulates through a number of smaller ramifying capillaries over the whole of the body; these may be considered as internal branchiæ, and are apart and separate from the vessels for circulating the blood.

Thus, then, in the Radiata, the lowest group of animals, many pleasing analo-

* Treatise on Pathological Anatomy, by G. Andral.

† Ibid.

‡ Oken and Geoffroy St. Hilaire.

§ The primary evolution and gradual perfection of development of the alimentary canal is a subject of the highest possible interest: and the structural changes that take place during its passage from a simple tube to a highly-complicated state of organization and formation, are well demonstrated in the metamorphosis of the Amphibia. This fact I was led to notice from repeated investigation; and having at length demonstrated these changes by dissection, I shall have some very interesting results to communicate on this head in my little work. These remarks will equally apply to the nervous, circulatory, and respiratory systems; and, indeed, I am quite sure, that insignificant as the Amphibia might appear to be, their internal anatomy is of the first importance, and that the degraded frog, if minutely investigated from its first escape from the ovum, would tend to throw great light upon a subject which, in this country, has so little engaged the attention of scientific individuals.

gies have presented themselves, and the rudiments of many important structures have already been demonstrated. The similar commencement of ossific deposit, and the analogies of the digestive cavities, are interesting; the early development of the vascular system, highly important, inasmuch as it governs the formation of other structures, and immediately regulates the growth of the whole nervous system, the varied parts of which we shall presently find developed in a corresponding ratio with the particular distribution of the arterial system *.

We now pass to the Articulata, the first class of which comprises animals whose habit and more immediate connexion with some of the diseases of the human body claim for us almost a double share of attention. I mean the Entozoa; under which class are arranged the different forms of hydatids and intestinal worms, parasitic inhabitants of the bodies of other animals. Some of the *ascaris* are provided with a dense external covering, transversely striated or articulated; others have sharp-pointed spines; others, called *epizoa*, have a skin much more dense, articulations more perfect, and a rudimentary development of legs, antennæ, and maxillæ. In the *hydra*, a species of hydatid, we find a simple digestive cavity, with but one aperture, capable of distention, when it has the appearance of a thin, white, transparent bag. In the *tænia* there are two communicating alimentary canals. In the *ascaris* I found the alimentary tube passing straight from mouth to anus; dilated in places, and in the middle third completely imbedded and surrounded by a leash of filiform tubes†. All the species have sharp spines, or rudimentary teeth, and many of them are well known to infest the human subject. A minute Nematoid Entozoon, inhabiting human muscle, has been recently described by Mr. Owen ‡; it first occurred in this hospital, and was then introduced to the notice of the profession by Mr. Hilton §. The formation and origin of tubercles have been by some imputed to a species of hydatid ‖. The nervous system in these Entozoa shews itself in the form of a fine, white, opaque streak, consisting of a double nervous filament without ganglia, extending the whole length of the body, and encircling the œsophagus. This is highly interesting, from the similar rudimentary condition of the nervous system in the human embryo; where, as in all other animals (the sympathetic nerves being first demonstrable), the spinal cord is formed before the brain. The first visible rudiments of a brain and spinal marrow appear in the fœtus (according to Tiedemann *) between the fifth and sixth week, in the form of a whitish vesicular fluid, contained in a membranous canal or tube in the trunk, leading to or forming a cavity or pouch in the head. At the seventh week the spinal cord (in consequence of the early development of the intercostal arteries) is large and thick, the rudiments of the two lateral columns of which are manifested in the form of a simple, pulpy, white streak — a nervous streak, according to Dr. Grant, precisely similar to what has just been described in the Entozoa; a groove in the posterior part marks the penetration of the pia mater; its structure is minutely globular†. The pre-existence of a nervous system in the form of globules has been before hinted at; these globules increasing and uniting form sympathetic ganglia ‡. Arteries and veins are distinctly traced, ramifying through the body of these parasitic animals, and, in the higher forms, a distinct pulsating cavity or vessel is demonstrable. In this way is a colourless nutritive fluid circulated, and this again oxygenated by the cutaneous transpiration of the fluids of the animals, in which they live. Here, again, may an analogy, a similar progress of development, be traced. We have seen that at first, numberless ramifying inactive vessels, without any central enlargement, circulated the vital fluids equally in

* This description of the Radiata is, it must be confessed, excessively deficient in originality, from my not having been able to procure at that time any marine animals; since then, having had the opportunity of collecting on the sea coast, I have been able to verify many of these statements by dissection, the results of which will be fully detailed subsequently.

† Subsequent dissection has, however, proved to me that there was only one long convoluted tube, the ovary of these animals.

‡ In a paper read before the Zoological Society. See Med. Gazette for April 25th, 1835.

§ In a paper read before the Medico-Chirurgical Society. See Med. Gazette for Feb. 2, 1833.

‖ Having mislaid my notes on this subject, I am unable to give the direct quotation; but the paper appeared in a number of the Lancet not further back than two years ago. The author was a Frenchman.

* Anatomy of the Fœtal Brain; a most elaborate work, with exquisite plates from nature.

† Though I have dissected fœtuses of an early age, I have not yet been able to demonstrate any portion of the nervous system; and, indeed, not having had the opportunity of immersing them in alcohol when recent, this could hardly be expected. I have now, however, some very early fœtuses of the squirrel, mouse, sheep, &c. hardening in alcohol, in which I hope to be more successful. Should this fail, the artificial incubation of the egg will probably afford some opportunity for dissecting the spinal cord in its earliest state, the development of which is undoubtedly the same in birds as in the human species.

‡ The manner in which the globules both of the nervous and vascular systems arrange themselves, according to geometrical principles, to form a pervious or impervious tube, is beautifully detailed by Carus, in his Anatomie Philosophique.

the early human embryo as in the lower radiata, and progressing simultaneously, an inactive dorsal vessel was formed. Now in these higher Entozoa, a central pulsating vessel has been demonstrated; so in the human embryo, at the fourth week, is a pulsation perceptible which is known to be the beating of the heart *.

Passing over the Rotifera and Cirrhopoda, from want of time, and as not illustrating much progressive development of the human embryo†, we come to the Annelida, or red-blooded worms; some of which, as organs of support, have the power of laying extraneous layers of shells or sand, others of exuding from their body a calcareous secretion, forming a tube of various shapes; others have hard spines, of a horny consistence. The digestive apparatus, in some, consists of a simple, partly dilated canal, with two openings; others, as the leech, have three jaws set with sharp teeth, and a membranous stomach divided into large communicating cells. In the *lumbricus terrestris* I found the intestinal tube presenting a sacculated form. The nervous system consists of a double longitudinal cord, united in two or more ganglia; the development of which, and the distribution of the filaments, depending on the complexity of organization and structure. These are the only invertebrate animals that have red blood; it contains, however (according to Dr. Grant), but little fibrin, and circulates in a double system of vessels—viz. a dorsal arterial trunk and two returning abdominal veins. Here we find, apparently for the first time clearly demonstrable, but still existing more or less in all the articulata, distinct venous trunks for the return of blood; the same, we may presume, takes place in progress of development in the human fœtus, and demonstrable, we may conceive, in the rudimentary vena cava, which I have myself found in the fœtus of nine weeks. The organs of respiration in some of these worms present branchiæ resembling tufts, attached either to the anterior or middle and back part of the body; others respire by means of air-sacs opening from the side, as in the leech.

Having spoken of the helminthoid, I now pass on to the Entomoid Articulata; noticing the Insecta, Myriapoda, Arachnida, and Crustacea, conjunctively. These all possess articulated members, the rudiments of which were perceptible in the preceding class: this may be considered as analogous to the primary bud-like development of the extremities of the human embryo, and their subsequent increased protrusion and elongation. In a fœtus of eight weeks I found the anterior extremities 1-12th of an inch long, the posterior appearing only like hemispherical tubercles; at ten weeks they both measured 5-8ths of an inch in length. The bodies of these entomoid animals are protected and inclosed in a hard, elastic, solid case, composed of a peculiar principle called *chitine* by the French, and either the carbonate or phosphate of lime, or both: in the Insecta there is scarcely a trace of carbonate of lime, while in the larger Crustacea it is very abundant. This skeleton or case is exuviable, to allow of the animal's growth; it is composed of a certain number of articulated rings, or segments, the rudiments of a perfectly developed vertebral column in the higher classes, the analogue of its development in the human embryo, where we can conceive the tubular canal before spoken of to have become more consolidated by cartilaginous and slight bony deposit. The digestive apparatus varies much in form, being tolerably simple in the Myriapoda, but in the Insecta we find a pharynx, œsophagus, crop, a muscular toothed gizzard, a stomach, and complicated intestines. In the dissection of some neuropterous larvæ, I observed these parts tolerably distinct; the intestinal canal was short and nearly straight, the gizzard and true stomach were supplied with a beautiful plexus of nervous filaments *. Most of the assistant chylopoietic glandular organs were present—viz. liver, pancreas; and these forming a beautiful rudimentary illustration of the same organs in the higher classes, and it is reasonable to suppose also in the human embryo. The form and structure of the mouth are very various; it is armed with huge-toothed maxillæ in some †, furnished with a delicate perforate proboscis in others ‡, and set with little laminæ, in the form of setæ, in those that live by suction §. The Arachnida and Crustacea being animals of a true carnivorous nature, have the alimentary canal short and straight; in the common edible crab I found the œsophagus and intestinal canal very short, the latter curved forwards, and the stomach curved with teeth. The nervous system in most of the true Insecta, Cuvier describes ‖ as consisting of

* Physiological Observations in Lizars's Anatomy and Cyclopædia Americana, article *Embryo*.

† These classes will be fully treated of in my subsequent investigations.

* Larvæ of *libellula depressa, agrion*, &c. These are carnivorous feeders. I have more recently had the opportunity of dissecting some vegetable-feeding insects (*melolontha, cetonia*, &c.), where the construction of the alimentary canal, biliary vessels, &c. was quite different.

† *Cicindela, carabus, libellula*, &c.

‡ *Papilio, sphinx, phalæna*, &c.

§ *Cimex*, &c.

‖ Règne Animal, translated by Griffiths.

a brain placed on the œsophagus (formed by two ganglions united at the base), giving off eight pair of nerves and two single ones, and of twelve ganglia, all inferior *. Here, then, we find a supra-œsophageal nervous accumulation; in fact, an imperfect brain demonstrable, and in the hymenopterous insects its division into hemispheres becomes first apparent. This we know to correspond with the progressive development and situation of the fœtal brain; the globular fluid in the cranial cavity before described, becomes similarly condensed to that in the spinal column; in the course of time (according to Tiedemann) two membranous pouches, the terminations of the pyramidal cords, form the rudiments of the two hemispheres, the various convolutions and cavities becoming subsequently demonstrable. In the Arachnida the ganglia are fewer, the nervous system is more concentrated, and holds, as it were, an intermediate condition of development between the Insecta and the Crustacea, in which latter class it presents itself in the form of simple abdominal filaments, which, increasing in development (with thoracic ganglia) coalesce into one great nervous accumulation in the centre of the body. The vascular apparatus is upon the same plan as in the other articulata, only that the dorsal vessel in the Insecta is divided into eight chambers, each having a contractile power, separated from each other by two converging valvulæ, which prevent the blood from retrograding and communicating by fissures with the abdominal cavity; the ultimate distribution and circulation of the vital fluid do not appear well understood, as no ramifying capillary vessels can be traced †. These are, however, better marked in the arachnida, where, indeed, the whole sanguiferous system is more highly developed. In the lower Crustacea, the head is only a lengthened dorsal vessel, but in the higher ones it consists of a fleshy ventricle placed on the back; from this the blood is transmitted to the different parts of the body; whence it is sent to the branchiæ, and then back again to the heart. Let us now turn to the vascular system of the human embryo: we have already traced its origin and progressive development up to the formation of a central and pulsating cavity; still progressing, we now find that a distinct muscular cavity is formed (analogous to that in the Crustacea), to be afterwards divided and subdivided to form the complex perfect heart of the hot-blooded Vertebrata. The respiratory organs in Myriapoda, Insecta, and Arachnida, consist of tracheæ or elastic vessels, which, receiving air through stigmata pierced in their sides, distribute it by infinite ramifications to every part of the body. In the Neuropterous larvæ before mentioned, I found this beautifully illustrated; two dorsal tracheæ, united by a transverse one, gave off in the thorax lateral tracheæ, opening externally by stigmata, and terminated in the abdomen by a complete leash of air-tubes. Some of the higher Arachnida respire by true air-sacs, and in the Crustacea there are branchiæ variously situated and protected, receiving air from openings in the skin. Rapid strides of advancement in the evolution and increased development of the various organs and systems have been made in these Articulata. A perfect external skeleton has been formed, the digestive apparatus has become complicated, a nervous system well established, and the vascular and respiratory systems more determined and concentrated. It may be interesting also to state that the first appearance of tubercular deposit in the lower animals, takes place in the larvæ of some Insecta *, and that a minute *acarus* has been lately found to exist in the vesicle of the itch †.

[To be continued.]

* All these parts, I am glad to say, I am now able to shew, by dissection, in the larva of *dyticus marginali*, of a species of *arctia* (Leach), &c. I am now working upon this beautiful tribe of animals, and have yet several interesting species to examine.

† In recently reading Carus, however, I find that he has discovered the circulation of the blood in the larvæ of some Neuroptera (*agrion puella*), &c.; which important fact he made known at the meeting of German naturalists at Dresden, in 1826. See, on this subject, his memoir Entdeckung eines einfachen, &c. Leipzic, 1827; his memoir in the Isis, 1828, p. 477; and his Traité Elémentaire d'Anatomie Comparée, translated by M. Jourdan, vol. ii. p. 319.

* I cannot recollect the direct quotation, and I have searched through various books for the original statement of the fact, but without success. I have a faint idea that it was in the larva of the *pontia brassica*, but am not at all certain.

† See, on this subject, the Medical Gazette for October 4, 1834, and the Medico-Chirurgical Review for January 1835. For the original description, see the Mémoire Comparatif sur l'Histoire Naturelle de l'Insecte de la Gale; by F. V. Raspail: Paris, 8vo.

OBSERVATIONS

ON A

FATAL CASE OF CONFLUENT SMALL-POX,

OCCURRING IN THE ADULT.

To the Editor of the Medical Gazette.

Sir,

Should the following observations upon a case of small-pox taken from my case-book, appear worthy a place in your valuable journal, from having a

tendency to elicit information from persons more competent than myself, their insertion will oblige, sir,

Your humble obedient servant,

SAM. J. JEAFFRESON,
M.B. Cantab.

Arundel St. Strand,
Oct. 7th, 1835.

I was called upon on Wednesday, Sept. 2d, to visit W. K., (a previously healthy individual of about 28 years of age) whom I found to be labouring under small-pox. The eruption first made its appearance on the evening before my visit, having been preceded by barely eight-and-forty hours of premonitory fever, not of a very severe kind. The eruption became confluent upon the face and extremities; the earlier part of his complaint was attended by no very extraordinary symptoms; there was, however, for some days a discharge of blood by stool; but as this was kept within due bounds, I did not think it desirable to check it by any very active means. About the 8th or 9th day he began to flag; his tongue being brown and his pulse feeble; with delirium. Small quantities of port wine and beef-tea were now given him, and he was ordered ʒiss. doses of Huxham's Tincture in an effervescing saline draught every six hours, with an opiate at night.

Under this plan he was going on remarkably well, when on the 14th (the 13th day of eruption) I was sent for to see him in the morning, his friends considering him worse. I did not, however, see him till noon; he had then become much more comfortable: it was ascertained that he had not had a very good night's rest. His tongue was now tolerably clean and moist; his pulse 110, of sufficient strength; bowels and urine natural; skin warm; wandering *slightly* in his mind, but retaining a perfect recollection of me, and consciousness of what was going on around him. The only symptom upon which I looked with any degree of suspicion, was that the pustules on the arms and legs, (which had run a good deal into each other in places, and so had become very extensive) were remarkably and suddenly fallen in. He was ordered to continue his bark, wine, and beef-tea. I saw him again in the afternoon, about half-past four: he was still going on well, and his pulse remained so steady that I requested them not to give him more wine unless he appeared to get lower, till my visit in the morning; but a powder at bedtime, consisting of 7 grs. of Dover's powder, and 1 gr. calomel.

An intelligent medical friend who saw the patient this morning considered him to be going on so well as to be nearly out of danger.

15th. Half-past 9 A.M.—The patient died at about 8 this morning. His friends stated that after I left him last evening he became more delirious; this, however, subsided in a short time after taking the powder; he then became calm, and expressed his conviction of his approaching fate; he afterwards "got a *comfortable* night's rest;" awoke at about 6 o'clock, and "went off without a struggle."

REMARKS.—What, it may be asked, was the cause of death? I regret that circumstances would not allow of my making a post-mortem examination, which might have thrown some light upon this subject; but I think it must be allowed that the symptoms during life did not indicate sufficient lesion of any one organ to have caused the fatal termination of this case. Delirium, it is true, had been present for some days, but it had not been of a very severe character, and was rather on the decrease than the increase; and, indeed, was absent for some time previous to his death. Would this have been the case if congestion or other cerebral lesion had been the cause of death? It cannot be supposed that active mischief within the thorax or abdomen could have been present to a sufficient extent to have proved fatal, without having given rise to some symptoms during life.

Be it remembered that he died on the 14th day of the eruption of confluent small-pox. May not exhaustion alone have been sufficient to destroy life? It may be said that this does not appear probable from the report of the day before; but it should be borne in mind that that report was made after he had taken stimuli, and that these stimulants had not been used from 4 o'clock of the previous afternoon up to the hour of his death. This, then, would give rise to the question, might the administration of stimulants on the morning of the 15th have prolonged life? Possibly they might.

But I incline to think that the fatal termination of this case was owing to other circumstances; circumstances

which would probably have defied the scrutiny of the morbid anatomist's scalpel—an instrument which, though it has greatly advanced medical science, too often, I fear, aims at laying down the exact bounds of structural lesion which are consistent with the preservation of life. It will be remembered that the only symptom which I considered unfavourable on the 14th was the sudden falling in of the large pustules on the extremities; a symptom which Dr. Gregory, the accuracy of whose observation is only equalled by the extent of his experience, considers highly unfavourable. Within twenty-four hours, then, of this circumstance the patient dies. Now it is a fact that the variolous pustule contains a poisonous matter capable of communicating the disease by inoculation; and it is a reasonable conclusion that if these pustules suddenly fall in, (their containing cuticle remaining entire) the greater part of their contents must have been reabsorbed into the system. Considering, therefore, the severe effects which succeed the introduction of the variolous poison (whether by infection, contact, or inoculation) into the system of a previously healthy and strong individual, can it be wondered that the sudden absorption of so large a quantity of this virus into the system of one worn down by sixteen days' severe illness, should itself be sufficient to cause death?

Supposing, therefore, the fatal termination in this case to have been owing to such a cause, it becomes a question of much interest, how similar cases (for it does not follow that all cases are similar—that is, owe their fatality to this cause) may be treated with the most reasonable hopes of success.

I think the objects to be attained may be considered under three views:—viz.

1st. To prevent the reabsorption of any, or at least of a great portion, of the noxious matter.

2dly. If absorbed, to enable the constitution to bear up against its baneful effects; or,

3dly. To administer (if any such exist) some principle which shall have the power of destroying, or at least diminishing, the injurious tendency of the variolous poison.

1st. Might not the first object be in part obtained (in instances like the present, when the pustules run much into each other) by puncturing them, and thus letting out their contents? This plan has, I believe, been adopted on a small scale on the face, not with the object now mentioned, but for the purpose of preventing the scars.

2d. This object will be obtained by generally supporting the system, and needs no further observation here.

3d. Whether any substance with which we are at present acquainted has the specific power of destroying or diminishing the virulence of the variolous poison, I will not upon my own experience pretend to say. This power has, indeed, been attributed to acids and to mercury; the latter of which, when rubbed up with the small-pox virus, has been shown to destroy its power of communicating the disease. If, therefore, a specific be found whose administration is safe, it would appear to be a particularly desirable remedy in such cases where the principal danger is threatened rather from the malignant nature of the disease than from any structural disorganization to which it may give rise. This idea is followed up further on.

Mrs. K.'s baby, 6 months old, was ordered to be vaccinated immediately on my first seeing her husband; this was, however, by some accident delayed until the following day (the 5th of fever, and 3d of eruption) The vaccination succeeded perfectly well; and although the child was exposed night and day to the influence of the infectious atmosphere, both before and after vaccination, yet it escaped without any illness whatever. This circumstance (and similar ones are not uncommon) leads me to reflect that possibly the introduction of the vaccine virus into the system at an early period of small-pox, might have the effect of modifying this disease. I am not aware that this plan has ever been tried, nor have I had opportunities of trying it myself; but I cannot see any reason why the experiment should not be made with safety. Since writing the above I have met with two cases where variola and cow-pox ran their course simultaneously in the same individuals, which, at all events, would lead one to suppose that this experiment is practicable. One of these cases is recorded in the Lancet by Mr. Macnee, in which confluent small-pox and cow-pox simultaneously ran their course in a child of 18 months old: the case proved fatal,

but the fatal termination does not appear to have been in any degree owing to the cow-pox. The other case is recorded in the Medical Gazette for June 13, 1835, by Mr. Grantham: it is one of great interest; and in this case I may remark that the cow-pox appeared to exert a decidedly beneficial influence over the variolous disease, the case terminating favourably.

REMARKABLE CASE

OF

PUNCTURED WOUND OF THE HEART,

FOLLOWED BY RECOVERY.

To the Editor of the Medical Gazette.

Sir,

I beg to offer you the following extraordinary and interesting case, as extracted from the Transactions of the Medical Society of Trinidad. It was read there last year by my friend Dr. Lynch O'Connor, a gentleman for whose accuracy, zeal, and medical ability, I can fully vouch, as, when in the army, he served along with me in the Windward Islands during the years 1815, 1816, and 1817, and was then distinguished by his zeal in the pursuit of professional knowledge. There are other papers from his pen, on yellow fever and similar subjects, but these, from their length, would, I conclude, prove unsuitable to the pages of the Medical Gazette.—I am, sir,

Your obedient servant,

Wm. Fergusson, M.D.

Inspector Gen. of Hospitals, H. P.

Windsor, Oct. 10, 1835.

Mr. J. H., a graduate of a British University, aged 30, was visited by me 20th April, 1821, under the following circumstances. He had lost his passage to Europe and that of his family (which was paid for by subscription), through his own imprudence. I found him in a sitting posture, perspiring profusely, with hurried respiration, and all the symptoms of acute inflammation of the heart or its membranes. I bled him in a full stream to sixty ounces, when deliquium coming on, he was able to lie down. The anxious countenance was nearly removed, and the voice, which before was scarcely audible, became more distinct. I remained with him for about forty minutes, when the previous symptoms returned with unusual violence; so much so, that I recommended his attendants to send for his friends, as I was apprehensive of immediate dissolution, and insisted on Dr. Williams being called in, as well as any other medical man he might wish. He obstinately opposed any further advice, in so earnest a manner, that I began to suspect he had some secret to conceal, and made so earnest an appeal to him regarding his orphan children, that he burst into tears, and stated that he had introduced a darning needle for the purpose of self-destruction, as he believed his object might thus be effected without detection, or disgrace to his profession and family; that he took half an ounce of laudanum previously, from the effect of which he slept eight hours, and after that the pain gradually increased to its present state.

Under these circumstances I sent for Dr. Williams, who said there was evidently carditis, but would not credit the introduction of the needle after he had carefully examined the part, which was situated between the fifth and sixth ribs. However, it was determined to make an incision down on the part, in conformity with the patient's desire, when the needle, three inches and a half in length, was found on a line with the external intercostal muscles. I attempted to secure it with the dressing forceps, but from the motion attending hurried respiration I could not succeed. Under these circumstances I sent for a watchmaker's pliers, with which I succeeded in extracting it. The moment the needle was extracted all the symptoms were gradually relieved, and in an hour after he merely complained of the incision. Purgative medicines were administered, and he was put upon low diet. In five days he was discharged from my care, and returned to Europe, where he arrived in perfect health, and lived for upwards of ten years.

(Signed) Jas. Lynch O'Connor, M.D.

ON FRACTURE OF THE PATELLA;

WITH A

DESCRIPTION OF AN INSTRUMENT PROPER FOR ITS TREATMENT.

To the Editor of the Medical Gazette.

SIR,

I SEND you the description of an instrument for treating fractures of the patella, by which the direct apposition of the two portions of bone can always be obtained, and the length of ligamentous union prevented, which is so often found to exist after this kind of injury, where the ordinary treatment is employed. If you can insert it in your valuable journal, I shall feel obliged.

I remain, sir,
Your obedient servant,
EDWARD F. LONSDALE.

8, Berners-Street,
Oct. 9, 1835.

The difficulty in treating fractures of the patella, by the ordinary method, depends upon the great degree of constriction it is necessary to make round the whole joint before a fixed point can be gained for the bandages to act upon, in order to bring the two portions of bone into contact; the consequence of which is, that the joint is unable to bear the pressure, and becomes swollen and painful, rendering it necessary to discontinue the bandages altogether, or else to slacken them so much, that all purchase on the bone is lost; and the muscles are then allowed to act upon the fractured portions, and produce separation between them, the upper portion being drawn from the lower.

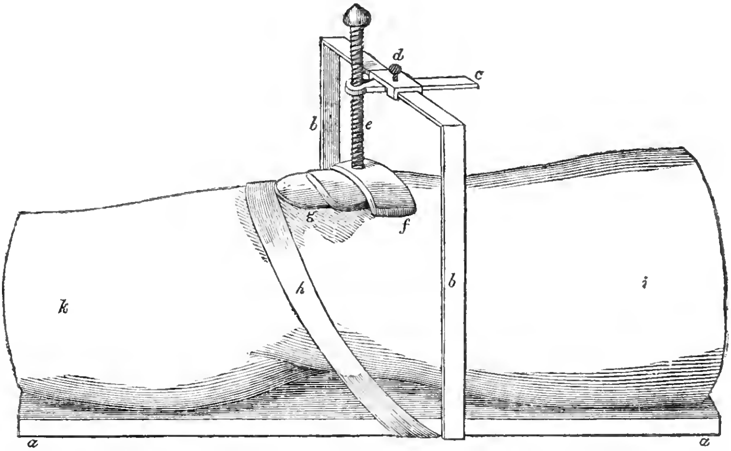
It often happens that swelling of the joint comes on immediately after the fracture, making it quite impracticable to employ the ordinary mode of treatment at an early period, and rendering it necessary to wait some time before any pressure can be made upon the part, or before any attempt can be made to approximate the two portions of bone; the consequence of which is, that much time is lost that is important for the production of bony union; and the muscles having contracted to a great extent, have become adapted to their new situation, and are little liable to yield afterwards when extension is made upon them, rendering it quite impossible to bring the upper portion of bone sufficiently down to the lower, to lie in contact. Ligamentous union is then a necessary consequence.

A case occurred lately at the Middlesex Hospital, in which the least attempt to approximate the two portions of bone by the ordinary treatment of placing the two lateral slips of bandage on either side of the patella, and confining them by a circular bandage and compress, placed above and below the bone, could not be borne, but caused so much pain and swelling of the joint, that it became necessary to discontinue the pressure at two or three different periods; the consequence of which was, that at the end of a month no approximation of the two portions of bone was effected, but a distance of nearly three inches remained between them.

It occurred to me some time ago, that an apparatus might be contrived by which the upper portion of bone could be brought down to the lower, by making pressure immediately above the bone only, and leaving the rest of the joint free, and unexposed to the constriction necessarily produced by the ordinary mode of treatment. The above case appeared to afford a good opportunity for trying the instrument that I had previously thought of, which I accordingly had made; and with the permission of Sir Charles Bell, under whose care the patient was, I applied it.

It consists of a thick wooden splint, which is placed behind the knee, long enough to pass half way up the thigh, and half way down the leg, and sufficiently wide to extend beyond the knee on either side. From a little above the centre of the splint, a bar of iron, about half an inch thick, is thrown across, so as to form a square or arch, which surrounds the joint, but does not come in contact with it; leaving a space of about three inches above the knee, and of about an inch on either side of it, so that the upper and lateral parts of the joint are free from all pressure. Through the central part of this square a flat bar slides backwards and forwards, and is perforated at its extremity to admit a vertical screw, to the lower end of which is attached a horizontal plate, wide enough to extend an inch on either side of the patella, and slightly hollowed anteriorly, to fit the upper convex margin of the bone. The object

of this part of the apparatus is to press immediately above the patella, and so fix the upper portion of bone, which, having been previously brought into contact with the lower, is kept there, and prevented being drawn up the limb by means of the sliding bar, which is fixed by a small screw made to turn upon it. The annexed drawing represents the instrument, and its application to the knee.



a a, The wooden splint applied to the back of the knee, extending half-way up the thigh and down the leg.
b b, The iron bar forming the square or arch round the joint.
c, The sliding bar fixed by the screw *d*.
e, The vertical screw which presses upon
f, The horizontal plate.
g, The patella fractured, illustrating the manner in which it is fixed by the plate *f*.
h, The strap fixing the lower portion of the bone.
i, The thigh.
k, The leg.

It is applied as follows:—The muscles attached to the patella are to be relaxed, as in the ordinary treatment, by extending the leg, and raising the heel and shoulders of the patient, so as to flex the hip-joint. The limb is then to be placed upon the back splint *a a*, which is done by passing the foot through the square formed by the bar *b b*, having previously elevated the vertical screw *e*. The upper portion of the patella is then to be drawn down close to the lower, and to be held there, care being taken that the integument is not pulled with it, so as to be included between the two portions of bone. The sliding bar, *c*, is now to be fixed at such a point, by means of the screw *d*, that the vertical screw *e*, when turned down, may cause the horizontal plate, *f*, to press, not upon the patella, but immediately above it, so that, if the pressure be sufficient, it will lie at a lower level than the bone, and prevent the upper portion being drawn up the limb. The lower portion requires but little fixing; it may be done by the strap *h*, with a small compress of lint placed between it and the bone. The splint itself is steadied by the weight of the limb, but an additional security may be given by attaching a bit of bandage to either side of the splint, and passing it under the sole of the foot. It will now be seen how the retraction of the upper portion of bone is prevented, by means of the horizontal plate; for if the sliding bar be firmly fixed, it cannot be pushed upwards without the whole apparatus goes with it, which is prevented by the bandage that passes under the sole of the foot, and by the weight of the limb pressing upon it.

The advantage of this mode of fixing the patella is, that the pressure employed is confined to so small a part of the joint, and is applied to the part that is best able to bear it, namely, upon the thick tendon formed by the extensor muscles of the thigh. The rest of the joint is left quite free and unexposed to pressure, and open for the application of local remedies; while in the ordinary treatment the whole joint is necessarily

confined and constricted to as great extent, by which any inflammation that may arise is increased, and the employment of local remedies, as leeches, &c. is prevented, without removing all the bandages, when the apposition of the two portions of bone must be disturbed, and an additional evil produced.

By the mode of treatment now recommended, the portions of bone, may be brought into contact immediately after the injury; for the swelling that takes place is mostly at the lateral and inferior parts of the joint, which parts are now free and unconfined.

The important point gained in this, as in other fractures, by the early approximation of the two portions of bone, is, that union can be effected at a much earlier period, and a more direct apposition of the fractured surfaces obtained, than can be the case where the two portions of bone are allowed to remain a long time before they are brought into contact, owing to the state of the joint being such as not to bear pressure generally upon it to the extent produced by the ordinary mode of treating fractures of the patella. This early approximation of the two portions of bone, then, can be obtained by the splint now recommended; for as the pressure does not confine the joint, any swelling that may arise will be free and unconstrained, and not require the removal of any part by which the apposition of the portions of bone will be disturbed.

The advantage of this splint was shewn in the above case, for it could be borne from its first application, although the joint would not bear the pressure produced by the ordinary treatment. The two portions of bone, however, could not be brought into direct contact; nor was this to be expected in this case, for the fracture had existed a month before the splint was applied; during which time the parts around the bone had become thickened, and no doubt ligamentous union already commenced, which would of course prevent the fractured surfaces being brought into immediate contact. The two portions of bone, however, were brought so near, that a distance of only half an inch existed between them, the distance at first being nearly three inches.

I have no doubt that when the splint can be applied at an early period, the portions of bone can be brought into direct contact, and so offer every chance for the production of bony union, the absence of which, in almost every case of fracture of the patella, renders this kind of injury of so serious a nature, by depriving the patient of the free use and strength of the limb, and causing lameness for life.

OBSERVATIONS ON ANIMAL TEMPERATURE,

Suggested by Dr. Williams's Paper.

By Dr. Wilson Philip.

To the Editor of the Medical Gazette.

Sir,

One of the most troublesome consequences of deviating from received opinions in our profession, and of all its consequences the most difficult to obviate, is, that those who reply, usually (especially if the question be a complicated one) reply only to part, not having before them all, that the author has said on the subject.

This may arise from two causes—either all that has been said being distributed among different publications, or the conclusions, in a great degree, depending on what has been ascertained on other parts of the subject, with which the part in question is intimately connected.

At one time I attempted, when such mistakes arose (which are very excusable, respecting the results of an investigation protracted for so many years), to set my opponent right, at least with respect to what I had advanced; but latterly I have allowed things to take their course, satisfied that the truth, whatever it may be, would sooner or later appear: and I the more readily arrived at this determination, because the majority of opponents on such occasions have never seriously considered the subject; an observation, however, which does not apply to the gentleman whose papers induce me to trouble you with this communication. He appears to me, from those papers, both capable of judging of such questions, and willing to take the trouble of carefully examining the data; two things which I have found seldom concur.

In the last communication of Dr. Williams, I find the following observations:—" Hence we must either ascribe to the functions of respiration and circulation the power of generating heat (although to small extent, unless supported by other functions), or, with those who attribute to the nervous energy a power almost omnipotent, assume that the heat generated after the removal of the brain is produced by the nervous organs still remaining in the body. This latter alternative is so entirely destitute of support, that it is unnecessary to discuss it. I will, however, remark of the only author who, to my knowledge, has advocated it, that he had a particular hypothesis involved in it. I allude to Dr. Wilson Philip, who considers animal heat to be a *secretion*, a *tertium quid*, resulting from the operation of the nervous energy, or electric fluid (for he holds them to be the same), on arterial blood."

It is premature to enter on any question of this kind till the point which must more or less influence all our opinions respecting the operations of the nervous influence is determined—namely, the nature of that influence.

I have, by the aid of many experiments, the most important of which have been repeated with the same results by eminent physiologists both of this country and the continent, endeavoured to determine this point; and in my Treatise on the Nature of Sleep and Death, lately republished from the Philosophical Transactions, and my Gulstonian lectures delivered this year, I have given a reference to these experiments; and from page 198 to page 213 of the former, and from page 42 to page 51 of the latter*, have collected together the arguments which appear to myself, and others well versed in such subjects, conclusive, respecting the identity of the nervous influence and voltaic electricity.

Let Dr. Williams examine these passages, in which the whole of the evidence is brought into a small space, and either admit the conclusion or distinctly point out the objectionable part of the evidence; and, the nature of the nervous influence being ascertained, we shall experience no difficulty in determining the points immediately connected with its functions. With regard to the particular position which he considers as wholly destitute of support, I believe he will find that there are more things to be considered than he is at present aware of. It would not, I believe, be difficult to prove, that the removal of the brain, in man altogether fatal to the *sensitive*, although it greatly impairs, does not destroy the *vital*, functions of the nervous system, the spinal marrow, the only other active part of that system, having, in its vital functions, no direct dependence on the brain.

Dr. Williams will find, if he refers to what is said of animal temperature in my Inquiry into the Laws of the Vital Functions (p. 150, third edition), that his term is too strong, when he says that I consider animal heat as a secretion. After referring to the whole of the facts I had stated respecting the subject, the inference is given in the following words:—" That the caloric which supports animal temperature is disengaged by the same means—namely, the action of the nervous power on the blood; by which the formation of the secreted fluids is effected, and consequently that it," that is, the particular action of the nervous power on the blood by which the caloric is disengaged, " is to be classed with the secreting processes."—And in a note, I observe that the intelligent reader will easily perceive how this passage must be modified, if the cause of heat is ever proved to be only a peculiar state of the particles of bodies.

If we employ the term caloric, and regard the cause of heat to be a distinct substance, it follows, of course, that nothing else can be meant in the passage just quoted than that its evolution from the blood is the effect of chemical changes produced in that fluid by the action of the nervous power, the immediate cause, as appears from direct experiment, of the formation of secreted fluids.

I am, sir,
Your obedient servant,
A. P. W. Philip.

Cavendish-Square, Oct. 14, 1835.

* The passage here referred to will also be found in the Medical Gazette, in which the lectures were first published. See Medical Gazette for May 30, 1835, pages 297, 298, 299, and 300.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Popular Treatise on Diet and Regimen; intended as a Text Book for the Invalid and the Dyspeptic. By W. H. ROBERTSON, M.D.

Of suicide Lord Byron somewhere says, "Some men judge so justly of themselves, as to anticipate the sentence of mankind." Our author, prescient of his doom, makes a sort of prefatory atonement, and *dies by his own pen*. However, as the law's fiat has a sanction that is always withheld from self-destruction, it shall be shown by the best of all witnesses—the author and his book (which last, containing *only* 252 pages, he modestly calls a *brochure*)—that, had the sacrificial knife been wielded by our hands, the critical fingers would not have been imbued in innocent blood. Had his literary life been prolonged, the first article in the impeachment would have been for impertinence,—videlicet:

"This book has been written partly for the convenience of my professional brethren, to save them much trouble and time; in fact, to put their patients in possession of general principles, without attempting to interfere with the application of those principles to individual cases of serious disease."

Let us hope he hath no professional brother, who would to a patient,—having a love of physic and a conceit that he could in any way minister to his own medical or dietetic wants,—present his own prescription with one hand, and with the other this "popular treatise," saying—

"Quod si tantus amor menti, si tanta cupido est,
—— et insano juvat indulgere labori
Accipe quæ peragenda prius."

Now come we to a pleasant lamentation over the excellencies of the book—

"The present *brochure* has suffered much from compression, from being confined as to space, and the consequence is the omission of much that is interesting on these subjects, and the superficial mention of much that deserves, and indeed requires, to be discussed at length, and entered into deeply."

Then follows great disinterestedness—

"I have been anxious to give the public a work so small, that no one should be frightened by its size; so cheap, that no one should be prevented from purchasing it by its price, having sacrificed much to gain these ends."

The whole winding up with a powerful recommendation of the style, which it were unjust to give in other words than his own:—

"This work, however, has other faults besides those that I have mentioned. The style is frequently faulty; the putting together of the fragments is, I confess, clumsily managed: but it is my misfortune to be one of those who cannot submit to the irksome task of thinking of the words in which the ideas are clothed, or of correcting, remodelling, or altering their dress. Let me anticipate the critic's censure, by the following quotation from Burton's Anatomy of Melancholy.

"'And for those other faults of barbarism, Dorick dialect, extemporanean style, tautologies, apish imitation, a rhapsody of rags gathered together from several dunghils, excrements of authors, toyes and fopperies confusedly tumbled out, without art, invention, judgement, wit, learning—harsh, raw, rude, phantastical, absurd, insolent, indiscreet, ill-composed, indigested, vain, scurrile, idle, dull, and dry; I confess all ('tis partly affected); thou canst not thinke worse of me than I do of myself.

"W. H. R."

The first chapter opens with two mottos. The first a very trite one from Martial; the other selected from a more modern author, commencing with, "Can I have my ailments removed without abridging my *appetites?*"—in which, if we substitute indulgences for "appetites," we shall find tolerably good sense.

After admitting the justice of the author's exceptions to himself, we have little to discommend, more than the utter superfluousness of such a production—it is supererogatory. It wants the science and vivacity of Dr. Paris's work on this subject, and vainly emulates the shrewd sprightliness of the "Peptic Precepts:" a bad imitation of both, it will supersede neither. Yet if the price be low, as the preface boasts, it is certainly not calculated to do harm.

The Appendix is amusing, inasmuch it professes to illustrate the doctrines of the book by "*fictitious cases!*"

Now, we have all heard of postulates in mathematics, and hypotheses in argument; but "fictitious cases" for the proof and elucidation of a practical art,

when the author confesses "it would have been much easier to have given real cases," are quite another thing. As the practice, however, is not without precedent, so we fear it will not lack imitators.

A New Practical Formulary of Hospitals of England, Scotland, Ireland, France, Germany, Italy, &c. &c. of MM. Magendie, Lugol, Dupuytren, Alibert, &c. Translated from the French of MM. Milne Edwards *and* Vavasseur, *and considerably augmented, by* M. Ryan, M.D.

The title of this book fully describes it; it is a sort of polyglot pharmacopœia, containing a good portion of useful matter, but mixed with a great deal more that will find but little favour at the hands of sagacious English practitioners. Occasions do present themselves when multiform prescriptions of incompatible substances appear to possess some advantage over simpler and more scientific combinations; but this being admitted, constitutes no reason for a general departure from prescribing upon principles so intelligible, that the physician shall always have a satisfactory answer to the medical Pyrrhonist's—*cui bono?*

Great number and variety of prescriptions infer corresponding faith in the prescriber; our confidence in whom would be nearly in the inverse ratio of the extent and diversity of his *materia medica*. It is certain that the number of medicines which admit of no substitute is exceedingly small: *e. g.* arsenic, antimony, mercury, acids, alkalis, opium and cinchona, and a few others. A great extension of this catalogue will rather embarrass than benefit patient, pupil, and practitioner.

Looking through this production, with a view of seeing what the translator did for it in the way of notes, or in what respect it has been "considerably augmented," we happened to alight on a section concerning *Aphrodisiacs*. We read it with disgust, and the more so as we found it was gratuitously introduced by Dr. Ryan. The feeling with which we perused it we can only compare to that excited in Gulliver by the filth and lechery of the Yahoos. He who could take an interest in such a subject would be well employed in assisting the philosopher who was busied with a certain extractive process in the flying island. To be more explicit in our allusions to these matters would be to pander to a taste, the existence of which we discredit, except in a very few instances, among the members of our profession.

MEDICAL GAZETTE.

Saturday, October 17, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

LACHRYMÆ TOOKIANÆ;

OR, A LAMENT FOR THE LOSS OF A CHARTER.

Poor Mr. Tooke! We never thought him so very simple a man as he has just shown himself. The late Duke of Saxe Gotha used to be trotted up and down a room, and then turned round, for the amusement of his company; but when he died, it was found that he had a bony tumor within his skull. Mr. Tooke, doubtless, has a crotchet of his own in his head, for he lets himself be spun like a tee-totum: or worse,—he suffers himself to be sent about like a common hawker, crying a sort of "last speech and dying words." At the bidding of his "very good masters" (the *Council* and *Professors* so called), he distributes through town a statement to inform the public that the joint-stock company to which he belongs, and in which he holds we know not how many shares, has been vilely treated; that *the* Charter by which he and his brother shareholders hoped to make a snug thing of the "University," has been basely refused; and that nothing more remains—nothing more is offered them—than a common part in the lot with the other medical schools.

There is, in short, to be no *dubbing*, after all, in Gower-Street.

This is a disappointment, assuredly, which the speculators on the price of stock cannot be expected to bear with patience. " It never," says Mr. Tooke, in the lachrymose and ludicrous document alluded to,—" it never entered into the contemplation of its friends that an expense of nearly 700*l*. should have been incurred to procure *a barren collegiate charter*, not worth the parchment on which it should be engrossed." We dare say it did not: 700*l*. is no inconsiderable sum to be squandered in idle litigation, more especially when funds have every day to be raised so ingeniously by the speculating parties in question.

But let us go through this " last speech" of Mr. Tooke *seriatim*, and in a manner befitting the occasion; for it is a solemn affair, bearing for its closing device the emphatic phrase " *Animam liberavi meam*,"—which we dare say means, that by this time the unhappy author has given up the ghost.

The opening is lawyer-like enough, containing the necessary quantity of sophistry, in the shape of the *suppressio veri*, and begging the question.

" The origin of this University in 1825, its opening in 1828, and its steady progress since in strict adherence to the *liberal* principles on which it was founded, are *too well known* to require illustration. A freehold estate of eight acres, on which is erected one of the most classical and ornamental buildings of the metropolis, and comprising within its site a convenient and well-conducted public hospital, constitutes an entire academical endowment, adequate to all the purposes of education in every department of liberal and professional knowledge."

The history and progress of the Gower-street school, no doubt, *are* " too well known." Its total failure as a school of arts—the successive resignation of all the respectable professors, (with but few exceptions) who originally belonged to it—its systematic puffing and pretension—and its monopolizing spirit,—are certainly too well known to need dwelling upon. But the *whole* truth, were it to be spoken, would require Mr. T. to say a little more: in alluding to the freehold estate, the valetudinarium, and the *most classical* nature of the building, he might have recollected that the first two are mortgaged, and the last all shell, emblematic of the hollowness of the whole establishment.

After this comes the piteous and " most lamentable comedy" of the grievous disappointment of the speculators: it is traced up from the "encouragement" held out by Lord Grey and his government in 1831 (regarding which *encouragement*, by the way, we rather suspect " the wish was father to the thought"), to the division of last session, on Mr. Tooke's motion for an address to the King; when as the honourable member boasts, it was carried by a majority of 110. But who does not know the history of that division, and the factious manœuvring by which the result was effected? Who can be forgetful of the weight of the radical influence on the occasion, and the foregone determination of embarrassing the ministry of Sir Robert Peel?

We take leave to pretermit the details on this subject with which Mr. Tooke favours us: they contain nothing new for our readers, though the *temperate* tone in which the " opposing petitions from Oxford and Cambridge" are mentioned, as well as those other memorials " from some medical societies and schools," might amuse, if we had only space to devote to it worthy of its merit. We might also notice the gentle whine in which it is told that " the Council were *induced to submit to a restriction* against granting medical

degrees," and the ridiculous blundering with which the complainant talks of rights and claims,—as, where he says that the Council and Professors (save the mark!) were not prepared to relinquish the *right* to confer degrees of the same description as those given by the English Universities—and that the latter bodies "had no pretence for an exclusive *claim*," &c.: but the paper is full of this sort of absurd talk—and a specimen such as we have given may be all that is necessary.

Then follows his Majesty's "gracious" answer to the House of Commons' address. Every body knows what that answer implied, and how nearly it resembled every other civil response given to addresses, however unpalatable and not to be complied with. But Mr. Tooke puts a meaning of his own upon it, and fancies all the world thinks as he does. "The general impression," says the writer, "consequent on this answer was, that the Charter, with very slight modifications, would be immediately issued by the then government."

Was it so indeed? Is this stupid credulity on the part of Mr. Tooke, or is it an attempt to gull his reader? We rather fear the latter: for the answer, in all conscience, was as plain as words could express it—simply that the Privy Council should be pressed for their report, in order that his Majesty might act accordingly. So far from misleading any rational person to suppose that it contained a promise of a charter, every infant must perceive that it was altogether a thing of civility—in fact, a polite put-off.

The present ministers are next taken to task for presuming to hoax Mr. Tooke. For some months, he tells us, they managed to amuse him; but at last, on the 17th of July, the Attorney-General took the liberty of speaking to him any thing but words of comfort. The first law officer of the crown told him, he says, that *two* charters were prepared (this must have raised high expectations in the listener, but, alas! only to be blasted by what followed)—one for "the University," reducing its style and title to that of a "College" (how dreadful!) and thereby precluding it from the privilege of granting degrees,—the other, constituting a Metropolitan University, which should examine for and confer degrees on students proceeding from the several recognized schools.

This seems to have been a thunder-clap to poor Mr. Tooke: but he rallied a little, and presently charged his small artillery to return the compliment; we rather surmise, however, that on consulting his friends, "the Council and Professors," he was recommended to reserve his fire till the opening of the session. It would not do to let the report be heard; much less to have it repeated with troublesome echoes that the chance of a charter was gone for ever: the more *that* untoward piece of intelligence could be bottled and smothered up the better, till after the opening of the session.

Some little change, by the by, seems to have come over the dream of Mr. Tooke since the summer. We observe he takes great pains in his present statement to make the world believe that the Privy Council were on the whole favourable to the proposition of a charter, and that had they not prematurely dissolved, their report would have been all that could be wished. Mr. T. is indignant with the public papers which pretended to announce the feelings of the Privy Council on the subject, and their decision against the measure by a majority: he says there is no truth in this. We fancy, however, it would not be difficult to bring to Mr. Tooke's mind the fact of his having himself been mainly instrumental in propagating such a story. Does he forget that in May

last he made no scruple of speaking freely of the *decision* of the Privy Council against the "University," and that he even went into minute detail to many listeners, naming those Lords who had unexpectedly absented themselves, and those who voted against the measure? The exact numbers were not omitted. But how conveniently short are the memories of some people! A few short months have wiped all the circumstances clean out of the Tookian tablets.

The board—this proposed Metropolitan University board—is the next rock of offence stumbled against in the statement. And it is amusing to observe with what perfect *naïveté* our poor gentleman endeavours to show that the Gower-Street establishment ought to be left free and independent of any such board: or that if it must submit, at least that the Council and Professors ought to be entrusted with the formation of it. "If the University of London," says Mr. T., "should obtain the power of conferring degrees as a separate body, there can be no doubt that *in no long time it would raise up a body of graduates, to whom its examinations might safely be entrusted.*" How very accommodating! No doubt it would be highly satisfactory to the other schools of the metropolis, if they objected to sending their pupils for degrees to the teachers in Gower Street, still to have a *dernier resort:* the said teachers would obligingly raise up a senate "in no long time,"—a disinterested uninfluenced senate, composed of the first fruits of the system—graduates of the Gower-street joint-stock self-styled University! Thus "the supposed difficulty," according to the statement, "would soon be obviated."

Yet will the reader believe that, in the very next paragraph, the "University" is said to desire "*no exclusive privilege?*" Such is the fact. Nay, again, in a subsequent passage it is stated, *totidem verbis*, that were *it* merged, as it would [we trust, *will*] be by the ministerial measure, with other institutions, "its energies would be repressed, its individuality extinguished, its integrity of purpose impaired," &c.; for which reason *it* spurns the proposed central board, and can only be satisfied by being itself, *par excellence*, allowed to confer degrees. Yet this immediately follows the assertion that "the University of London solicits no exclusive privileges"! What have the Council and Professors been about, that they should suffer their unhappy "solicitor" to make such a confounded exhibition of himself?

In a postscript, evidently of recent date, we find the writer in a high delirium — the precursor, no doubt, of the regular expiration which he makes of it in the end. He has just discovered that the Central University Examining Board is to be composed of Commissioners named by the King, and that candidates for degrees are to be admissible to it from all parts of the United Kingdom, and from every recognised seminary of education, whether chartered or unincorporated. This is more than he can bear: he becomes furious, and abuses Ministers fearfully; he denounces the Board that is to be, sneers sarcastically at "the erudite, the Lords of the Treasury, and the Secretaries of State," and "that very learned body the House of Commons,"—totally forgetful of the high terms in which he mentioned the said House a little before, for lending him the assistance of a packed majority. At last, though, he bethinks him of an expedient: a happy thought strikes him: he suggests an antidote. It is this: to take advantage, and make the most possible, of the King's answer. "His Majesty having, in his most gracious answer, recognised by name the University of London" ["so called" being evidently omitted by mistake], is it not

proper that the proprietors should avail themselves of "this *sanction*," and "confer all manner of degrees, except in theology and medicine?" Ought not such a power, he asks, to be exercised in the ensuing session "in proud and fearless competition with any existing institution?"

Truly, Mr. Tooke, you have "liberated your soul" in a hopeful mood. You would deserve to be canonized for the integrity and wisdom of your dying suggestion. The plan proposed is quite worthy of the party who suggested, not long ago, at a meeting of the proprietors, to lay an embargo on the shares, and to confiscate the property vested in the establishment, in order to meet the objection of the "University" being a joint-stock concern.

It now only remains to be seen what the "University" will do. Will they really venture to act on the advice of their worthy solicitor? or has that suggestion of his been only thrown out as a feeler, like the confiscation project? It is truly pitiful to observe the shifts of the whole set: nothing seems too mean—nothing too tricky, for their adoption, so as it only opens the prospect of saving them from ultimate bankruptcy. We should not be at all surprised if they tried the plan of their worthy solicitor: nor shall we say that they have taken us unawares if, ere long, we see them embarked in a brisk traffic of degrees,—in humble imitation of the *flourishing* "College of Medicine, Lancaster Place, Strand."

Since the above was written, we perceive with much pleasure that Professor Key, in an opening lecture, has expressed himself in terms of satisfaction regarding the new charter. Only let his colleagues speak out in like manner, and our hostility shall no longer subsist.

"FAMA CLAMOSA."

CASE IN THE SCOTTISH COURTS INVOLVING THE QUESTION OF *VIABILITY*.

To the Editor of the Medical Gazette.

SIR,

I BEG to direct the attention of your readers to the following report of proceedings in one of the Scottish Church Courts. It will interest them as professional men, and may induce some of them to favour us with the results of their experience on the point at issue.

The child in this case is stated not to have been three pounds in weight: now last summer I saw one, prematurely born (certainly before seven months), which at birth weighed only two pounds and a quarter; it was alive when I saw it, and lived twelve days. But the important question, and on which I seek information, is, at how early a period of premature birth has the life of a child been preserved?

On Wednesday there was a *pro re nata* meeting of the Presbytery of Kirkaldy, relative to Mr. Jardine, of Kinghorn, whose lady has given birth to a living child within six calendar months of his marriage. Mr. Jardine was married on the 3d March, and Mrs. Jardine, on the 24th August, was delivered of a very weak child—not three pounds in weight—but which, with great care, has hitherto been kept alive. About three weeks since, after a meeting of Presbytery, held on the subject of shutting up a road which led to the kirk and manse of Kinghorn, Mr. Jardine informed the Presbytery of Mrs. Jardine's premature delivery; and as there were reports in circulation injurious to his character and usefulness, threw himself on the Presbytery, and called on them to make the fullest investigation, solemnly declaring at the same time his innocence of every thing criminal. The Presbytery agreed to request Dr. Hamilton, Professor of Midwifery in the University of Edinburgh, to come to Kinghorn to investigate and report; but if not convenient for him to come, a similar application was to be made to Dr. Thatcher. Dr. Hamilton could not come, but wrote two letters, stating that his own experience was opposed to the probability of a child, born in the sixth month, surviving; referring, however, to two cases where children had lived under similar circumstances of premature birth to that of Mr. Jardine's child. One of these occurred in 1710, when the wife of a clergyman in the Presbytery of Wigton was delivered of a living

child within five lunar months after marriage; and the celebrated Dr. Pitcairn, of Edinburgh, and other two eminent practitioners, gave it as their opinion that that child had been procreated after the marriage of its parents. The other case occurred in Paisley in 1815, when a married woman, who previously had children, gave birth to a living child nineteen weeks after conception, and it lived a year and a half. Dr. Thatcher was then written to, and he came to Kinghorn and examined the child; as also Dr. Reid, of Kirkaldy, who was accoucheur to Mrs. Jardine; the sick nurse; and the other persons present at the birth; but the certificate he gave, which was laid before the Presbytery on Tuesday week, not being so explicit as some members wished, he was again written to, when he transmitted a certificate, of which the following is the substance:—

"I hereby certify, in connexion with my former certificate and note appended, that I consider it quite possible that the child of the Rev. Mr. Jardine was begotten on or after the 3d day of March last, born on the 24th day of August subsequently, and reared in the premature state described; and I consider the *fama* on this circumstance, against the minister of Kinghorn, as utterly groundless."

On Wednesday this communication was laid before the Presbytery, when the majority of the members expressed themselves perfectly satisfied; and Mr. Guild, of Auchtertool, submitted a motion to the effect, that from the certificates of Dr. Thatcher, it appeared there was no evidence to criminate Mr. Jardine, and that the *fama* against him was unfounded. A good deal of conversation followed, in the course of which a petition, signed by about thirty of the parishioners, was given in, praying that the Presbytery would order a Presbyterial visitation of the parish, "before coming to any decision whatever in reference to the subject of the birth of the child." This petition was received, and ordered to lie on the table. The discussion was here renewed with considerable keenness, Mr. Jardine deprecating the Presbytery being driven from their purpose of coming to a decision by the side-wind of this petition, stating, that if the petitioners were prepared to libel him, he would meet them most readily. The Presbytery, however, was bound to come to a finding on the evidence before it, which would not preclude the parishioners taking any steps they might hereafter consider proper. Mr. Murray, of Dysart, Mr. Cunninghame, of Kinglassie, and others, supported the Presbytery coming to a finding similar to that proposed by Mr. Guild. Mr. Christie, of Durie, coincided in opinion that the medical certificates completely exculpated Mr. Jardine; but thought the petition of the parishioners ought to be attended to, and suggested that the Presbytery be adjourned. Mr. Bell, of Kennoway, concurred in the suggestion of Mr. Christie. Mr. Murray, of Abbotshall, and Mr. Thomson, of Dysart, contended it was for the interest of Mr. Jardine that the Presbyterial visitation should be ordered, the more especially that the decision of a *pro re nata* meeting might be cancelled by an ordinary meeting. Mr. Murray, of Abbotshall, argued that the Presbytery ought to have called Dr. Reid before it, and examined him on oath. To this it was replied by Mr. Brewster, that it could hardly be expected that any member of Presbytery would conduct such an examination: hence Dr. Thatcher had been applied to to take evidence, and his report was now before them. Mr. Seiveright, of Markinch, in reference to some remarks made at a former meeting relative to Dr. Reid, read a letter from that gentleman, in which he denied having used certain expressions generally attributed to him. When examined by Dr. Thatcher, whom he had never before met, he (Dr. Reid) considered himself in the character of a witness. Dr. Martin, in a lengthened and remarkably logical speech, delivered with much feeling, contended that, besides the medical certificates, every presumption was in favour of the innocence of Mr. and Mrs. Jardine. Their previous character was free from all suspicion, and there had been nothing like concealment practised. Their intended marriage was approved of by their respective friends months previous to its celebration, and there was nothing to prevent its taking place. When the child was born there was no preparation for it—not a single article of clothing—the parties were all taken by surprise, and no one ever expected the infant to survive. After some further conversation, the motion of Mr. Guild, altered to the following effect, was unanimously adopted:—

"The Presbytery, having deliberately considered the evidence before them, as afforded by the certificates and letters of Dr. Thatcher, relative to the case of Mr. Jardine, declare that there is no ground to charge him with the criminality alleged against him."

The Presbytery agreed to delay consideration of the petition till next ordinary meeting. Against these proceedings Mr. Barclay, on the part of the petitioners, protested, and appealed to the Synod*.

D. A.

* Fife Herald, Oct. 1, 1835.

GENERAL LYING-IN HOSPITAL.

MIDWIFERY REPORTS.

By Edward Rigby, M.D. F.L.S. &c.

[Continued from page 32.]

Case complicated with Mental Depression.

The next case is, indeed, a melancholy one. The patient had been born to better and brighter hopes. The favourite of a respectable family, well educated, and possessed of considerable personal attractions, she had been seduced by a licentious foreigner under promise of marriage, and then deserted. During her pregnancy she had lost her mother under peculiarly distressing circumstances. She had been seized with a fit shortly after being made aware of her daughter's ruin, and being near the fire at the moment, had fallen against the stove, and been so severely burnt as not to survive long. This had tended not a little to increase her state of mental suffering; she considered herself to have been the cause of her mother's death, and from that time nothing appeared to rouse her from the melancholy subject which occupied her thoughts. She made no complaint; and to any inquiries after her health almost always returned a favourable answer. Her nights were passed quietly, but she seldom appeared to sleep. For the particulars of this long and interesting case I am indebted to my friend, Mr. Bell.

"Dec. 21st, 1834.—M. H., æt. 22 (single); delivered of a boy; first child; second position; labour natural, and very quick. She was in the hospital a short time before her delivery, during which there was a remarkable depression of her spirits; her face and lips were always pale, and this paleness did not leave her even when straining during labour. About two hours after her delivery she complained of giddiness; and thinking it might arise from fatigue, the nurse gave her some tea; immediately upon taking which she was seized with a profuse discharge of serous fluid from the vagina, which at first flowed slowly, but afterwards came in gushes, accompanied with great faintness, ringing in the ears, and sinking of the pulse. The discharge was so profuse as to completely soak the sheet which was doubled round her, and several folds of the bedding beneath. On examining the uterus it was found enlarged, extending to the umbilicus, and soft, but there were no clots felt on introducing the hand into the vagina. She had another sheet bound tightly round her, and the following draught was given:

> Sp. Ammon. Arom. ♏x.; Ætheris Sulph. ʒss.; Aq. Menth. Pip. ℥j. M.

after taking which the faintness left her, and the pulse rose.

22d.—Feels better, and had only one return of the giddiness during the night; she has slight tenderness of the abdomen, and constant pain in her back; she has not passed water since her delivery, and the bladder is felt much distended above the pubes. Lochia profuse, and some small clots have come away this morning. The catheter was introduced, and a large quantity of high-coloured urine was drawn off, which relieved the pain of abdomen; pulse frequent and feeble; tongue white, and very tremulous.

27th.—Says she is quite comfortable, and has had no pain in her back for several days. Lochia regular, but unnatural in appearance since the second day after her delivery. Bowels have been kept regular by castor oil; but as she expressed great dislike to it, she had to-day four grains of comp. ext. of colocynth, and the same of ext. hyoscyami. Spirits much depressed; face very pale. Appetite good, and she has meat; notwithstanding which her milk has diminished in quantity.

28th.—In the morning appeared much as she was yesterday; had a grain and a half of quinine at noon. In the evening she became slightly incoherent, and when she was spoken to started as from a stupor, but answered quite collectedly. Dr. Rigby saw her, and ordered

> Camphoræ, Ext. Hyosc. aa. gr. iv., and to be repeated in three hours.

She has slept indifferently for some nights.

29th, 8 A.M.—Makes no complaint, and says she feels quite comfortable. Bowels opened three times since last night; pulse 144, very feeble; tongue white and tremulous.

> ℞ Liq. Opii sed. ♏vj.; Mist. Camph. ʒss. statim.

Noon. — The bowels have not been opened.

> To have a grain and a half of quinine. Arrow root and port wine for food.

Has been occasionally incoherent during the forenoon. Dr. Rigby saw her soon after.

> ℞ Opii duri, gr. j.; Camphoræ, gr. iij. M. omni horâ usque ad somnum. Let her have emulsion of egg and brandy.

After taking three of the above pills she fell asleep, but was very restless.

Pulse 144, very feeble; subsultus tendinum.

9 P.M.—Has just waked suddenly and leaped out of bed, and said she wished to dress herself, as it was time she was up; she seemed to be labouring under great nervous irritation, and spoke hurriedly, though collectedly. She returned quietly to bed.

Continue the opium and camphor, and to have a draught containing one grain of quinine, ten drops of diluted sulphuric acid, in a little cold water, at 3 in the morning.

30th, 9 A.M.—Has passed a quiet night; tongue very dry; bowels opened once; pulse 144, rather fuller.

Repeat the quinine and sulphuric acid immediately, and every two hours.

Half-past 3 P.M.—Complains of pain in her left side when she breathes. Tongue moister; pulse 100, intermitting, and very feble.

To have the side rubbed with anodyne liniment; the quinine to be continued; and to have beef-tea frequently.

5 P.M.—Much the same as at last visit. Dr. Rigby saw her, and ordered the opium and camphor to be given at 6, and the quinine at 8.

Omit the beef-tea, and let her have calf's-foot jelly instead.

9 P.M.—Pulse again up to 144, fuller, and more regular; tongue dry, and very tremulous.

Half-past 11.—Has had some sleep, but moaned very much during it. When spoken to she requires some time to collect herself to answer. Dr. Rigby saw her.

℞ Ext. Hyosc., Ext. Gentianæ, aa. gr. iv.; Quinini Sulph. gr. ij. M. and divide in pil. ij. to be taken at 2 in the morning. Continue the beef-tea.

31st, 8 A.M.—Slept a good deal during the night, but moaned and seemed much distressed. Is much weaker, and has great difficulty in speaking, or in opening her mouth to shew her tongue, which is very dry. Has taken very little of the beef-tea; stomach slightly distended.

10 A.M.—Not having passed water since last night, the catheter was introduced, and brought away nearly two pints of urine with much relief.

℞ Ext. Gentianæ, Camphoræ, aa. gr. vj. Quinini Sulph. gr. iij. M. ft. pil. iv., two every two hours. Continue the beef-tea. The catheter to remain in, and the water to be drawn off every two hours.

Jan. 1st.—The powers of life continued to sink gradually, and she died without any suffering at 10 this morning.

REMARKS. — There are several remarkable facts about this interesting case, which are well worthy of notice. The great and overpowering depression of mind must be looked upon as the main cause of her having sunk thus; it was a state of profound melancholy, which was beyond the passionate demonstrations of common sorrow, and which annihilated every other thought and feeling. To the great depression which the physical powers suffered from this state of mind must we attribute the sudden and very remarkable discharge of serous fluid from the vagina, which took place after labour. Serous discharge from the vagina may occur under very different states, and from equally different causes. In the present case it appeared to result from insufficient contraction of the uterus, allowing the watery portion of the blood to escape from the mouths of the vessels which open upon the lining membrane of its cavity. This affection has been lately noticed by Dr. Ashwell, in the Medical Gazette, and also by Mr. Bury; but with the exception of Dr. Ashwell's last case, it appears to have been of a different character.

The delirium and other symptoms which appeared on the 28th, were as much the consequence of sleeplessness as of debility. She had passed her nights so quietly, that to a person merely passing through the ward she appeared asleep, although, on going up to her bed, she was always found awake. It was to calm the cerebral excitement and produce sleep that I prescribed the henbane and camphor. The next day passed much in the same manner; she said she felt quite comfortable; but the fact was, she had not slept to any extent, and a return of incoherence justified the active doses of opium which I then exhibited. It now, in short, became a struggle (and a fruitless one) between the prostration into which she was gradually sinking, and the efforts of her attendants to rouse the failing powers of life. My own conviction is, that even if the powers of her constitution had been able to withstand the depression under which she laboured, the mind would not, and that mental aberration would have been the result.

[To be continued.]

VAGITUS UTERINUS.

M. DUBROCA, of Barsac, has communicated a remarkable instance of this kind to the *Bulletin Méd de Bordeaux*. He says that in 1827 a midwife called upon him,

and mentioned, with evident incredulity of her own story, that she had just heard an infant cry in the womb of its mother. M. Dubroca went immediately to see the woman. She was a person in good health, had already given birth to two children, and was now in the eighth month of her pregnancy. The crying had begun during the preceding night, and had continued since, with intervals only of a few minutes cessation. The noise was that of a new-born infant, but not so loud; rather resembling the cry of a child shut up in a box. When the woman walked, the voice always seemed to proceed from the place where she was. After three days the phenomenon ceased altogether; but it was not till a month and a half afterwards that the child was born. It was a fine male child, which uttered cries at birth, exactly like those previously heard: it is still living. The poor woman (the mother) was greatly alarmed, and shed tears copiously. Somebody had frightened her by saying that she must be possessed with the devil, if she was not a witch or a ventriloquist. M. Dubroca was satisfied that she was no witch at least, and seems persuaded that she knew nothing of the nature of ventriloquism.

RADICAL CURE OF VARICOCELE.

Dr. Fricke, of Hamburgh, has cured three patients labouring under varicocele, by simply passing a needle and thread through the dilated veins, and allowing the thread to remain for twenty-four or forty-eight hours, according to the degree of reaction produced. He recommends the method as being as safe as it is simple.—*Gaz. Medicale.*

TURPENTINE IN SCIATICA.

Dr. Ducros, of Marseilles, gives an account of the great efficacy he found in spirit of turpentine administered in cases of sciatica. He employed it in the form of lavement; gradually increasing the quantity at successive injections, until it amounted to two or three ounces. He conceives it to have a specific effect.—*Gaz. des Hôpitaux.*

PUBLIC HEALTH IN PARIS.

A species of typhoid fever has prevailed epidemically in the metropolis during the last year, and still continues its insidious ravages. An alarming number of young men, students in law and medicine, have been cut off: and of the troops which have come in the course of the year from the provinces to Paris, above one-third have been lost.—*Revue Medicale Française.*

ECOLE DE MEDICINE.

The opening of the school is fixed for the 2d of September. M. Broussais is to deliver the introductory address, which it is expected will be a very different thing from that pronounced last year by M. Fouquier.—*Gaz. des Hôp.*

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

October 15th, 1835.

William Field, Longdon, Staffordshire.
Albert Philip Owen.
William Williams, Ponty Poole.
William Williams Morgan, Inerthyn Tydvil.
Richard Henry Meade, Risboro', Bucks.
Henry Charles Adolphus Clarke, Ramsgate.
Jonas Hellawell, Huddersfield.
Joseph Henry Spencer, Leicester.
George Crozier.
Henry Foote Edwards, Brislington.
William Tesseyman Jackson, Harworth.
Richard Jones.
Thomas Boadshaw, Huddersfield.
Wellington Ellis, Abergavenny,

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *Oct.* 13, 1835.

Abscess	1	Lungs and Pleura	5
Age and Debility	31	Insanity	1
Apoplexy	12	Jaundice	1
Asthma	5	Liver, diseased	5
Cancer	1	Measles	11
Childbirth	1	Mortification	5
Consumption	53	Paralysis	1
Convulsions	40	Small-Pox	20
Dentition or Teething	6	Sore Throat and Quinsey	1
Dropsy	8	Stone and Gravel	1
Dropsy on the Brain	14	Thrush	1
Fever	4	Tumor	1
Fever, Scarlet	18	Worms	1
Heart, diseased	4	Unknown Causes	1
Hooping Cough	2		
Inflammation	23		
Bowels & Stomach	4	Stillborn	16
Brain	5		

Increase of Burials, as compared with the preceding week 53

METEOROLOGICAL JOURNAL.

Oct. 1835.	Thermometer.		Barometer.	
Thursday . 8	from 45	to 58	29·92	to 29·59
Friday . . 9	43	55	29·47	29·20
Saturday . 10	42	49	28·80	29·02
Sunday . . 11	38	50	29·20	29·59
Monday . . 12	36	51	29·79	29·82
Tuesday . . 13	50	62	29·85	29·94
Wednesday 14	51	58	30·07	30·23

Prevailing wind, S.W. and N.W.
Generally cloudy, with frequent showers of rain.
Rain fallen, ·625 of an inch.

Charles Henry Adams.

ERRATUM.

In Dr. Wilson's letter, last number, page 62, for "*concerted*, yet active hostility," read '*concealed*, yet active hostility."

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, OCTOBER 24, 1835.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE IV.

ON THE SECONDARY OR THERAPEUTICAL EFFECTS OF MEDICINES.

EXPERIENCE has demonstrated that diseased action may be modified by the exhibition of medicines, and the modifications which are thus produced have been denominated *secondary* effects, because they are subordinate to those already described under the name of physiological. As we take advantage of them in the treatment of diseases, they have been likewise called *therapeutical* effects; and there are certain terms in use which refer solely to them, such as—febrifuge, bechic, stomachic, antiseptic, antispasmodic, antiperiodic, deobstruent, antiscorbutic, anodyne, antiarthritic, antivenereal, antihysteric, lithontriptic, &c.

It were, however, much better if these terms had been altogether banished from the Materia Medica, since they convey little meaning. To say that a substance is febrifuge gives you no information as to its mode of operation; for bloodletting, emetics, purgatives, sudorifics, blisters, stimulants, sedatives, narcotics, mercurials, and tonics, have been successfully employed in fever, and deserve, therefore, to be termed febrifuge, notwithstanding that their action on the body is diversified, and in some cases completely opposite.

But these terms are objectionable on another ground; they are not always appropriate. You cannot say that sulphate of quinia acts as a febrifuge when fever is not present; nor that opium is an anodyne, when no pain exists. We ought rather to imitate the naturalist who draws his characters of animals and vegetables from those properties which are constant and invariable, and avoids employing all those which are temporary and accidental. If it be desirable (and no one can doubt but that it is so) to have some term expressive of the effects which certain agents produce on the body, let that term apply to those effects which are constant and universal; let all phrases of dubious meaning be avoided—phrases that can only be applicable in particular conditions of the body, and not always even then; for sulphate of quinia is not febrifuge in every fever, nor is opium anodyne in all painful affections.

Remedies are not uniform in their influence over diseases: at one time we find them beneficial; at another, the same agents are insignificant and useless, or even absolutely hurtful, apparently under similar circumstances. In a large proportion of diseases, if not in all, there exists a natural tendency in the constitution to the healthy state—a tendency which constitutes the *autocrateia* of Stahl, the *vis medicatrix naturæ* of Cullen. In those diseases usually termed malignant (cancer and fungus hæmatodes, for example), we have no evidence of the existence of such a restorative principle beyond their occasional stationary condition. Now in most cases our remedies facilitate the removal of diseases only by removing those circumstances that prevent the full operation of this tendency; a *cure*, consequently, is rather referrible to nature than to the efforts of the practitioner. Nay, when we see patients recover under the most opposite means of treatment, we can hardly refuse our assent to the observation of the late Sir Gilbert

Blane, that in many cases patients get well in spite of the means employed; and sometimes when the practitioner fancies he has made a great cure, we may fairly assume the patient to have had a happy escape.

It will readily be admitted that medicines frequently influence and modify the progress of disease; but their *modus operandi* is not so easily explained, in consequence of the little knowledge we possess of the nature of vital action. However, it is probable that every medicine acts either on the causes of diseases, or on the vital properties of the part affected, and on this belief we may construct the following table:—

Medicines may act	On the causes of diseases	directly *(specifica qualitativa)*	
		indirectly.	
	On the vital properties of the diseased parts	immediately	
		mediately by	mechanical relations,
			functional relations,
			sympathetic relations.

I. *Of the operation of medicines on the material causes of diseases.*—Under this head I include all those cases in which medicines remove known causes, or, to use the language of Dr. Pring, which act upon a direct principle of causation. We make two subdivisions; the first including those causes *directly* acted on, and the second those that are affected *indirectly*.

a. Those medicines which act *directly* on the causes of disease are denominated by Hufeland *specifica qualitativa;* they are the substances usually called *specifics*. We know, for example, that the tape worm (*tænia solium*) sometimes occasions serious symptoms; and that the oil of turpentine frequently relieves, by causing its destruction and expulsion. Now it is supposed that the oil acts as a poison to the worm. It has long been suspected that scabies is produced by an insect called the *acarus scabiei*, and that sulphur cures by destroying it. If this opinion be correct, we have here another instance of the direct operation of a medicine on a morbific cause. I may remark, however, that, although the existence of an insect has frequently been demonstrated, yet it is not determined whether we ought to regard it as the cause, the accompaniment, or the effect of the disease, and, therefore, we can speak but doubtfully as to the mode by which remedial means influence scabies.

Corrosive sublimate, and the salts of copper, taken into the stomach in large quantities, inflame this viscus. Now, albumen, or the white of eggs, decomposes them, and diminishes their activity. Hence the chemical antidotes, like the substances called specifics, act on the causes of diseases. It is much to be regretted that our list of specifics is so extremely limited. Foy, a French pharmacological author, admits a distinct class of medicines under this title; but he employs it in a far more extensive sense than is generally done. If, says he, the term specific be confined to those agents which invariably cure a disease, there are not any substances deserving this appellation; he therefore calls those medicines specifics which are better adapted than others to certain diseases.

In this sense he admits the following subdivisions of the class *specifica*.

1. *Specifics for syphilitic affections*, including the preparations of mercury and gold.

2. *Specifics for cutaneous diseases.*—These he divides into vegetable (namely, *fumaria*, *cichorea*, *rumex patientia*, and *arctium lappa*;) and those furnished by chemistry (namely, sulphur and its preparations, arsenicals, iodnret of mercury, and some preparations of phosphorus.)

3. *Specifics for periodic affections.*—Under this head are the principal vegetable tonics, as cinchona, willow, simaruba, and other barks, sulphate of quinia, salicine, &c.

4. *Specifics for scrofulous affections*, divided into (*a*), general means, as elevated places, healthy habitations, strengthening food, exposure to the sun, &c.; (*b*), vegetable substances, as hops, gentian, cinchona, &c.; (*c*), substances furnished by chemistry, as iodine and its preparations.

5. *Specifics for intestinal worms, or anthelmintics*, arranged under the heads of vegetable substances, as Corsican moss, male fern, wormseed, castor oil, &c.; agents furnished by chemistry and mineralogy, as tin, calomel, petroleum, &c.; and, lastly, animal substances, as white coralline.

This table is interesting as showing what agents have been celebrated in particular diseases, but the term specific is employed too loosely.

b. Substances which act indirectly on the material causes of diseases.—Suppose a gall stone to have escaped into the ductus choledocus; it may, in this situation, excite spasm and inflammation. Now it has sometimes happened that the stone has been dislodged by the mechanical influence of vomiting. If, therefore, vomiting had been caused by ipecacuanha, this medicine would have given relief through its indirect

action on the cause (the calculus) of the disease (spasm and inflammation). We can conceive, also, other cases in which the relief obtained is referrible to a similar mode of causation.

In some instances medicines indirectly remove the effects of a previous disease, which effects, by their reaction, maintain a secondary morbid state. For example, the bowels may be loaded with depraved secretion, the result of some previous disorder, but which has now subsided; and this secretion may excite a secondary morbid condition of the system. A purgative may relieve all these symptoms, by removing the cause or reagent.

II. I proceed to examine those cases in which we believe the beneficial effect of a medicine depends, not on its action on the cause or causes of the disease, but by its influence over the *vital properties* of the *diseased part*.

In a large majority of instances, the causes of disease are either not of a material nature, or are unknown. In all such cases we administer medicines with the view of producing certain changes in the actions of one or more parts of the system, and thereby of so altering the diseased action as to dispose it to terminate in health. Thus inflammation of the lungs frequently subsides under the employment of nauseating doses of tartarized antimony; and emetics will sometimes put a stop to the progress of hernia humoralis. In these instances the remedies appear to have an indirect relation only to the disease.

The following arguments have been advanced by Barbier, to prove that the therapeutic effects of medicines are, in most cases, consequences of the physiological changes they have produced in the system.

1. A medicine seldom produces an amendment in a disease without primarily giving rise to an organic operation in the body affected.

2. When from age, bad preparation, or any other cause, a medicine has lost its power of acting on the living organs, or when from habit or idiosyncrasy the organs are insensible to its action, it becomes useless as a therapeutic agent; it no longer possesses a power of alleviating or curing diseases. So that those causes which annul the active force of medicines destroy also the curative powers.

3. Those medicines which produce the most violent physiological effects are the most powerful therapeutic agents; for example, tartar emetic, opium, bark, &c.

4. A specific curative power in medicines cannot be demonstrated. To admit that medicines act on the causes of disease would be to assume that these causes are of a material nature—an assumption which, in nine cases out of ten, is unwarrantable.

5. Sometimes medicines which are regarded as specifics in certain diseases exasperate, instead of removing the symptoms. Frequently we find febrifuges increasing the intensity of a fever, tonics augmenting the debility of the system, and antispasmodics adding to the violence of the nervous symptoms. Now this could not be the case if these agents possessed the powers that their names indicate.

6. In order to produce beneficial effects, medicines must be exhibited at the proper time. Cinchona bark, for example, may at one stage of an intermittent put a stop to the paroxysm, at another increase its intensity.

7. Various external circumstances have occasionally been useful in diseases, and which cannot possess any specific curative power. Thus sudden fright has sometimes prevented the paroxysm of an intermittent fever; removal to a warm climate frequently causes all the symptoms of pulmonary diseases to disappear.

Medicines which act on the vital properties of a diseased part may do so in one of two ways, either immediately, that is, by local contact, or mediately, that is, by an operation on some part having a relation with that diseased.

a. Medicines operating immediately on the vital properties of the diseased part.—Under this head we include those agents called topical, as unguents or lotions in cutaneous diseases, ulcers, &c.; gargles in affections of the mouth and throat; collyria in ophthalmic diseases; and injections into the vagina and uterus in affections of these organs. In all such cases we can explain the therapeutic effect in no other way than by assuming that the medicine sets up a new kind of action in the part affected, by which the previous morbid action is superseded, and that the new action thus set up subsequently subsides. Sometimes it may be suspected that the influence which certain agents exercise in diseases of remote organs, arises from the medicinal particles being absorbed, and, through the medium of the circulation, carried to the parts affected. Thus the beneficial influence which the turpentines occasionally exert on mucous membranes (as in gleet and leucorrhœa) may perhaps be owing to a topical influence of this kind; as also strychnia in affections of the spinal marrow.

b. Medicines acting mediately on the vital properties of the diseased part.—Under this head I include all those agents operating on some one or more parts of the body, which have a relation with the diseased part. Now I have before mentioned that we

make three kinds of relations, namely, the mechanical, the functional, and the sympathetic; and I purpose still adhering to this arrangement in discussing the operation of this class of medicines.

Mechanical relations.—We know few, if any, instances in which the therapeutic effect can be attributed solely to the mechanical relation existing between the vital actions of the diseased part and the organ affected by the medicine, although now and then the effect must, in some degree, be referred to this circumstance. Thus, vomiting may modify the functions of remote organs by the mechanical violence produced; and diseases of these parts may, in consequence, be influenced.

Functional relations.—Every portion of the living body requires two conditions to enable it to carry on life: 1st, the presence of a certain quantity of arterial blood, of a proper quality; 2dly, nervous influence: so that every organ has a functional relation to the vascular and nervous systems; and, therefore, alterations in the quantity or the quality of the blood sent to it, or in the supply of nervous energy, must be attended with corresponding alterations of function. Now we can modify the condition of the blood by varying the quality of food, and, therefore, in a secondary way, the functions of distant organs become affected. For example, it is well known that abstinence from vegetable food, and the continued use of salted meats, give rise to an alteration in the quality of the blood, attended with other symptoms, constituting the disease called scurvy: and we know, also, that the blood will resume its healthy character, and the disease be removed, by the employment of fresh vegetables, and citric or tartaric acids. Hence it seems fair to infer that the beneficial effect of these remedies depends on their improving the quality of the blood. In cutaneous diseases benefit may sometimes be gained by the use of medicines which excite the vascular system generally, and which thereby promote the secretion of the skin: in such cases the therapeutic effects depend on the functional relation between the vascular and the cutaneous systems. So also, when we employ digitalis and refrigerants in inflammatory affections, our object is to diminish indirectly the quantity of blood sent to the affected part, by acting on the general vascular system, and thereby influencing the diseased action.

The effect produced by what is called *contiguous sympathy* must, I think, be sometimes referred rather to a functional than a sympathetic relation. In amenorrhœa, when we are desirous of determining to the uterus, we sometimes effect our object by acting on the vascular system of a neighbouring organ (the rectum), as by aloes, knowing that the vessels of the uterus in consequence receive a greater supply of blood.

In the second place, we sometimes obtain valuable therapeutic effects by modifying the supply of nervous energy to the affected part. Thus, in spasm of the bowels, as also in pain arising from the passage of calculi, great relief is experienced by the employment of opium; which, acting on the nervous system, gives relief by the functional relation existing between this system and the part affected.

Sympathetic relation—*Sympathy.*—Medicines sometimes influence morbid actions mediately under circumstances which cannot be referred either to mechanical or functional relations, and, therefore, we are compelled to call to our aid this unknown relation termed sympathy. Thus purgatives relieve cutaneous disorders and affections of the head; blisters are frequently useful in internal complaints, diaphoretics in affections of the alimentary canal, diuretics in dropsies, &c.

In general, however, the relief obtained does not arise solely from any one mode of action. A purgative administered in an affection of the head may give relief in several ways; by evacuating morbid secretions, which may be a source of irritation; by determining blood towards the bowels, and thereby relieving the cerebral vascular system; by influencing the digestive process, and thereby the quality of blood; and lastly, by sympathy.

FUNDAMENTAL METHODS OF CURE.

Let us in the next place examine the *fundamental methods of cure* which have been adopted and followed in the treatment of disease. According to Hahnemann there are only three possible relations between the symptoms of diseases and the specific effects of medicines—namely, *opposition*, *resemblance*, and *heterogeneity*. It follows that there are only three imaginable methods of employing medicines against disease; and these he denominates *antipathic*, *homœopathic*, and *allopathic*.

Antipathia.—1st. The *antipathic* method consists in employing medicines which produce effects of an opposite nature to the symptoms of the disease, and the axiom adopted is *contraria contrariis opponenda*; hence the origin of the term *antipathic*—from αντι, opposite, and παθος, a disease. We may regard Hippocrates as the founder of this doctrine, for in one of his aphorisms he says, "all diseases which proceed from repletion are cured by evacuation, and those which proceed from evacuation are cured by repletion: and so in the rest, contraries are the remedies of contraries."

We adopt this practice when we employ purgatives to relieve constipation, depletion to counteract plethora, cold to alleviate the effects of scalds, or narcotics to diminish preternatural sensibility. Hahnemann objects to the practice, on the ground that it is only palliative. Whenever we attempt to influence the system by any external agent, say his followers, a tendency to reaction is set up: so that if the primary effects of an antipathic medicine be of an opposite kind to the phenomena of a disease, the secondary effects, or those which arise from the reaction of the system, are similar to the morbid phenomena, and, therefore, only produce an aggravation of the original malady. To all theoretical objections of the kind here offered, our only answer can be an appeal to experience; which, I think, warrants us in frequently ascribing a beneficial influence to this mode of treatment.

Homœopathia.—2dly. The *homœopathic method* was founded by Hahnemann, a German physician, towards the latter end of the last century; but the first systematic account of it was published in 1810, under the title of *Organon der Rationellen Heilkunde.* The true mode of curing a disease mildly, promptly, certainly, and permanently, says Hahnemann, is, in all cases, to select a medicine capable of producing an affection similar to the one to be removed; and he therefore adopts as his axiom, "*similia similibus curantur;*" from whence he has taken the designation of his doctrine—ὅμοιος, like, or similar, and παθος, a disease. The following are some of the statements made by him, in support of his opinion:—The author of the fifth book, Ἐπιδημιῶν, attributed to Hippocrates, speaks of a patient attacked by the most violent cholera, and who was cured solely by white hellebore; which, according to the observations of Forestus, Ledelius, Reimann, and many others, produces of itself a kind of cholera. The English sweating sickness of 1485, which was so fatal that it killed 99 out of 100 affected with it, could only be cured by the use of sudorifics. Diarrhœa is sometimes cured by purgatives. Tobacco, which causes giddiness, nausea, &c. has been found to relieve these affections. Colchicum cures dropsy, because it diminishes the secretion of urine, and causes asthma in consequence of exciting dyspnœa. Belladonna produces difficult respiration, burning thirst, a sense of choking, together with a horror of liquids when brought near the patient; a flushed countenance, eyes fixed and sparkling, and an eager desire to snap at the by-standers; in short, a perfect image of that sort of hydrophobia which Theodore de Mayerne, Münch, Buchholz, and Neimicke, assert they have completely cured by the use of this plant. When, indeed, belladonna fails (which it often does) to cure canine madness, it is attributable, according to Hahnemann, either to the remedy having been given in too large doses, or to some variation in the symptoms of the particular case, which required a different specific—perhaps hyoscyamus, or stramonium. Drs. Hartlaub and Trinks have subsequently added another homœopathic remedy for hydrophobia—namely, cantharides. The best application to frost-bitten parts is cold, either by the use of some freezing mixture or by rubbing the part with snow. In burns or scalds the best means of relief are the exposure of the part to heat, or the application of heated spirit of wine or oil of turpentine.

Such, then, are a few of the statements made by Hahnemann in support of his doctrine—a doctrine which has been facetiously termed, "the art of curing, founded on resemblances; or the letting one nail drive out another." When we bear in mind the great spread of this doctrine on the continent (in the "*Pharmacopœia Homœopathica,*" published in 1834, above three hundred physicians and surgeons are enumerated who practise it); when we find that the French Minister of Public Instruction this year called on the *Académie de Médecine* to report on it; when we see several works (professional and non-professional) published in our own country on the subject, and find it stated in the public prints that an illustrious lady had submitted herself to the practice of an homœopatheker, we cannot refuse to examine it in a serious and attentive manner.

You will naturally be desirous of knowing on what principle, if he have any, Hahnemann explains the efficacy of his medicines. In his opinion, however, it is of little importance to endeavour to elucidate in a scientific manner how the homœopathic remedy effects a cure; but he offers the following as a probable explanation. The medicine sets up in the suffering part of the organism an artificial but somewhat stronger disease, which, on account of its great similarity and preponderating influence, takes the place of the former; and the organism from that time forth is affected only by the artificial complaint. This, from the minute dose of the medicine used, soon subsides, and leaves the patient altogether free from disease; that is to say, permanently cured. As the secondary effects of medicines are always injurious, it is very necessary to use no larger doses than are absolutely requisite, more especially as the effects do not decrease in proportion to the diminution of the dose. Thus eight drops of a medicinal tincture do not produce four times the

effect of two drops, but only twice: hence he uses exceedingly small doses of medicines. Proceeding gradually in his reductions he has brought his doses down to an exiguity before unheard of, and seemingly incredible. The millionth part of a grain of many substances is an ordinary dose; but the reduction proceeds to a billionth, a trillionth, nay, to the decillionth of a grain, and the whole materia medica may be carried in the waistcoat pocket.

The following is the method of obtaining these small doses. Suppose the substance to be a solid; reduce it to powder, and mix one grain of it with ninety-nine grains of sugar of milk: this constitutes the *first attenuation*. To obtain the *second attenuation*, mix one grain of the first attenuation with a hundred grains of sugar of milk. The *third attenuation* is procured by mixing one grain of the second attenuation with ninety-nine grains of sugar of milk. In this way he proceeds until he arrives at the *thirtieth attenuation*. The following table will shew the strength of the different attenuations, with the signs he employs to distinguish them:—

Signs.		Strength of one grain.
1.	First attenuation	One hundredth part of a grain.
2.	Second	One thousandth.
I.	Third	One millionth.
II.	Sixth	One billionth.
III.	Ninth	One trillionth.
IV.	Twelfth	One quadrillionth.
V.	Fifteenth	One quintillionth.
VI.	Eighteenth	One sextillionth.
VII.	Twenty-first	One septillionth.
VIII.	Twenty-fourth	One octillionth.
IX.	Tenty-seventh	One nonillionth.
X.	Thirtieth	One decillionth.

Here is a tabular view of the doses of some substances employed by the homœopathekers:—

Charcoal, one or two decillionths of a grain.
Chamomile, two quadrillionths of a grain.
Nutmeg, two millionths of a grain.
Tartar emetic, two billionths of a grain.
Opium, two decillionths of a drop of a spirituous solution.
Arsenious acid, one or two decillionths of a grain.
Ipecacuanha, two or three millionths of a grain.

These doses are given in pills (*globuli*), each about the size of a poppy-seed. You will, perhaps, hardly believe that it is gravely asserted, the length of time a powder is rubbed, or the number of shakes we give to a mixture, influences the action on the body!! In mixing a powder with sugar, the exact period we are to rub is therefore laid down: in dissolving a solid in water, we are told to move the phial "*circa axin suam*," and at each attenuation to shake it *twice*—"*bis, brachio quidem bis moto, concute.*" But enough of such absurdities.

The principal facts to be urged against this doctrine may be reduced to four heads:—

1st, Some of our best and most certain medicines cannot be regarded as homœopathic: thus sulphur is incapable of producing scabies, though Hahnemann asserts it produces an eruption analogous to it. Andral took quinia in the requisite quantity, but without acquiring intermittent fever; yet no person can doubt the fact of the great benefit frequently derived from the employment of this agent; the paroxysms cease, and the patient seems cured. "But," says Hahnemann, "are the poor patients really cured in these cases?" All that can be said is, that they seem to be so; but it would appear, according to this homœopatheker, that our patients do not know when they are well. We are also told, that whenever an intermittent resembles the effects of cinchona, then, and not till then, can we expect a cure. I am afraid if this were true very few agues could be cured. Acids and vegetable diet cure scurvy, but I never heard of these means causing any disease analogous to it.

2dly, In many cases homœopathic remedies would only increase the original disease. Only contemplate the evils likely to arise from the exhibition of acrid substances in gastritis, or of cantharides in inflammation of the bladder, or of mercury in spontaneous salivation!

3dly, The doses in which these agents are exhibited are so exceedingly small, that it is difficult to believe they can produce any effect on the system, and, therefore, we may infer that the supposed homœopathic cures are referrible to a natural and spontaneous cure. What effect can be expected from one or two decillionth parts of a drop of laudanum? Hahnemann says it is foolish to doubt the possibility of that which really occurs; and adds, that the sceptics do not consider the rubbing and shaking bestowed upon the homœopathic preparation, by which it acquires a wonderful development of power!

4thly, Homœopathia has been fairly put to the test of experiment by some of the members of the *Académie de Médecine*, and the result was a failure. Andral tried the system on 130 or 140 patients, in the presence of the homœopathists themselves, adopting every requisite care and precaution, yet in not one instance was he successful.

The whole doctrine of homœopathia seems to me to be founded in error (or something worse), and to arise from mistaking the *post hoc* for the *propter hoc*. It is a happy illustration of the amusing story told in the *Poggiana*, of a countryman who bought six pills of a quack, which were to enable him to discover his lost ass. The pills beginning to operate on his road home, obliged him to retire into a wood, where he found his ass; in consequence of which the clown soon spread a report of the wonderful success of the empiric, who in consequence, no doubt, reaped an ample reward from the proprietors of strayed cattle.

Allopathia.—3dly, The *allopathic method* consists in employing medicines which give rise to phenomena altogether different, or foreign (neither similar nor exactly opposite), to those of the disease: hence this plan of treatment has been denominated *allopathia*, from ἄλλος *another*, and παθος *a disease*. As it involves the doctrine of *counter-irritation*, I shall defer it until our next lecture.

A PAPER

ON THE

APPLICATION OF COMPARATIVE ANATOMY TO HUMAN EMBRYOLOGY,

Read before the Junior Physical Society, Guy's Hospital, May 2d, 1835.

By John Anderson, M.E.S.
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[Concluded from p. 79.]

We now proceed to the Mollusca, in whose skeleton not a particle of phosphate of lime is discoverable. With regard to the Acephalous Mollusca, some, as the Tunicata, are protected only by an elastic cartilaginous tunic, varying in form and consistence, and having a respiratory and an anal aperture; others, as the Conchifera, are protected by shells (generally bivalve), composed principally of animal matter and carbonate of lime, secreted by the mantle, a thin tunic covering the body, connected by teeth and ligaments, and having on their internal surface one or two muscular impressions. These animals have a mouth always edentate, but furnished in the Conchifera with tentacula, leading by a short œsophagus to the first stomach, near the pyloric orifice of which the ducts of the surrounding liver open: the length of the intestines is very various. Their nervous system consists (according to Cuvier*) of one ganglion situated on the œsophagus, and generally two further down; from these nerves are given off, the distribution of which is not well known. The sanguiferous system in the Tunicata is simple, the pulsating heart consisting of a mere enlargement of the arterial trunk; but in the Conchifera it is more perfect, more complex. We find a heart, consisting of a ventricle, receiving the aerated blood from a thin muscular contractile auricle, and propelling it by two large aortal trunks through the body. Here, then, we find the development of an auricle; rendered necessary, indeed, to prevent the stoppage of the blood in the venous trunks by the contractions of the ventricle, and to allow a sufficient accumulation of blood during those contractions. The same we may presume to occur in the human fœtus; the previously simple cavity of the heart, equally as in these Conchifera subject to necessity and regulation, becomes now divided—a distinct auricle and ventricle become now demonstrable†. The respiratory organs are in the form of branchiæ, lamellated, tufted, or pectinated, through which rush in currents of water, excited by the action of vibratile ciliæ; these, again, pass out by the anal orifice; thus assisting in the expulsion of extraneous discharges, aerating the blood, and bringing food for the nourishment of the animal.

Passing over the Gasteropoda and Pteropoda‡, we come to the Cephalopoda, the highest of the invertebrate animals. In this class we find both an internal and an external skeleton; in some the shells appear in the form of cones, varying in texture and form, and in the number, communication, and division, of the internal chambers; in others, again, the shell is thin and horny, and internal to the mantle: this latter circumstance is interesting, as exhibiting the rudimentary development of the internal skeleton of the vertebrata: here, also, a rudimentary cranial cavity exists. These animals are carnivorous, and have a mouth armed with two stout horny jaws; the œsophagus swells into a crop, and then communicates with a fleshy gizzard, to which succeeds a third membranous and spiral stomach. The intestine is simple, short, and terminates with a valved orifice. Here is a great advancement in the perfection of the digestive apparatus, differing in form from that in man, but presenting a rudimentary con-

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* Règne Animal, translated by Griffiths.

† I would rather correct this observation, by saying that an auricle becomes developed; the division of a cavity generally takes place in the formation of the ventricles.

‡ These classes have been fully treated of in my recent investigations.

dition of what we shall find in birds. Their œsophageal ganglia, acquiring the form of a lobed brain inclosed in a more or less perfect cranial cavity, give off filaments to the eye, and two large nervous cords passing backwards; not yet, however, inclosed in a cartilaginous tube. In the abdominal cavity are seen sympathetic ganglia.

The heart consists of an auricle, into which the vena cava pours its contents; this auricle propels the blood into the laminated and pectinated branchiæ, which is then conveyed by branchial veins to a fleshy ventricle, whence it is propelled into all parts of the body. Respiration is effected by the water which flows in by the two sides of the mantle.

We now pass to the Vertebrata, and to them we have been led by the closest analogies; in fact, nature's ascent is by easy gradations, and is obedient to fixed laws. We shall find organization and structural development in them, as in the human fœtus, rising by rapid paths towards perfection, the analogies becoming more and more apparent and important. In the lowest, or cartilaginous fishes, we find traces of an external skeleton; for their bodies are covered with dense white plates, composed of phosphate of lime, a soft, almost gelatinous, vertebral column, and cranium, only being developed internally. This soft flexible state of bone bears an important analogy to the skeleton of the human embryo; there we know that shortly after conception the whole of the osseous system, from being originally in a fluid state, forms a transparent gelatinous mass, which changes into cartilage at different intervals in different bones, the cranium and vertebral column, as in the cartilaginous, and the ribs, as in the osseous fishes, being among the first in which this change takes place. In a fœtus of ten weeks I found the ribs ossified; in one of five months, the cranium the thickness of stiff paper. The osseous fishes have an articulated vertebral column, composed of a denser material, each vertebra being united by concave surfaces filled with a soft substance, and communicating with each other. Many of them have long spinous processes, from which are developed rays of a hard or soft structure, which support the various forms of fins: from the transverse processes are developed ribs. There are also cranial vertebræ (a continuation of the vertebral column to the cartilage of the nose), the first of which is large, and forms, indeed, a portion of the occipital bone*. The cranium, generally of a large size, varies much in form, and consists (according to Cuvier) of twenty-six bones variously united and developed, and formed by the cranial vertebræ: there are teeth, very various as to form and situation.

Here, then, we are advanced a step higher in osteology; earthy matter has become deposited; true bone is formed. The consideration of the number of pieces of bone composing the cranium is interesting, from its analogy to what takes place in the fœtal skull: there, as in the cartilaginous fishes, it is at first a uniform, transparent, cartilaginous case, without sutures. Progressing in development, as in the osseous fishes, it is composed of numerous pieces, the result of ossific deposit shooting like radii from various centres, and forming diaphanous laminæ, with but very little diploë. Again, the anterior extremities are those most perfectly developed in fishes; and this corresponds with their early evolution in the human fœtus, as already described. We have seen, also, that the vertebral column was the first formed part of the skeleton; and this, equally in man as in the cold-blooded Vertebrata, may be regarded as the source and origin of all the other pieces of the skeleton*. The bones of the internal ear are well developed in fishes; and this corresponds with their well-known early formation in the human embryo.

The digestive apparatus in fishes consists of a wide short œsophagus, leading to a capacious stomach, frequently more or less of a spherical form. In the *scomber scombrus* (mackarel) I found it long, and gradually tapering. The pyloric extremity is variously situated; beyond it are the orifices of the hepatic and pancreatic

* This is a subject concerning which I have collected some very interesting particulars, principally from the Anatomie Philosophique of Carus. The vertebral column, strictly speaking, extends from the extreme point of the nose to the tip of the coccyx; the whole skull consists of but six vertebræ, with their arches more or less developed, each element of which is formed according to strict geometrical principles. To give a momentary insight into the assertions just made, I might observe that the basilar portion of the occipital bone forms the body of the first cranial vertebra, the posterior and anterior portions of the body of the sphenoid, the bodies of the second and third, the vomer the body of the fourth. The fifth and sixth have not their bodies developed, but their arches are formed by the cartilages of the nose. In a cold-blooded vertebrate animal, or in a very young quadruped, these parts are permanently detached. I have in my museum the young and separated skull of a horse, where these elements can easily be traced.

* This is a bold assertion, and one which may appear at first sight unfounded; but it is impossible to read Carus without being convinced of its truth. The ribs, for instance, are nothing more than protovertebral arches, to which the segments of the Articulata bear the greatest analogy. The extremities are only radiating tritovertebræ, given off with mathematical accuracy from the deutovertebral arches (the canal for the spinal marrow.)

glands, which latter consist of pyloric cæca, very various as to number and form. I injected them in the mackarel, and found them very numerous, of a lengthened tapering form, and of exceedingly ready communication with the alimentary tube. The intestinal canal varies in length, and terminates in a cloaca; there is but little distinction into large and small intestines, and no true cæcum. Their nervous organs Cuvier describes as consisting of external senses, of a central medullary apparatus, and of nerves which establish their communication. In the lowest cartilaginous fishes are two slender nervous cords, extending along the middle of the back. In order to illustrate accurately the brain of the osseous fishes, I dissected and examined it in three species*. In all it was extremely small, and did not fill the cranial cavity; it consisted of cerebral hemispheres, anterior to which were the olfactory tubercles; posteriorly were situated the optic lobes: these six portions I found particularly well marked in the brain of the eel; the olfactory tubercles were rather more than usually complicated. Here we shall demonstrate many beautiful points of analogy to the progressive development of the human fœtal brain; there, already have we traced out the primary deposition of medullary matter, in the form of a dorsal nervous streak, and have shewn its analogue in the Entozoa and Chondropterygious fishes: progressing, we found a medullary mass observable in the cranial cavity; a rudimentary brain, indeed, analogous to the supra œsophageal ganglia of insects, and, as in them, a commencing division into hemispheres. We shall now find how closely this brain approximates in progress of development to the fishes, to which latter class the Cephalapods themselves have, in their nervous organization, gradually advanced. The cerebral hemispheres of the previously mentioned brain of fishes were small in size, smooth and solid within. In the *gadus morhua* they were situated below the optic lobes, inferior to which, in the median line, was situated a small round body, having a central depression. In the fœtus of five months I found the hemispheres destitute of convolutions; and these in an earlier state contain no cavities, the ventricles being first developed in the corpora quadrigemina, which I found of a comparatively large size in a fœtus of two months: in fishes the optic lobes, their analogues, are largely developed, and contain a cavity where, in the mackarel, a vessel was seen beautifully ramifying. In the fœtus of eleven weeks the olfactory nerves and tubercles are perceptible; and in the osseous fishes these parts are present, varying much in form, size, and position. The cerebellum, destitute of convolutions, is but imperfectly developed in fishes, and in some is almost wanting, though its relative size is somewhat considerable: it consisted in the common cod-fish of an undivided tongue-shaped median lobe, beneath which was the fourth ventricle, communicating with a canal in the spinal marrow. This lobe had no transverse striæ on its outer surface; but in the cartilaginous fishes it is transversely laminated, and rudimentary lateral hemispheres are developed; this I found the case in examining the brain of a species of *raia*. In the very early human fœtus the cavity of the fourth ventricle (formed by the separation of the corpora restiformia), exists as an open cavity, communicating with a canal extending along the spine. Between the seventh and eighth week the cerebellum is perceptible, smooth, and without furrows, its median portion, as in the fishes, being developed before the lateral hemispheres, and the subsequent laminæ on its outer surface being, as in them, demonstrable before the convolutions in the cerebral hemispheres. The spinal cord in fishes is comparatively large (in the cod-fish its transverse diameter nearly equalled that of the brain at its widest part); it is of pretty equal diameter in its length, and from it spinal nerves arise by two sets of roots; this corresponds with the early development of these parts in the human embryo, and the want of the middle and posterior enlargements of the spinal cord, from the extremities not yet being developed. The medulla oblongata is large in fishes, and gives origin to most of the cranial nerves, of which there are eight pair; on it are already perceptible the corpora pyramidalia, olivaria, and restiformia; of these, the former are demonstrable in the human fœtus at the fourth month, the latter as early as the eleventh week.

Fish have a complete circulation, also a renal, portal, and hepatic one, thus advancing a step higher in the development of the vascular system; they have a heart (as was exemplified in the genera *scomber*, *salmo*, *raia*), consisting of an auricle and ventricle, from which latter arises an artery conveying the blood (brought to the auricle by the venæ cavæ) to the branchiæ, which being oxygenated, is returned for distribution by a large branchial vein, the analogue of the aorta in other animals. We thus see that their circulation has an appropriate character, and itself represents the embryo condition of the Batrachian reptiles.

* *Gadus morhua*, *scomber scombrus*, *anguilla* —— ? I have since dissected the brain of the *perca fluvialis*, and have succeeded in tracing out the cranial nerves, with their origins.

Respiration is effected by means of tufted or laminated branchiæ (varying in number and form), situated on each side of the neck, and supported by the os hyoides; there is also in most fish an air-bag, or natatory bladder, of various forms, situated in the median line, communicating with the alimentary canal, and assisting in respiration.

The Amphibia approach, in the form of their skeleton, to the fishes, but the texture of their bones is more compact; and in the cranium there is a tendency to anchylosis and union, a prelude, indeed, (Dr. Grant observes), to the sutures, and reminding us of the increased development of the ossific radii in the bones of the human fœtal cranium, and their consequent nearer approximation. In the higher Amphibia, as in the genera *rana* and *bufo*, extremities and a rudimentary sacrum are developed; there are no ribs, but a circular arch, formed of a sternum in front and two scapulæ posteriorly, separated by small clavicles, support the anterior extremities. In a similar manner are they supported in the human fœtus, and the clavicles and scapulæ are of early formation. Amongst the true Reptilia, the *ophidia*, or serpents, have a pliant though secure vertebral column, from which are developed ribs. The *sauria*, or lizards, have a complex sternum and scapular apparatus, a pelvic arch, and anterior and posterior extremities. The *chelonia*, or tortoises, are shielded in a strong, almost entire, bony case; the teeth are replaced by horny mandibles.

As regards the digestive apparatus, the amphibia in their tadpole state have a large capacious alimentary tube, which I found in the tadpole of the frog to measure four inches and a half in length; this is, however, lessened in size on their metamorphosis. The serpents have a large dilatable stomach and œsophagus, a small cæcum, and convoluted intestines, terminating in a cloaca. In a British species of *coluber*, I found the alimentary canal upwards of three feet in length, and the stomach long and tapering. The lizards have a short simple alimentary canal, as was exemplified in the *lacerta agilis*. The tortoises have a tolerably large cæcum; the distinction into large and small intestines is pretty obvious, and internal longitudinal folds are developed. Our last notice of the alimentary canal in the human fœtus demonstrated it as tolerably perfect with regard to the œsophagus and stomach, and extending from mouth to anus; and now, progressing in development, as in these Reptilia, a marked division into large and small intestines takes place, a rudimentary cæcum becomes developed, and rugæ, the prelude to valvulæ conniventes, are formed.

The nervous system of the Amphibia changes with their metamorphosis; in their tadpole state it passes down through the numerously prolonged coccygeal vertebræ, and presents much the same simple character as in the highest fishes. I found the brain of the tadpole quite a miniature representation of that of an eel; but when their change takes place, the cerebral hemispheres become enlarged, and the optic lobes diminished; the spinal marrow appears to shrink upwards, from the coccygeal vertebræ becoming filled up, and enlarges opposite where the anterior and posterior extremities are given off*. This is precisely similar to what occurs in the human embryo, where a similar retraction and enlargement of the spinal marrow take place, consequent on similar structural and organic changes, as also a corresponding enlargement of the hemispheres, and consequent relative diminution of the corpora quadrigemina. In the other Reptilia, the cerebral hemispheres have still more increased in size; the olfactory tubercles and optic lobes have diminished, but the whole brain is remarkably small: these characters quite agreed with the dissection of the brain of the *coluber natrix*.

The sanguiferous system of the Amphibia presents us with some interesting changes: in the tadpole state they have a heart and vascular apparatus, similar to the fishes, and, like them, they breathe by branchiæ; but as they advance in development, and finally leave the water, their air-sacs are converted into cellular lungs; the artery given off from the ventricle divides into two, by which one-half of the blood is propelled through the lungs, which, returning into a developed pulmonic auricle, and thence into the ventricle, is circulated, half arterial, half venous, through the general system. I have found the heart in the adult *triton cristatus* to present these appearances. In the other Reptilia there is still greater development, especially in the pulmonic system, for both auricles are more muscular, more distinct; they breathe by means of lungs, which are either in the form of equally or unequally developed undivided sacs, or minutely divided cellular sacs, as in the higher orders. Keeping pace with the progressive development of the vascular system, we may presume that in the human fœtal heart, as in these reptilia, a pulmonic auricle now becomes demonstrable; this I found the case in a fœtus of ten weeks, the sep-

* Some of these interesting changes I have since been able to demonstrate by dissection, in the various states of the genera *rana* and ***triton***.

tum between the ventricles being incomplete; in a fœtus of five months, however, it was completely closed up. The development of the organs of respiration in the human fœtus does not seem well understood, and I touch but lightly on the subject. In the embryo state there are simple pulmonary sacs, opening externally by three branchial apertures on each side of the neck, which become subsequently closed up, though Ascherson relates cases where they have remained open, and formed congenital fistulæ *; their use does not appear obvious. We may conceive these air-sacs at first simple, afterwards internally divided into cells, and in progress of development to become as minutely cellular as we know them to be at the termination of fœtal existence. In examining a fœtus of nine weeks, I found what appeared to be an aperture in the larynx opening externally, and one or two imperfect ones apparently, on the left side; the lungs appeared quite solid. In a fœtus of twelve weeks, I found on one side three minute apertures; in six other human fœtuses of various ages I could distinguish no orifices whatever; neither could I in the fœtus of *erinaceus europæus, mus musculus,* and *felis catus.*

In remarking upon the skeleton of birds, the texture and composition of bone have here arrived at their maximum. The earthy deposit has greatly increased, and the bones are compact, brittle, and hollow. The structure of the skeleton is very uniform, and gradually approximates to that perfection of development exhibited in ourselves; the pelvis is lengthened and widened; the vertebræ are numerous; the sternum and coracoid bones are large, and vary greatly in form; the skull is lengthened and tapering; the orbit large; the jaws are invested with a notched or jagged horny substance.

The digestive apparatus (excepting in the carnivorous birds) is very complicated. In the genera *falco* and *strix* I found the stomach simple: in *columba* it was composed of three parts—an immense crop formed by two lateral enlargements of the œsophagus, a membranous stomach with internal glands, and a powerfully muscular gizzard. The intestinal canal was of some length (50 inches); true valvulæ conniventes, as in the human fœtus, were not yet developed; there were two cæca coli, and the rectum terminated in a cloaca, into which also opened the spermatic ducts and ureters. In birds this cloaca is the prelude to the more perfectly formed urinary bladder of the Mammalia. In the early human fœtus I have demonstrated a similar pouch or cloaca, which, in a like manner, Dr. Grant observes, "may be considered the rudiment of, and is indeed, that same part of the intestine which develops the bladder, while the rectal pouch, being prolonged posteriorly, terminates in a true and distinct anal opening." The brain of birds is proportionally large, and, as in the human fœtus, now fills the cranial cavity. The optic lobes and olfactory tubercles are diminished, from the increase in size of the smooth, still unconvoluted, cerebral hemispheres, which contain but small cavities, and in which are now perceptible the thalamus and corpus striatum, with diverging fasciculi crossing the median line, the rudiments only of the corpus callosum of the Mammalia; these parts I observed in the brain of a species of *columba.* The same occurs in the progressing human fœtal brain. The corpus callosum is of late formation (I found only the rudiments of it at the fifth month); the hemispheres are much increased, but yet without convolutions; the lateral ventricles, with their contents, are developed; and in the fœtus between the fifth and sixth month, I found present the corpus striatum and thalamus. The cerebellum, with its lateral hemispheres, is enlarged in birds; and transverse sulci, dipping into the interior, are distinctly seen on the surface. This was beautifully demonstrable in the brain of *phasianus gallus* and *columba,* and quite corresponded with the progressive development of the cerebellum in the human fœtus, where, at the fifth month, I found the lateral hemispheres formed, and a few transverse furrows presenting themselves on the external surface. In the spinal marrow of birds the middle and posterior enlargements are distinctly manifest, the reason and analogy of which are obvious: the great sympathetic is also highly developed *. As regards the heart, the ventricle being divided by an impervious septum, this organ has now arrived at its most complicated form, such as it presents in the mammalia and the human fœtus, when commencing the era of a separate existence. The respiratory organs of birds consist of fixed lungs, and of communicating air-sacs, situated in the abdomen, neck, and extremities. Similar air-cells exist between the muscles; and when we consider that the bones also are hollow,

* In a memoir lately published in Latin at Berlin, "On the Branchial or Gill-like Openings in the Neck of the Human Fœtus, as a Cause of certain Malformation." See Cuvier's Règne Animal, translated by Griffiths, vol. ii. p. 411.

* Having lately had the opportunity of dissecting the brain of some other species of birds, I have noticed, by accurate measurements, that the cerebrum and cerebellum are larger in proportion to the optic lobes in carnivorous than in graminivorous birds; in which anatomical peculiarity physiology fully bears me out.

and contain air, we see, as anatomists, how highly organized and extended is their respiratory apparatus; we observe, as physiologists, how indispensable this is to their structure and habits. A diaphragm, composed of small muscular bands, shooting upwards from the ribs over the surface of the lungs, is observable in all birds, analogous to the rudimentary development of this muscle in the human fœtus, and which at five weeks I found scarcely at all developed; at ten weeks, quite a thin membrane; at five months, a firm muscular structure.

We have now arrived at the last and the highest class of the animal kingdom—the Mammalia—at the head of which stands man, so justly styled "the lord of the creation *." In them there is a greater uniformity of plan as regards the skeleton; the bones in texture are more fibrous than in birds; there is a larger proportion of animal matter; their cavities are filled with marrow, and it is needless to say they more or less resemble the human adult bone. Perhaps the greatest osseous structural difference will be found in the *cetacea*: there the vertebral column is prolonged greatly backwards; there are no posterior, and the anterior extremities are but imperfectly developed. These animals, observes Dr. Grant, are interesting emblems of the embryo of our own species, in the texture of their bones, in their want of posterior extremities, and of cavities in the long bones of their imperfectly-developed arms. With the general osteological characters of the other orders of mammalia we are familiar. It would have been interesting, had time permitted, to have traced out the elements of the human hand and foot in the extremities of various quadrupeds, the gradually diminishing ferocious characters of their teeth up to the comparatively harmless ones of our own species, and the particular structural dependence of those teeth upon the graminivorous or carnivorous food of the animal, up to those forms indicated by the omnivorous food of man.

The digestive apparatus of the mammalia is highly developed, and presents numerous varieties intimately connected with the habits and food of the animal, and the structure of its teeth. It consists, speaking generally, of an œsophagus, leading to a variously-shaped stomach, which I have found presenting a simple (and more or less resembling the human) form in about twelve of the higher mammalia. In the *sheep* it was of a peculiarly complicated nature, consisting of four parts, and adapted to the ruminating habit of the animal. In the *pachydermata* and *marsupialia* it was internally divided. The intestinal canal (free from valvulæ conniventes, as in the human fœtus), I have invariably found short in the carnivorous, long in the herbivorous, quadrupeds. In the former the cœcum was small or wanting; in the latter, very large. The appendix vermiformis is generally present in the mammalia, and forms in the lower orders, as in the human fœtus, the rudiments of an adult cœcum. I found it well developed at five months, and the cœcum small.

The nervous system is now fast approaching to its maximum of development, making many rapid strides of advancement through the variously-organized mammalia. The great sympathetic is now fully developed; the commissures of the cerebrum and cerebellum, the corpus callosum, and pons varolii, are now demonstrable; the cerebral hemispheres become larger, and their convolutions deeper and more numerous, as we approach the human species; the cineritious matter is less abundantly proportioned to the white fibres. The corpora quadrigemina (large in the lower orders) diminish in size as we ascend the scale, and the ganglia on the symmetrical sensitive nerves become enlarged and approximated. Still, however, the lateral ventricles are smaller, and there is a less extension backwards of the cerebral hemispheres than in the human brain, where, indeed, various structural and organic differences (as we all know) exist, corresponding with the increased energy of man's intellect and the superior vigour of his mind.

The heart and vascular system are on the same general plan as in the birds; but it has here arrived at its maximum of development; and even here many interesting modifications are observable. In the herbivorous *cetacea*, the heart is of a flattened form, and presents at the apex between the ventricles a cleft appearance, produced by the drawing up of the internal septum. A similar one exists in the heart of the human embryo in the early division of these cavities; and at the termination of fœtal existence, when the various familiar changes of the vascular system take place, then does it present the same structural development of the higher mammiferous quadrupeds.

The lungs of quadrupeds are highly cellular, and respiration is more immediately confined to them. The muscular partition formed by the diaphragm is here completed, as in the advanced human fœtus, in which a similarly cellular lung

* The mammalia here are treated of quite superficially, for reasons assigned at the commencement of the paper. More recently I have dissected the different organs in almost every order of this class of hot-blooded vertebrata, and have described their anatomical peculiarities.

is now ready to obey the mandates of separate existence.

Disease also is common to man and other animals, but modified in them according to their organization; thus forming another pleasing analogy. The tubercular deposit, which I before mentioned as first occurring in lepidopterous larvæ, passes through its phases of development in the scale of animal life, until it arrives at its most complicated form in man. According to Dr. Reynaud*, almost all the Quadrumana brought to this country die tuberculous; and I have myself had an opportuity of examining a species of *simia* in which this was the case †.

Thus, then, in conclusion, I have traced up the gradual development and perfection of the most essential organs and systems (those of generation excepted, from want of time only); and I have by that investigation illustrated their progressive evolution in the human embryo and fœtus.

In detailing these investigations, it has been my endeavour to make the subject of as much general interest as possible,—rather to state general than minute analogous anatomical facts, and to confine myself to those, in preference to building up physiological hypotheses. Many of those facts have been obtained from actual dissections, some made expressly for this paper; and I terminate with the hope that when the nature and extent of the subject are considered, the time of the society will not be grudged,—the lash of criticism will fall kindly. Those analogies that may have been the result of my own observations I offer with diffidence, and for the general imperfections and omissions I must indeed plead guilty; but, let me ask, have not even these investigations shewn the high interest, not to say positive importance, of comparative anatomy and physiology? Inasmuch, then, as the permanent structure of the varied kinds of lower animals has an analogous relation to man's gradual fœtal development, who shall say that comparative anatomy does not unfold to us numberless important facts? Who can say that its study is not intimately blended with our other professional duties? Let us be assured it is; and that while we are at the same time perfecting our knowledge of the human body, and thus administering to its relief in disease, we are enlarging and expanding our minds with the great truths of nature, by the combinations of which we more clearly prove the existence of a God, and redound to the glory of an all-wise Creator.

George-Street, Richmond,
Sept. 22, 1835.

* In a memoir read before the Academy of Medicine, Paris, and published in the Archives de Médicine, vol. xxv. p. 149—171, and p. 301—326.

† Since this paper was read, I have had the opportunity of dissecting the *simia satyrus*, Lin. (Orang-outang), which has recently died at the Surrey Zoological Gardens. The lungs were quite tuberculous, but the liver, spleen, and kidney, were healthy. While on this subject, I may remark, that through the liberality of our treasurer, and the kindness of Mr. Bell, the dissection of this rare animal has been entrusted to me. I have preserved every part deserving of notice, and shall not fail to take an early opportunity of making known every anatomical peculiarity connected with so interesting an animal.

ON THE CLIMATE OF MADEIRA,

AND ITS TRUE VALUE TO THE CONSUMPTIVE.

To the Editor of the Medical Gazette.

Sir,

Owing to the progress of the cholera in Italy, and the consequent quarantines, many individuals are thinking of spending the winter in Madeira. Having been in the island last winter, on account of my own health, I trouble you with a few remarks which may, perhaps, be useful to some at this moment.

The situation of the island in the midst of a very large ocean, the latitude, the position of the town of Funchal, &c. all tend to produce great uniformity of temperature and great moisture, by the common principles of meteorology. The moisture is, of course, little seen in the town itself, being kept in such perfect solution by a uniform temperature, but it is generally visible, in the shape of a fog, on the side of the mountain above, whilst its existence is certain below, not merely from theoretical reasoning, but from the observations which have been frequently made there; particularly from the full and scientific tables of Dr. Mason. I avoid, however, all theoretical reasoning, both regarding the climate and also disease, at present; wishing merely to give a few practical hints as shortly as possible. I therefore only add that the feelings of all persons when they first go, both sick and healthy, those benefited and those not, fully bear out the meteorological conclusions. Hence the climate is extremely useful to those patients who suffer from a dry irritable state of the mucous membranes, whether local or general; and in order to avoid a detail

of symptoms, I may express them all under the *strictum* of the ancients: they are well detailed in Dr. Clark's work on Climate; and he recommends Devonshire, Guernsey, Jersey, &c. for them, and it is for these symptoms that I recommend Madeira; a winter here being, in many respects, a summer in those places. On the other hand, I am very anxious to express my strong conviction that, in the opposite class of symptoms, the *laxum* of the ancients, particularly if there are present symptoms of increased mucous and still more of purulent secretion, hemoptysis, a tendency to relaxed, or what is called a slippery state of the bowels, if moist and emollient treatment have disagreed before, that in all these cases the climate will do harm in proportion to the good in the contrary: it will, indeed, often, in cases of general constitutional relaxation, do good at first, by removing some local irritation brought on by disease—as a dry cough produced by tubercles—but it will subsequently increase these by increasing the constitutional disorder which was connected with their formation. Such patients, therefore, must change their climate just as they would change their medicine, with change of symptoms.

All that Dr. Clark has said about the impropriety of sending patients in the advanced stage of disease is quite true; and I am only anxious to follow out his remarks by saying, that not only must the symptoms be slight, but they must also be of the peculiar character I have mentioned. The vague idea that Madeira has the finest climate in the world, and therefore is applicable to all cases of lung disease, is just as absurd as a similar notion about any valuable medicine; and the idea has hastened the death of many who might have long lived by means of a dryer air, slight tonics, &c. It is important also to recollect that this dryer air is not to be found in the island: the higher grounds are very generally covered with fogs in winter, and the few houses there are in summer occupied by the owners; besides that, the air does not become drier as we ascend, but only cooler; at least it remains equally near the point of saturation, which is a very important distinction. Hence the invalid must look to the town, and to it only, as his residence; and consequently eight out of ten must leave the island in summer, or their symptoms will be aggravated, as consumptive symptoms so often are in very hot weather, and as has happened to a friend of mine who remained in the island this summer. The town of Funchal (and it is the only one) was full last winter, and should the cholera send more persons there now, it is probable that great inconvenience will be felt by many. The voyage there, and still more the difficulty of getting home, is a much more serious thing to an invalid than those who have not shared in their sufferings imagine: many certain, and more contingent evils, both moral and physical, beset the patient from the moment he leaves home till he reaches it again; and these ought, as far as possible, to be explained to him. If tubercles, in however slight a degree, are known to exist, let him be told what is the utmost climate can do for him, or his distress will be very great, when, far from home, all the Utopian delusion connected with the word Madeira is torn from him. Here, however, I must strongly recommend the perusal of the first and second chapter of Dr. Clark's work, in which he insists on the care necessary in diet, clothing, medicine, &c. &c. for those who go abroad: the negligent way in which many patients are sent off, with no other advice but to give up all medicine, and trust to that ill-understood thing "climate," is, I know, the cause of irretrievable mischief. The patient very naturally persuades himself that climate will compensate for every imprudence, instead of being, at best, but the most favourable opportunity for care and good treatment. A winter in Madeira is, to all practical purposes, but a summer in Devonshire or Jersey; whilst there are many peculiarities in it which make care in all the common habits of life still more necessary. Recommending, as I do very strongly, Dr. Clark's work, to all who travel for health, he will, I hope, allow me to say that I think he has over-coloured Madeira, and I must suggest two hints to his readers: that Madeira is, with few exceptions, fit for a winter residence only, and that it does not combine the advantages of two kinds of climate, but must strictly be classed with the warm and moist; as Pau, Devonshire, &c.

I have intentionally dwelt more on the evils than good of Madeira, in this short letter; my object being to prevent

those who were going to the dry bracing air of Nice, &c. from ignorantly turning round with the idea of finding it at Madeira; this being, in fact, just the contrary. My difficulty in writing has been not to dilate, and I would gladly at this moment state the advantages of the island: this I may probably hereafter do, in conjunction with one or two friends; whilst I must for the present be content to say that the good, in well-selected cases, is proportionate to the evil in the contrary; and to shew that I am impartial, I may add that it is not improbable I may, before the winter is over, go back to the island myself.

I remain, sir,
Your obedient servant,
J. M. Calvert, M.D.

Frankfort, Oct. 6, 1835.

EXTRAORDINARY WOUND OF THE SCROTUM AND ABDOMEN,

TERMINATING FAVOURABLY.

To the Editor of the Medical Gazette.

Sir,

I am induced to send you a rough sketch of the following case of lacerated and punctured wound, in hopes (if you think it sufficiently interesting) that you will find it a corner in your valuable journal.—I am, sir,

Your obedient servant,
William Ilott.

Bromley, Kent,
Oct. 13, 1835.

On the 13th of August, I was sent for to see Thomas Langridge, a labourer, who it was reported had been wounded by a pitchfork, which had entered into his bowels. On arriving at the man's house (whither I was told he had been conveyed), and making an examination, I found a lacerated wound on the left side of the scrotum, near the septum, about an inch in length and three-quarters of an inch in depth; there was also a contusion of the abdomen, near the cartilage of the seventh or eighth rib, on the right side. The abdomen was tender, and the man complained of great exhaustion, but the pulse was firm and good; and the general complexion of the case led me to conclude that the extent of the injury had been overrated—that the instrument, in fact, had first wounded the scrotum, and then passed up inside of the clothes, but outside of the skin, as far as the point where I had observed the contused mark. The man, however, was full of the idea that the pitchfork had entered into his belly, and mentioned, as a confirmation, the extreme difficulty with which it had been extracted by the men who came to his assistance. As these persons were absent, I thought there was no criterion so good as the general complexion of the case; and this led me to a directly opposite conclusion. I may as well here give an account of the manner in which he described the accident to have occurred. He found it necessary, it appears, to descend from the top of the stack to a lower part, and to ease himself down, he threw the pitchfork first, so as to make it stand upright on the forked end, and used the handle to break the force of his descent; but his hand slipped, and the scrotum came with such force on the end of the handle, that it entered into the cavity of his abdomen.

I dressed the wound in the scrotum with strips of adhesive plaister, took sixteen ounces of blood from his arm, applied a saturnine lotion to the abdomen, and gave him an aperient dose. On the following day I found him labouring under fever, but the wound was quiet, and the abdomen free from pain or tension; I therefore, in addition to the use of the lotion, merely ordered some stronger aperient medicine, as the first dose had not moved the bowels.

On the 16th the febrile symptoms were aggravated; the abdominal pain was very acute; and there was now on the skin a line of intense inflammation, an inch and a half in breadth, extending from above the left abdominal ring obliquely across, till it terminated nearly at the same point on the opposite side, where I have described the contusion to have been in the first instance. In addition to the lotion, I ordered a combination of saline medicine, with opiates.

On the following day the symptoms were not relieved; the man complained of intense pain, and got no sleep. I could also, as I thought, feel a slight fluctuation. I increased the dose of opium, and ordered the lotion to be superseded by warm fomentations and a large linseed poultice.

I now somewhat doubted the correctness of my first opinion; I therefore removed the plaister from the wound (which required dressing), and examined the depth of it, to ascertain if it led up to or near the abdominal ring; I could, however, only pass the probe about three-quarters of an inch in that direction; and I still felt convinced that there could not have been any actual laceration of the abdominal parietes.

From this time to the 23d the symptoms of extensive suppuration became every day more distinct; the patient suffered severe pain, which required the frequent exhibition of opiates; there was a high degree of fever, and the bowels were with difficulty kept open by the constant use of purgatives. There was also sloughing of the cellular substance at the scrotal wound, which of course had made no progress towards healing.

By the 23d the abscess had attained a very great size, and the extreme tension of the skin was a source of constant distress. On the morning of the above day the skin gave way midway between the scrotum and eighth rib, and a small quantity of pus found exit, by which he was in a slight degree relieved. On my visit I gave vent, by pressure, to about half a pint of healthy-looking, but insufferably fœtid, pus. By this a good deal of relief was afforded. I took care every day to empty the abscess as nearly as possible, by pressure, and thus a great quantity of matter was discharged, so that any enlargement of the original orifice did not appear expedient. He was directed to lie chiefly on his left side, and sometimes raise his buttocks by pillows, by which the opening became, as much as possible, a dependent one, and the exit of the pus was much facilitated.

On the morning of the 31st he described himself as completely relieved (for the first time since his accident) by having the matter entirely drained from the wound; and his wife exhibited to me a piece of straw, about half an inch in length, which had that morning issued from the wound, mixed with the discharge! Here was now convincing proof that the extent of the injury had not been originally overrated by the patient; that, in fact, the handle of the fork actually had passed up from the wound of the left scrotum, near the abdominal ring, and had followed the track afterwards occupied by the abscess till it reached as high as the seventh or eighth rib on the right side! How this could have occurred with so slight an external wound does not appear so easily explained.

Thinking it right, for obvious reasons, to have a capacious aperture, I enlarged the orifice to the extent of an inch above and below. The man was much relieved thereby, and the abscess was for the first time entirely emptied; but from this time to the end of the case no more pieces of straw came away. Having seen the men who witnessed the accident, I found, on inquiry, that the pitchfork, when removed, was almost free from blood; but they added, that the testicle on that side was entirely denuded, by the very forcible retraction of the skin.

After the escape of the straw and enlargement of the opening, the man's progress towards recovery was so rapid, that in three or four days the only discharge was a little very thin pus, and the pain and constitutional irritation had quite subsided.

By the 12th of September the abdominal wound had completely healed, and that in the scrotum nearly so. There remained under the skin of the abdomen a space of about a foot in length, evidently hollow: to expedite the closing of this, I directed him to wear a broad linen belt, pretty tightly fastened, over the seat of the abscess. About a week since he called on me, when his wounds appeared entirely healed, and he expressed himself quite able to return to his occupation in a few days.

Remarks. — The most remarkable features in the above case are, first, the occurrence of so extensive and dangerous a punctured and lacerated wound without greater external marks of laceration, and without being followed by constitutional irritation of the most alarming kind; in the next place, the rapid and complete recovery of the patient; and lastly, the difficulty of reconciling the disproportion between the size of the scrotal wound and that of the instrument to which it gave admission, as well as accounting for the precise manner in which the handle penetrated to so great an extent, without wounding either the vessels and nerves connected with the

abdominal ring, or any of the other important parts near to which it must have passed. The fact of its entrance cannot, however, I think, be for an instant doubted, after the discovery of the straw midway between the scrotum and chest.

I think the case may prove both interesting and instructive to the profession; but it would be superfluous for me to point out in what manner it may be so. I therefore submit it to their perusal, through the pages of the Gazette, without further comment.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Practical Observations on Diseases of the Heart, Lungs, Stomach, Liver, &c. occasioned by Spinal Irritation: and on the Nervous System in General, as a Source of Organic Disease. Illustrated by Cases. By JOHN MARSHALL, M.D.

"THAT many in the profession will be found to cavil at the opinions I have advanced, and the facts I have adduced, I make little doubt; but, conscious as I am of the general strength of my position, and the purity of the motives which have induced me to lay the present work before the public, I shall endure with great indifference the *ephemeral* remarks and opinions of such persons."—Page 149. These are the last words of Dr. Marshall; they belong to his preface. When Gulliver fell into the milk-pot, at Brobignag, he of course thought it was very deep. There is something unpleasing in the self-importance which never appears in print or in public without expecting universal hostility.

Surely he must fancy himself an apostle, who never leaves his cell save in expectation of the fate of

> "That bless'd saint, who doffed his skin, to make
> The Smithfield rabble leap from their's with joy."

We like not the bullying—"who's afraid?"—of such defiers of criticism. It betrays a consciousness of something peccant, which, as it cannot be defended, is affected to be despised.

The author had better omitted any allusion to the purity of his motives, until they were impugned: nevertheless we challenge the *disinterestedness* of publishing 172 pages 8vo. for 6s. 6d. We would not muzzle the ox while treading out the corn, but this justice by no means extends to eating out the whole harvest. In this work there is neither novelty nor originality. A system so important, with such universal relations to others, as is the nervous system, cannot play a subordinate part in the production of disease; nor escape with impunity when the vascular system is primarily affected. All this is well known; and he must be deep in the prophets who shall make clear and perspicuous the *modes* of derangement of any part of the nervous system.

Changes in the structure can be detected only by dissection; and we go all the way with Dr. Marshall, who says—"I continued to cultivate the study with the same ardour, never in any instance neglecting a *post-mortem* examination where it was possible to obtain it. The more assiduously I pursued it, however, the more fully did I become aware of the truth, that morbid anatomy, however indispensable to the scientific practice of our profession, never could be to me, or any other physician, the *infallible* guide to diagnostics I had so fondly hoped to find it. Constantly did cases in private, as they formerly had in hospital practice, come under my observation, where, on *post-mortem* examination, no lesion or structural disease was found adequate to account for the symptoms during life; and again, not a few presented themselves where severe lesion and structural disease had actually existed, and no complaint had ever been made during life which could have led to the suspicion that such was the case." When will the day come on which it will be admitted that morbid anatomy exhibits nothing but the *exuviæ* of disease? Nor is this without interest and instruction, but assuredly not of the kind commonly supposed and anticipated. To diseases of the nervous system generally these observations apply with peculiar force; for the majority, even when fatal, after years of duration, oftener astonish by the absence of appreciable change of structure, than instruct by its presence.

On these points Dr. Marshall has some sensible and well-expressed observations; but his delusion must be great indeed if he thinks they are novel.

"Vascular energy is wholly derived from the nervous system. If by any means the nerves destined to supply any vessel, or set of vessels, with this energy, become debilitated or destroyed, then these vessels cease to be capable of duly performing the function of propelling the fluids to and from the heart; and therefore it inevitably follows that turgescence, more or less severe, takes place, and results in disease, depending upon the organ affected for its particular character. Thus a sudden temporary suspension of nervous energy in the vessels of the brain, rendering them for the moment incapable of propelling forward the blood, may occasion giddiness, dimness or distortion of vision, loss or faltering of speech, and all the other well known symptoms of approaching apoplexy: and if these symptoms are neglected, this temporary becomes a lasting loss of energy, producing either immediate death, or a total suspension of vital power in the parts dependent upon the debilitated nerves.

"It is well known that in many cases of apoplexy suddenly terminating in death, no morbid appearance whatever can be detected in the brain. From analogy, I am inclined to believe that there are cases where the interruption in the flow of nervous energy has been so sudden and complete as to arrest the circulation, without giving time for any visible turgescence to take place; and from minute and careful investigation of all that can be collected from surviving friends of the circumstances preceding such cases of sudden extinction of life, I am strongly inclined to believe that such never occur without previous warnings, however these may have been disregarded by the ignorance, heedlessness, or fool-hardiness of the individual. It must be obvious that, the vessels having once become turgid, if the nervous energy be not revived with force sufficient to restore perfectly the balance of the circulation, congestion, and in most cases effusion on the brain, must finally ensue."

The sensations and other effects of irritation at the sources of nervous supply being referred to remote rather than to contiguous parts, is, as is well known, as old as the hills.

It would be injustice towards the author to omit that he pretends to no peculiar practice in his treatment,—which is rational, and upon general principles. This disclaimer goes no farther, for he evidently exults in a superior diagnosis with less humility than beseems a philosopher. We see nothing in the details of the sixth case to make it desirable to veil in the flimsy disguise of *medical* Latin some very ordinary matters; especially as the *names* of the parties are not given. We do not charge upon him the gravest literary offence — pedantry without learning; but it cannot be forgotten that one swallow does not make a summer. We join issue with Dr. Marshall on the "purely nervous origin" of phlegmasia dolens. Its pathology is as well known as that of any disease; phlebitis of the femoral or iliac vein, sometimes complicated with similar disease in the veins of the uterus.

Medical practitioners are too frequently but sorry logicians—*e. g.* "*one fact carefully noted and faithfully reported, is of more value than whole volumes of theory and hypothesis.*" Theorising is reasoning; and of what use are facts if we reason not on them? —it is the use and application of facts; the fact on which we reason not is a *caput mortuum* in ratiocination.

We never heard a man of talent and acuteness repudiating theory: perhaps upon the principle that the handsome and well-formed never decry beauty and the graces; but we have known people as lovely as Quasimodo, or an Ogre, descant on the nothingness of "beauty," and wind up with the original apophthegm—that handsome is, that handsome does! It is worthy of remark, that the contemners of theory are persons notable for a habit of generating crude hypotheses, to which they cling as tenaciously as a pediculum, while they are as intolerant of dissent as a Jew or an inquisitor. Long will it be ere—if ever—we shall be able to contend against the fact that the detection of disease is purely an affair of *observation*, and its cure little else than one of *empiricism;* although neither is the less valuable on that account. This will give but little offence to enthusiasts, if they take care

not to confound empiricism with *quackery*. Nor will the seeker be disheartened, if he remember that diagnosis and therapeutics come not by inspiration.

Practical Anatomy of the Nerves and Vessels supplying the Head, Neck, and Chest. By Edward Cock.

Compendium of the Ligaments; illustrated by Woodcuts. By A. M'Nab, Junior.

Here are two little works which we think will be found particularly useful to the student of practical anatomy. Mr. Cock has acquitted himself in a creditable manner: the method adopted in treating his complicated and difficult subject, shows that he has profited by his experience as a teacher. Mr. M'Nab gives a large quantity of information in a condensed and very concise form. His woodcuts we do not like; but it is not of much consequence, as the book is chiefly intended for a guide in the actual business of dissection.

MEDICAL GAZETTE.

Saturday, October 24, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
Cicero.

PHRENOLOGY IN A QUANDARY.

A Scotch journal, called the *Phrenological*, has attempted to defend the awkward and indecent transaction which took place in Dublin about two months ago. It will be recollected that, at the last meeting of the British Association, a busy little crew of *phrenologists*, ever on the watch to attract some public notice, contrived to get possession of the skull of Swift. They say as much as that they came honestly by it: that it was almost thrust upon them, and that they only improved the *opportunity*.

"In making some alterations *on* the building of St. Patrick's Cathedral at Dublin, it was found necessary to shift several coffins, among which were those containing the remains of Dean Swift and Mrs. Johnson (better known as Stella). The Rev. Henry Dawson, Dean of St. Patrick's, with his usual *liberality* and *anxiety for the advancement of science*, allowed Dr. Houston to [open the coffins and] remove the skulls of *these two celebrated persons*, in order that drawings and casts of them might be made, and *that they might be submitted to phrenological examination*; under an engagement that they should be duly restored to the coffins. Mr. Combe *luckily happened* to be in Dublin at this time, attending the annual meeting of the British Association for the Advancement of Science; so that he had an *opportunity* of examining the actual skulls."

Of the usual liberality and love of science of the Rev. Mr. Dawson, we profess to know nothing; but we believe there will scarcely be two opinions entertained regarding the decency of his conduct in the occurrence just stated. He allows the remains of the most illustrious of his predecessors—of him who gave an importance which it would never have otherwise attained, to the title of Dean of St. Patrick's—to be rifled in their resting-place, and the skull to be removed for the purpose of being submitted to phrenological investigation; and this is called "liberality, and anxiety for the advancement of *science*"! When Swift contemplated his own death, and wrote that touching epitaph which used to mark the spot where his bones *were*, his warmest wish was that he might at length rest "where indignation could no longer lacerate his heart." How little could he have thought that his grave was to be ransacked by a parcel of buzzing busy-bodies, and his head abstracted for the indulgence of impertinent curiosity!

But "Mr. Combe luckily happened to be in Dublin at the time." How very lucky!—especially when we know

that Mr. Combe attended the Dublin meeting as phrenological missionary in chief, and that if a scene could not be got up in which he could take a part, phrenology must have remained in the same abeyance at Dublin this time as it did at Edinburgh last year. How fortunate, then, that he should have had the opportunity of examining the skull!

Let us see, however, what he and his companions made of the said skull. Swift was always a stumbling-block for fools, and he seems to continue to be so nearly a century after his death. He was the scourge of blockheads: oh, that he could have had any conception of the advent of phrenology! But this remained for happier and more *scientific* days.

We have no intention of tiring our readers with the detail of the skull-measuring, nor of filling our pages with the long list of faculties—35 in number—discovered to be either "large" or "very large," "full" or "rather full;" we will just give a few of them, by way of specimen, and we doubt not the reader will at once see how *exactly* they suit the character of the Dean. How could any set of men be so demented as to publish facts so damning to their doctrines?

"Amativeness, large.
Philoprogenitiveness, large.
Love of approbation, very large.
Benevolence, small.
Ideality, small.
Wit, small (skull thickened here).
Imitation, rather full.
Number, moderate.
Language, large (skull very thin).
Comparison, moderate.
Causality, moderate (skull thickened)."

Now who is there that knows any thing of the life of Swift that will not, on glancing over this table, be immediately convinced that either phrenology is *an impudent lie*, or that this could not have been Swift's skull? One or other inference is unavoidable, when the facts are tested by the rules of ordinary reason. The phrenologists, however, have not a doubt about the identity of the skull, and make the best attempt they can to remove the difficulties which it presents them with.

One worthy, upon viewing the astounding contradictions offered by the head, saw no better way of accounting for them than by cutting the Gordian knot at once: he suggested "that the *extraordinary powers of mind* which Swift exhibited on many occasions, might have arisen *from diseased activity*." Did this "learned Theban" ever take the trouble to inquire what were the powers of mind possessed by the illustrious Swift? If he did not, his ignorance and his folly are deplorable: if he did, we leave it to *phrenology* to account for *such* powers being the result of "diseased activity."

The writer of the article on this subject in the Phrenological Journal, assumes a position equally absurd; he contrives, however, to set up two or three hypotheses, to secure for his friends some plausible kind of retreat. He admits that at "the first blush," it seems irreconcileable that "the caustic and powerful Swift" should have had "a skull with small intellectual and large animal indications;" but he doubts not that, on a closer acquaintance with the case, "it will be found to harmonize in all its features with *the phrenological philosophy!*"

The first ground alleged for the apparent contradiction between the *developments* of the skull and the mental powers of him to whom it belonged, seems to be, that this was the headpiece of Swift *old* and *diseased*. The writer assumes that in the latter years of Swift's life his skull became greatly altered in shape and structure: he has even the

boldness to say, "*we know* that Swift was for years *idiotic in intellect*, and that even so early as 1734 (*eleven* years before his death) his memory became imperfect," &c.; and he infers hence, with all the *vis consequentiæ* of phrenology, that the work of disease, and the change of structure in the bones of the head, had thus early begun. *We* know that Swift was reduced for the *four* or *five* last years of his life to a state of dementia—that is to say, from about the 74th to the 78th year of his age; but we utterly deny that there is any warrant in his previous history to make it likely that such organic changes as those gratuitously assumed by our phrenological advocate could possibly have been going on. The very supposition is absurd. It takes for granted—on the mere ground that his memory was beginning to fail, and his temper to be unusually violent — that his skull from being a proper skull (phrenologically speaking) changed its shape and consistency into what the Dublin seers found to be such a difficult nut to crack. It takes for granted that at the age of 67, not only may a skull shrink and become more compact (which we do not gainsay), but that it may *grow thick* over a shrunken *organ* of the brain—the said *growth* being normal, and exhibiting no appearance (so far as we are informed) of exostosis, or disease. But we deny — we repudiate — the whole hypothesis; we reject it in the lump.

It was not till the year 1736 that Swift gave up all interference in public affairs—though, as is well known, he continued to take a warm interest in them up to the period of his expiring reason,—and it was in that very year that he produced his "Legion Club," the wittiest and most forcible satire perhaps ever written. He was then in his 69th year. Nor let it be supposed that here ended his contributions to the world of letters: his "Verses on his own Death," his inimitable "Instructions to Servants," and other most humorous shorter pieces, were produced after he had passed even his 71st or 72d year. Messrs. Phrenologists, explain away these facts if ye can: show that the bones of a man's head might be changed into a shape resembling those of a *changeling*, while the said man was not only not idiotic, but the actual author of some of the wittiest things in our language. If ye contend, that perhaps these changes occurred afterwards—that is to say, during the last three or four melancholy years of the Dean's life, we leave you in the hands of the pathologists; they will settle the matter with you.

But what shall we say to the attempt made to reconcile the bony *developments* to the character of Swift; or rather to pervert his character, so as to make it match the developments detected?

"It is curious," says the writer referred to, "to notice the coincidence between the development of many of the organs of the propensities and sentiments, and the Dean's habitual manifestations during life." Upon which he proceeds to remark, that Firmness is large, and Combativeness is large, and so forth; but not a word about most of those "faculties" in the above list, in which there was any thing but a coincidence to be noticed. It is idle to tell us that in three or four or half a dozen instances the organs were such as might be expected in a man like Dean Swift. When five or six and thirty guesses are made, it would be wonderful indeed if some of them did not happen to be right. Let us see, however, how some of the most remarkable "harmonize with the phrenological philosophy."

"Amativeness, large." Was the Dean amative at all? Hear Sir Walter Scott, to whose excellent life of Swift

we appeal the more willingly, as our phrenologist pretends to quote the work also, but, as we think, not fairly.

"His whole intercourse with Stella and Vanessa indicates the very reverse of an ardent or licentious imagination; and proves his coldness to have been constitutionally inherent, both in mind and person. * * * * The coarse images and descriptions with which Swift has dishonoured his pages, are of a nature directly opposite to the loose impurities by which the voluptuary feeds his imagination. The latter courts the seductive images of licentious pleasure; but Swift has indulged in pictures of a very different class, and dwelt on physical impurities, calculated to disgust, and not to excite the fancy."

Whether Swift was *philoprogenitive* or not, we will not venture to dispute. But "love of approbation, very large;" this, we submit, is, as an indication of the man, totally erroneous. Sir Walter Scott, in summing up the character of Swift, says, there were three peculiarities for which he was remarkable—viz. his originality, his total indifference to fame, and the distinguished excellence which marked him in all his undertakings. As a literary man,—the proudest light in which he can be viewed, if we except his patriotism, and even of that he was not vain—"the careless mode (says Sir Walter) in which Swift suffered his works to get to the public, his refusing them the credit of his name, and his renouncing all connexion with the profits of literature, indicate his disdain of the character of a professional author."

"Benevolence, *small*." Not only so, but it appears that, phrenologically speaking, Swift was totally destitute of benevolence.

"Above the frontal protuberances (in the region of Benevolence) the bone was thickened, apparently by a deposition of bony matter on its inner surface—making the inner surface at that part on both sides flat in place of concave, and smoother than the other parts; which was the more remarkable as the other portions of the skull were rather thin."

So says the report in the *Phrenological* journal. But was there ever such gross blundering? What says Sir Walter—for we will speak "by the card:"—

"The Dean's real and discriminating charity aimed at a better reward than popular applause. Even in his latter years, when habits of economy had assumed the appearance of parsimony, they could not overcome his principle of benevolence." And, again, he was "charitable and benevolent to the extreme limits of a moderate revenue."

Need we support these statements by reference to particular facts? The whole biography of this eminently benevolent man is full of instances. We cannot and need not occupy our pages with them.

Again: "Ideality, small." We presume this includes the faculty of *imagination;* and if so, every man, woman, and child (excepting the phrenologists), must see the utter folly and absurdity of such a *development.* What, the author of Gulliver, the Tale of a Tub, the Battle of the Books, and so many other productions in which the fancy and the imagination are predominant—destitute of "ideality"! He of whom it was pronounced by one of the most illustrious of his cotemporaries, "*qu'il avoit l'esprit createur*"—he deficient of "ideality!" If we be told that we mistake the meaning of *ideality*, we must only bow with submission, and acknowledge the jargon of phrenology to be quite too deep for us.

"Wit, *small*." But do we not fatigue our readers by noticing such downright nonsense? If Swift had *small* wit—who possessed *any?* Where are we to look for his superior?

"Imitation," too—"large," in this most original of all authors!

"Language," also, "large," in one who seldom used any but his vernacular: who was never noted for his philology, and who, in fine, simply used his native tongue as a mere means to an end—a tool by which some desirable object was to be effected.

The intellectual faculties, moreover, "comparison" and "causality"—"moderate," in this the finest and most subtle reasoner that ever wrote a political or controversial essay! But we have done.

We now leave the phrenologists to digest their discoveries, wishing them joy of the fortunate hit they have made in Swift: but we recommend them to try once more to reconcile the glaring contradictions between the developments which they detected and the well-known character of their subject.

After all, it may happen—we are half persuaded (from what we know of former *small mistakes*) that it has happened—that they have failed in their quarry. Let us hope that it has not been Swift's skull that has been examined. Let us fervently trust that the remains of him who so sorely felt the pangs of indignation during his lifetime, and aspired to escape them in the grave, are still undisturbed.

"Rest, rest, perturbed spirit!"

KING'S COLLEGE.

Is is not true, as some of the newspapers have stated, that Dr. Paris has been appointed to the chair of Materia Medica at this school, vacant by the resignation of Dr. Bisset Hawkins. We confess we were surprised at the announcement that the place had been vacated and disposed of in such a marvellous hurry: we are glad that nothing of the kind has been done. Publicity and fair election should attend all such appointments. Dr. Paris is said to be a candidate.

MIDDLESEX HOSPITAL.

Strangulated Inguinal Hernia — Operation — Death — Post-mortem appearances.

John Smith, æt. 45, was admitted into the Middlesex Hospital, on Tuesday evening, September 8th, with an irreducible inguinal hernia on the left side. The taxis were employed, and the man placed in a warm bath, and a large injection administered. There were no symptoms of strangulation at this time, and the swelling became much softer. At four o'clock the following morning the man began to vomit and hiccough, which increased, and at seven o'clock Mr. Tuson was sent for; he found that the hernia could not be reduced, but at the same time that it was not tense, nor was there much pain in the abdomen.

The man stated that he had had the rupture for many years; that it had only been down since the following afternoon; and that the bowels had not been relieved for the last eight days. He was ordered a large injection of house medicines and castor oil; and a consultation called upon the case at nine o'clock, at which time it was found that the bowels had been freely opened by the injection, but what had passed was considered as the contents of the large intestines. The hernia was softer, and the man could bear the hand pressed upon the abdomen without feeling pain; the vomiting and hiccough, however, continued, and he had violent cramps, so as to draw up both the legs. He stated that he was very subject to the cramp; that the last time it was down he had them just the same. It was considered advisable to wait till the usual hour of visiting the hospital, and in the meantime he was ordered one drop of castor oil. At 12 o'clock a consultation was again held upon the case, and the operation recommended, but the patient would not consent to it being performed. From this time the symptoms increased, violent pain came on in the abdomen, and the pulse began to sink.

September 11th.—The man was extremely restless; the pulse weak; constant vomiting and hiccough. At one o'clock he consented to the operation being performed; and as it appeared the only, although shadow of a chance of relief, it was performed. Mr. Tuson found, upon opening the hernial sac, the intestines in a gangrenous state; a large quantity of serum was discharged, nearly half a pint. It was thought advisable not to return the intestine, but to relieve the stricture which

was at the external abdominal ring, and to open the intestine, so as to make an artificial anus. This was done, and the contents of the intestine discharged. The part strangulated was the lower portion of the cœcum, the edges of which were left out of the wound. The patient was placed in bed, and a dose of Tr. Opii given him, with a view to produce a little rest and allay the sickness.

10 o'clock P.M.—The man was somewhat better; the sickness, perhaps, a little less; hiccough continues very troublesome; complains of pain at the pit of the stomach. A small quantity of the contents of the intestines have been discharged through the wound.

Apply a mustard poultice to the upper part of the abdomen.

℞ Sodæ Carb. ʒss.; Acid tart., ℈j.; Aq. puræ, ℥iss. Capt. in act. efferv. omni hora.

℞ Cal. gr. ij.; Opii, gr. ss. alterna quaque hora.

12th, 10 o'clock A.M.—The sickness continues, and the hiccough; a little more pain in the abdomen; little or no discharge from the wound.

℞ Haust. Efferv. cum Tinct. Opii, ♏vj. 4tis horis.

1 o'clock P.M.—Vomiting just the same, with hiccough. As there has been little discharge from the intestine, a canula was introduced into the wound, and directed towards the ileum. When the contents of that intestine were freely discharged, a canula was left in the wound, and half a seidlitz powder was directed to be given every four hours.

10 o'clock P.M.—Much the same; very restless; vomiting and hiccough; the bowels are open through the canula.

℞ Cal. gr. ij.; Opii, gr. j. statim, et rep. cras mane.

13th, 10 o'clock A.M.—Passed a very restless night; vomiting and hiccough not relieved; complains of more pain at the pit of the stomach; compares it to a hard ball; has a great desire for ginger-beer, which the nurse was ordered to let him ha e; it remained in the stomach for a short time, and relieved the hiccough a little, but only for a short time; it was ordered to be repeated, with

℞ Hyoscyami, ♏xx.

1 o'clock P.M.—The man appears much the same. The pulse is much better than it was before the operation. The canula has slipped out of the wound; it was therefore introduced again, and the contents of the intestine discharged. The pain still continues at the pit of the stomach, but the mustard poultice appears to relieve it.

A little Sp. Ammonia and Ether was ordered to be applied to the part, and covered with the poultice.

10 o'clock P.M.—Much the same; still vomiting, and complains of much inconvenience from the hiccough.

Soda water, with 20 drops of Tr. Opii was ordered.

14th, 12 o'clock.—Passed another very bad night; symptoms just the same; complains of great heat and thirst, although the skin is cold and the tongue moist. severe pain at the pit of the stomach.

Apply six leeches, and foment the part with the decoction of poppies.

℞ Aq. Menth. v. ʒiss.; Tinct. Opii, ♏v.; Magnesia, gr. x.; Magnesiæ Sulph. ℈j. 4tis horis sumend. Eggs with brandy to be freely given.

10 o'clock P.M.—Much the same; pulse a little stronger; kept some of the brandy and egg upon the stomach. Fæculent matter is still being discharged through the canula in the wound.

15th.—Passed a very bad night; restless; vomiting and hiccough; complains of much pain in the course of the inguinal canal; pulse weak and irregular. Still the same pain at the pit of the stomach.

Apply a blister to the part.

℞ Pil. Hyd. gr. iij.; Opii, gr. ss. 6tis horis sumend.

8 o'clock P.M.—Very restless. The canula has slipped out of the wound; it was introduced again, and a small quantity of fæculent matter discharged.

16th.—Passed a very restless night; pulse weak; skin cold, particularly the feet and hands; vomiting and hiccough continues.

A little lime-water was ordered, likewise the egg and brandy to be continued.

12 o'clock.—Complains of much pain in the course of the inguinal canal, and the part is much inflamed; a small quantity of the contents of the intestines pass by the wound.

The part was ordered to be fomented.

4 o'clock P.M.—He was much worse, the pulse sinking; he complains that he cannot move his arms or legs, which were found to be paralysed. The sickness, the pain, and hiccough, the same. From this time he gradually sunk, and died the fol-

lowing morning at 1 o'clock, being nearly one week after the operation.

Post-mortem examination, 12 hours after death.—Upon opening the abdomen no unusual appearances presented. The peritoneum was not inflamed; but upon tracing the small intestines at various parts of the jejunum, the vessels were in a state of congestion, almost approaching to inflammation. Upon dilating the wound it was found that the lower part of the colon, just above where the cæcum had been opening, the intestine had become in a gangrenous state, and that nearly half the circumference of the colon had sloughed to the extent of an inch and a half, and also the anterior part of the inguinal canal; at the same time there was no strangulation, the intestine being perfectly free. The brain was next examined. Upon removing the dura mater some fluid was found under the arachnoid membrane; and upon opening the lateral ventricles there was some fluid found, but not any great quantity; but at the base of the brain, and running down the spinal canal, a considerable quantity of serum was discovered, which might account for the palsy before death.

GENERAL LYING-IN HOSPITAL.

MIDWIFERY REPORTS.

By Edward Rigby, M.D. F.L.S. &c.

[Concluded from page 95.]

Attack of Milk Fever.

The next is a smart case of fever preceding the appearance of the milk, in a very irritable nervous subject:—

Oct. 25th, 1834.—Ann Barber, æt. 25, delivered of a boy; first child; first position. She is a short delicate looking woman, and during her labour never ceased from the expression of suffering by an incessant nervous whining, although the duration of her labour was but twelve hours, and its progress regular and equable. Twenty-four hours after delivery a very smart attack of fever set in, commencing with shivering, heat, and sweating; this paroxysm lasted three hours, at the end of which time she was hot and dry. Pulse very quick; eyes suffused and red; headache, tongue furred, and thirst.

℞ Hyd. Submur. gr. vj.; Pulv. Antim. gr. iv. statim. Cataplasma abdom.

10 p.m.—Bowels have been freely open; skin has a tendency to become moist.

27th, 5 a.m.—No sleep; is now in a high state of feverish excitement; the skin is burning hot; pulse bounds against the finger, 120 in a minute; great restlessness. Tongue white; thirst; much headache; bowels have been repeatedly purged; dejections watery and light-coloured.

℞ Liq. Ammon. Acet. ℥ss.; Vin. Antim. Tart. ♏xxv.; Træ. Hyosc. ʒss.; Mist. Camph. ℥vj. M. statim et secundâ quâque horâ.

Noon.—There is now a slight remission of the fever, but is very restless; occasionally dozes for a few minutes, but has not had regular sleep since her confinement. The bowels continue to be purged.

4 p.m.—The fever has increased considerably; pulse 120, hard and full; countenance very anxious; features sharp; lips pale and glazed; great headache; skin hot and dry. She was bled to ℥xvj. while reclining in bed; became faint, and the pulse diminished in force.

℞ Liq. Ammon. Acet. ʒiij.; Vin. Antim. Tart. ♏xl.; Aq. ℥ss. M. statim.

Vespere.—Secretion of milk has commenced; she had a copious bilious dejection directly after the bleeding, and again in an hour. The pulse is softer, and not so frequent; skin cooler; tongue moist.

Repeat the mixture, with Sp. Ætheris Nit. ʒss.

28th, 8 a.m.—Has had four hours' sleep, by which she is refreshed; has been perspiring. Skin now soft and cool; tongue moist; pulse slower. Her bowels have been purged every quarter of an hour whilst awake; the motions are scanty, and chiefly mucus. The mixture makes her sick.

Omit it, and let her have an enema of starch, with Træ. Opii, ♏xl.

Blood cupped and buffy.

Noon.—Bowels more quiet.

℞ Pulv. Ipecac. Comp. gr. v.; Hyd. c. Cretâ, gr. iv. tertiâ quâque horâ.

6 p.m.—Is perspiring copiously; had another dejection, for which the enema was repeated. The secretion of milk is now copious.

29th.—Has had a comfortable night; skin is moist; tongue cleaner; pulse soft; bowels not again open.

Noon.—She awoke suddenly from her sleep, much frightened about her child, which she had been dreaming was burned; is much agitated; eyes staring; feels low; mutters to herself. ℥ss. of castor oil produced a free motion.

Vespere.—℞ Camphoræ, gr. iij.; Ext. Hyosc. gr. v. Sago and wine for supper.

30th.—Has had four or five hours of comfortable and sound sleep; eat her breakfast heartily.

31st.—Continues to improve.

Peritoneal and Puerperal Fever.

There have been one or two cases of what the late Dr. Gooch would have termed peritoneal fever; and towards the end of the year, of puerperal fever, some of which recovered, but two have proved fatal. I have little or nothing satisfactory to offer on this subject; there is no lack of recorded cases, and I should be only occupying your pages to no purpose. The morbid anatomy of puerperal fever has been successfully investigated by the researches of medical men in this country and in France. I wish I could say as much in favour of its pathology. Scarcely any cases of uterine phlebitis have occurred during the last two years, although till then it was by far the most common—in fact, I might almost say the only—form of puerperal fever which existed in and about the metropolis.

The following case is interesting, because, upon the first sight, it would appear to have been an attack of phlegmasia dolens; further investigation, however, will shew that it was something very different. The case is also interesting because it occurred in a female remarkable for extreme spinal deformity.

Phlegmasia Dolens—Complicated—Morbid Anatomy.

Nov. 6th, 1834.—M. A. P. M., æt. 34, (single), delivered of a girl; first child. Labour natural and regular; first position. This woman has a very prominent lateral curvature of the spinal column, tending to the left side; it occupies principally the dorsal portion of the spine; the pelvis is ample and well formed.

On the following day (7th) had two shivering fits, followed by heat and sweating; the pulse quick; complains of some abdominal pain, and also pain of the chest. There is cough, but has no difficulty of breathing, or stitch. The pain appears to be in the parietes, from constantly lying in one position (on her right side) in consequence of the spinal curvature. A powder consisting of calomel, gr. v., pulv. antim. gr. iv. was given immediately, which acted freely on the skin towards evening. The bowels had not been open; she had headache and was restless, and therefore in the night took mist. senna, ℥iss.

8th.—Bowels have been open three times; continues to perspire; has some pain of the abdomen on pressure, but does not complain of it unless particular inquiry is made. She sleeps much. Dr. Rigby saw her, and advised a mixture of carbonate and sulphate of magnesia in peppermint water, of which she took two doses. The bowels were purged twice by this means, and it was stopped.

9th.—This morning she looks very anxious; the eyes are staring; brows corrugated; very restless; frequently changing her posture in bed. Pulse is quick and small; tongue furred and dry; skin hot but moist; bowels continue to be purged; the dejections watery, bilious, and offensive. Slight lochial discharge has commenced this morning, and the breasts have begun to secrete. The abdominal pain continues, but it is of a very slight and uncertain character.

A hot poultice was ordered to be applied, and Camphoræ, gr. iij.; Extr. Hyosc. gr. v. statim.

In four hours after taking the pill she fancied there were people about the bed. Pupils natural; pulse quick; perspiration constant; cough troublesome. In the evening the lochia ceased.

℞ Liq. Ammon. Acet. ℥iij.; Træ. Camph. Comp., Sp. Ætheris Nit. aa. ℥ss.; Aq. Anethi, ℥vj. quartâ quâque horâ. The pill to be repeated at night. The vagina to be washed out with warm water.

10th.—Has slept twice, for a couple of hours each time; looks less anxious. Pulse not so quick or small; bowels still relaxed. She talks foolishly, but in a coherent manner.

Cont. Mistura, et nocte repet. Pil. Camph. et Hyosc. Sago and wine.

11th.—Appearance improved this morning; pulse quick and soft; tongue clean; perspiration free, but very fœtid. Has had one watery motion in the night.

℞ Camph. gr. ij.; Ext. Hyosc. gr. iij. secundâ quâque horâ. Beef-tea for dinner.

Vespere.—She is better.

12th.—She was much improved in her appearance last night, but has not had any sleep, and this morning has a very pale anxious countenance. Has been purged six times, and this morning has two or three times vomited glairy mucus, containing a fluid like coffee-grounds. She has now great pain in the uterus, increased by pressure. Pulse small and quick; skin warm; tongue moist, and of a natural colour. Two grains of opium were

given to check the vomiting; arrow-root and brandy. At half-past 9 she was seen by Dr. Rigby, who prescribed effervescing draughts, with five grains excess of carb. ammoniæ, to be repeated whenever the vomiting occurred. At 11 vomited again, and the draught was repeated, with the addition of ♏xxx. of tinct. opii. In half an hour afterwards she complained of sudden acute pain about the umbilicus, which has become slightly tympanitic. The parietes are so thin that the contents may be traced; the pain is increased on the slightest touch; she does not now complain of any pain of the uterus. A mustard cataplasm was applied over the painful spot for half an hour; inflammation of the skin was thus excited, and the pain removed. At 2 P.M. passed a coagulum from the uterus the size of a walnut; has vomited two or three times. She was removed to a fresh bed in a separate room, and two grains of opium were given.

4 P.M.—Stomach has been quiet for two hours; vomiting was then renewed by drinking some tea. The presence of fluids appears to excite vomiting; jelly was therefore obtained for her, and she kept it upon her stomach.

10 P.M.—Has not vomited for six hours, and is more comfortable. Took a glass of port wine, with bread sopped in it, and had sago with wine to take during the night.

13th.—Has slept well, and is much better this morning; she has lost the offensive odour; lochia are in good quantity, and of natural appearance. Pulse good; skin warm. She had a mutton chop for dinner this day; the eyes have a bilious hue; tongue rather dry; has had four watery offensive motions.

℞ Hyd. c. Cretâ, gr. iv.; Pulv. Ipecac. C. gr. x.

14th.—Has been remarkably well since last report; towards evening she became irritable about trifling matters. Had a mutton chop for her dinner, but complained of feeling exhausted after eating it, and she vomited after taking some table beer which she had begged for; no pain.

To have arrow-root for supper.

15th.—Had a very good night, but looks anxious to-day, and refused animal food. Complains of pain at the outside of the left thigh, extending from the ilium to the knee, very exactly in the course of the inguino-cutaneous nerve. It is tender to the touch. There is *no pain* on pressing the femoral vein at the groin. Towards evening the pain of the thigh became more severe, and slight swelling is now perceptible.

To be rubbed with anodyne liniment.

16th.—Has had no sleep last night; she looks extremely anxious; her eyes are sunken, face pale, and features sharp; pulse small; has again some diarrhœa; complains greatly of the pain at the outside of her thigh; it is very tender, and more swollen. The femoral vein was again carefully examined, but still there is no pain on pressing this vessel.

℞ Camph. gr. iij.; Ext. Hyosc. gr. v. statim.

At 12 she was seen by Dr. Rigby, and by his request had

Quinini Sulph. gr. j.; Acid. Sulph. Dis. ♏x. tertiâ quâque horâ. Hyd. c. Cretâ, gr. iij.; Sodæ Carb. gr. v., and nourishment.

At 3 she was seen by Dr. Ley, who from the appearance of the thigh judges it necessary to have leeches and hot fomentations applied.

The Hyd c. Cretâ to be repeated, and the other medicines to be omitted.

Sixteen leeches were applied, and followed by hot fomentations and poultices. Immediately after their application she became so low that it was necessary to administer brandy to prevent syncope. At 3 the next morning her powers had greatly failed. Brandy and laudanum were given, and repeated at intervals, with other cordials; but she sunk rapidly, and died at noon.

Post-mortem Examination.—Body much attenuated; left thigh one-third greater in circumference than the right; abdomen tympanitic, not tense; parietes very thin; the lower part of the ileum, caput coli, and arch of the colon, contain air: a streak of inflammation is delineated along the anterior surface of the colon from the centre of the arch, throughout the descending portion of this intestine, to the left iliac region; it is marked by a transverse band of capillary vessels minutely injected in the thickened peritoneum along the whole of this course. A few convolutions of the small intestines were smeared with recent lymph, and one fold was found to adhere closely to the left side of the pelvic peritoneum, at the point of reflection of the ligamentum latum uteri. A few small portions of coagulable lymph were also found loose amongst the intestines. At the posterior surface, and left side of the body of the uterus, soft lymph and pus were effused for the space of an

inch beneath the peritoneal covering of this viscus, the membrane itself being highly vascular from inflammation, but still showing the effusion through its texture; the fundus of the uterus, where it has the fallopian tube and round ligament attached, was similarly affected, though in a slighter degree; lymph and pus were effused here also. From these two points the inflammation appears to have spread to the rest of the serous membrane; from the first indicated point it has progressed along the posterior fold of the broad ligament to the surface of the rectum and colon; from the second situation the round ligament and fallopian tube have formed the continuous line of its progress. On raising the peritoneum from the iliac fossa, the cellular membrane which envelopes the round ligament where this cord is about to pass under the epigastric vessels, after quitting the peritoneal cavity, was found infiltrated and condensed with lymph and pus. The whole of this cellular membrane (which it will be borne in mind is the fascia propria of Sir A. Cooper, and which fills the *femoral* ring, and moreover forms the medium of transmission for the lympathics of the thigh) was in the same condition, densely matted by lymph, and containing pus in the interstices*. The lymphatic glands in the groin were slightly enlarged, and some serous fluid was effused into the surrounding tissue; the femoral vein and artery were free from disease; the inner coat of the former vessel, as well as the external and internal iliac veins, and vena cava, had not the slightest trace of increased vascularity or thickening; the chain of glands from the femoral ring along the course of the iliac vessels and aorta *on the left side* were enlarged, soft, and vascular; several of these lymphatic bodies contained between the layers of the meso-colon, were found enlarged, and to contain soft lymph. The uterus was of the size usually found at this period; its tissue dense; the section shows the sinuses still large; the openings on the internal surface plainly indicated by adherent coagula. The curvature of the spine is the lateral, and takes in the whole of the column, from the first dorsal to the last lumbar vertebra; the two uppermost vertebræ have a slight tendency to the right side; a sudden turn is then made to the left, and there results a very acute angle at the centre of the dorsal portion in the left side of the thorax: from this to the sacrum the column is gradually regaining its proper direction; the ribs are much distorted, and the left side of the thorax is greatly encroached upon: the pelvis ample in every direction. The post-mortem examination was made with the greatest care by my friend Mr. Nordblad, to whom I am also indebted for the report of the case. The entire absence of inflammation in the veins is an interesting fact, and shows that phlegmasia dolens may exist to a considerable extent from inflammation of the above-mentioned portion of fascia, thus rendering impervious the large lymphatic trunks which pass through it. This cannot, however, be looked upon as a pure case of phlegmasia dolens: the situation of the pain on the *outside* of the thigh, and absence of pain in the course of the femoral veins, the absence of venous inflammation upon examination after death, the peculiarly purulent sloughy state of the cellular membrane in the groin, the effusion of puriform fluid and lymph in the abdomen, the rapid sinking after the application of leeches, shows that it partook in great measure of that malignant form of puerperal fever which is accompanied with sloughing of the cellular tissue, and formation of large collections of ill-conditioned pus in different parts. From the entire absence of pain in the course of the femoral vessels, I had ventured to overlook the local affection for a time, and direct my attention entirely to the state of the general system, and rouse its powers to a better and more healthy action; how far I was right in doing so under the existing circumstances may perhaps be a question; the pain, however, had in three hours increased so much that my excellent colleague considered it necessary, at all events, to hazard local depletion, but the powers of the system proved unequal to the demand.

Double Pregnancy.

Another case of doubtful pregnancy occurred during 1834, but the error was detected before she came into the hospital: the time gradually passed away; no labour appeared, and examination was requested, to ascertain her real condition.

May 28th, 1834.—Anne Ireland, a well-formed but unhealthy-looking woman, received an in-door admission on the 10th of March, expecting soon to be confined; has gone on to menstruate regularly during the whole time she has supposed herself pregnant, and has suffered much from dyspepsia, and bearing-down pain whenever she walks: for the last three months has felt lancinating pains shooting through the groin and sacrum, coming on without any evident cause; has had difficulty in retaining her water for any length of time

* This condition of parts bore the closest analogy to the state of the cellular membrane so constantly observed in fatal cases of phlegmonoid erysipelas, or diffuse cellular inflammation.

for the last three years; much pain is produced in passing solid fæces, but liquid dejections pass easily; abdomen is swelled, especially to the right side; there seem to be irregular masses of tumor, some harder than others, arising out of the right iliac fossa, where she first observed the swelling; the whole abdomen elastic; no fluctuation, and not peculiarly painful.

On examination per vaginam the cervix uteri is about three-quarters of an inch long, rather hard, and low in the pelvis, the os uteri forming a transverse fissure; the lips are too close to admit the finger; her bowels at times have been much deranged, and occasionally she has passed a quantity of blood per rectum, without relief or diminution of size: active purging was tried for a short time without benefit; she went to the sea-side, and I lost sight of her.

This was, in all probability, a case of ovarian disease, the swelling having commenced on the right side. The uterus itself, as far as the finger could reach, being quite natural and merely prolapsed, lead one to this conclusion: the difficulty in passing solid fæces, and inability to bear much urine in the bladder, show the existence of a large and heavy mass at the brim at the pelvis; the feel of the abdomen externally confirms this. I did not see her long enough to form any decided opinion.

Pemphigus Infantilis.

During the last year a disease has prevailed, to a considerable extent, among the children born in the hospital, which is otherwise of rather rare occurrence, viz. the pemphigus infantilis, appearing on different parts of the body, particularly the extremities and neck, in minute vesicles, which gradually increase into bullæ, and which burst, leaving a raw discharging surface: the ichorous fluid which is secreted inoculates the surrounding parts, and this keeps up the disease for some time. Generally speaking, the children have recovered from it, but in two or three it has spread to a great extent, and produced such constitutional irritation as proved fatal. In the following instance it showed itself peculiarly severe, and I requested Mr. D. Dalrymple to draw up a report, in order to give a view of the general character of the disease.

"May 29th, 1834.—Mrs. Martyn's child. About four days after delivery the vesicles first made their appearance on the neck, breast, and arms, of the child, accompanied with a green slimy discharge from the bowels; the mother suffered severely from inflamed nipples, which entirely prevented her from suckling the child.

June 1st.—Thirteen days after birth the child presented the following appearance: the nose, lips, and chin, were one raw surface; the hands and arms were entirely denuded of cuticle, except just in the palms of the hands and tips of the fingers; the scrotum, inside of the thighs, nates, and back, were studded all over with vesicles: the skin of the breast and belly presented a curious appearance, from the veins being very numerous and prominent, and crossing each other in every direction; the mouth and fauces were in an aphthous state; the child dwindled to a skeleton, and capable of retaining nothing in its stomach: no topical applications seemed to produce the slightest effect: hyd. c. creta c. sodæ carb. were given internally, but remedies proved unavailing, and on the evening of the following day it died. The body was examined about twenty hours after death. The mucous membrane of the œsophagus, from the thyroid cartilage to the cardiac extremity of the stomach, was studded with aphthous spots; the stomach was in a very inflamed state, presenting patches varying in colour from bright red to dark brown, and patches also of effused lymph; the duodenum was also inflamed, but in a less degree, and presented a few specks of ulceration: the rest of the intestinal canal showed but few spots of inflammation, and the villous coat was but little altered from its natural appearance. The left lobe of the lungs was much gorged with blood, and did not crepitate freely, the rest of the viscera were in a natural condition. The head was not allowed to be opened. This disease was not confined to the children only, but some of the mothers had many spots, Mrs. Martyn in particular, and in a few days after the post-mortem examination, during which I pricked myself, several spots made their appearance on my face and chest, which were some days before they healed, and to the pain and irritation which they excited I can testify."

Morbus Cæruleus.

The following case of morbus cæruleus is interesting, because it did not depend on a previous state of the foramen ovale in the inter-auricular septum, but of the ductus arteriosus:—

Jane Stanway's child, (boy) born Oct. 23rd, 1834.—This child was a few minutes before it obtained free respiration, or contracted the muscles, but it cried and also sneezed vigorously before the funis was tied; it was laid before the fire for some time previous to washing and dressing, and appeared to have fully established the respiratory function. About four hours after birth it was observed to moan constantly, and appeared in a comatose condition; the surface especially of the face and the nails was discoloured, and the temperature diminished; the motions are

very feeble; it refuses to take the breast; occasionally the discolouration increases, accompanied with contraction of the features and an expression of increased distress; the action of the heart is very feeble; the respiration hurried; frothy mucus or saliva constantly passing from the mouth; the eye-lids closed: an injection was immediately administered, which quickly brought away a good deal of meconium, and a teaspoonful of castor oil was given, which it swallowed, as also a few teaspoonfuls of gruel, with much difficulty.

24th.—Has continued in the same condition during the night, constantly moaning feebly, lying motionless and refusing to take the breast; the bowels have been opened freely by the castor oil. It was put into a warm-bath; whilst in the water it became more discoloured, apparently accompanied with great distress; the bowels pretty freely opened; dejections pale; liver does not seem to be secreting; five drops of sp. ammon. fœtid. in gruel, to be thrown up the rectum, and to have hyd. c. creta, gr. ij.; sodæ carb. gr. j. It continued in the same condition during the day; towards the evening some slight spasmodic twitches of the trunk and extremities came on, evidently producing much distress. It was immediately put into the warm-bath for ten minutes: the body below the axillæ is now of a death-like paleness; the face, and especially the lips, blue, cold, and soft, very closely resembling the external characters of cholera asphyxia.

25th.—Is much more feeble; pulsation of the heart scarcely perceptible; inspiration at long intervals; surface cooling; 11 A.M. died.

Post-mortem Examination.—A small quantity of bloody fluid was contained in the pericardium; both auricles filled with black blood; the right cavities tensely distended; the foramen ovale perfectly closed by the valve of the interauricular septum, through which the contents of the left auricle were seen of the same hue as the venous blood. The ductus arteriosus was still a pervious canal; the umbilical vein was found contracted, and containing a small *adhering* coagulum; the ventricles natural; the ascending portion and arch of the aorta of proper dimensions and structure, but at the point where this vessel becomes the descending aorta, and precisely where it received the ductus arteriosus, the canal was not larger than the calibre of a crow's quill; the system of vena portæ and umbilical vein were perfectly normal." For the report and highly interesting dissection of this case I am indebted to my friend Mr. Nordblad.

ON THE EFFECTS OF POISONS ON THE ANIMAL SYSTEM.

A REPORT from Dr. Roupell was read at the Dublin Meeting of the British Association, the object of which was to shew the effects produced by poisons introduced into the circulatory system, and the affinity they appear to exercise for the component elements of different parts of the body. Several plates were exhibited, illustrating the results.

Plate 1 represented the stomach and intestinal canal of a dog, poisoned by *arsenic*. An ounce of the saturated solution of arsenic, made by boiling arsenious acid, and allowing it to cool, was injected into the femoral vein of a dog. In three minutes afterwards, the animal became sick, and made an attempt to vomit; his breathing also became very much hurried. In ten minutes more, great intestinal movements appeared to be going on, and the abdominal muscles were forcibly contracted; in twenty-five, vomiting took place, followed by paralysis of the hind legs; in thirty-five the animal died. The body, when examined shortly afterwards, was found rigid, the blood fluid, the lungs stuffed with mucus, but not inflamed. The peritoneum was rough, and had lost its shining appearance; the stomach presented the hour-glass contraction, and was found to contain an ounce of tough mucus. The great end was much inflamed, the lesser differed very little from its healthy state. The large intestine was free from disease, and contained solid fæcal matter. The chief alteration was in the small intestine, which was extensively inflamed, and covered with a layer of tough mucus, tinged with blood. There was no change in the mucous membrane of the trachea or bronchi; or in the lining of the heart, veins, or arteries. The chief points of interest connected with this experiment are the absence of inflammation in those parts with which the poison came in contact, and the circumstance of its being restricted, almost exclusively, to the intestinal canal.

Plate 2 represented the stomach of another dog, poisoned in the same way. The subject of this experiment was a strong animal, and the solution of arsenic which was employed had been filtered. The same quantity, however, was administered as in the former case, and the poison was injected into the femoral vein. Shortly after the operation, vomiting took place; in twelve minutes solid fæces were passed from the bowels, followed by tenesmus. In thirty-five minutes, vomiting, cramps,

and dysenteric symptoms, occurred, which continued with more or less severity, and in about two hours the animal died. The lungs were injected, but not inflamed. The stomach and intestines were universally inflamed; the former contained about four ounces of frothy mucus, the latter, throughout its entire length, a bloody secretion. The mucous membranes of the rest of the body were redder than natural, but no change could be detected in the lining membrane of the venous or arterial systems. In this instance, the longer interval between the injection of the poison and the death of the animal gave time for a greater extension of inflammation.

Several experiments were made with smaller quantities of the arsenical solution, but without any fatal result. Half an ounce, thrown into the femoral vein of a strong dog, appeared to produce but little inconvenience. Half an ounce of the *liquor hydrargyri oxymuriatis* was injected into the veins without producing any appreciable result. An ounce (which contains half a grain of the corrosive sublimate) produced dysentery and considerable distress, but not death.

The next trials were made with *tartar emetic;* the preparation employed was the vinum antimonialis. An ounce of this was thrown into the saphena vein of an active terrier. The first and almost immediate effect of this was to produce symptoms resembling intoxication,—a circumstance which may be attributed to the quantity of alcohol contained in the preparation employed. The animal was able to stand and run, but reeled about and tottered in his gait. On being visited some hours afterwards, it was found dead; from the appearance of its jaws, vomiting seemed to have taken place. The brain was found natural in appearance, and the intestinal canal presented nothing different from the normal state; the chief alteration appeared to have occurred in the stomach, which exhibited signs of intense vascularity, particularly at its greater end.

Several other experiments were made, but of less interest: when a solution of a metal in strong acid was employed for the purposes of injection, death took place rapidly, and the mucous membrane presented a marked red appearance. Dr. Roupell had been able to satisfy himself as to how far such changes in the intestinal canal are to be attributed to the compound, or to the effect of the simple acid, which in itself would coagulate the blood, or greatly predispose to that condition. The injection of a solution of *kreosote*, a substance which appears to possess the greatest power in this way, has no influence on the intestinal canal A drachm of this substance, mixed with water, produces no effect; but when injected pure, death has been the result. The appearances seen on dissection in this case, were confined to the lungs, which were black and gorged with blood that appeared to consist of minute granules, mixed with a fluid of inky blackness.

Dr. Roupell concludes, that any attempts at explaining the effects of the foregoing experiments, must, in the present state of animal chemistry, be merely conjectural. How far poisonous substances prove irritant by their chemical agency, or by exciting in certain parts the peculiar susceptibility to inflammatory action, it is not easy to determine, but it must be admitted that something more than mere contact is required in those cases where irritants applied to the surface, or thrown into the veins, provoke inflammation of the intestinal canal. Whether it be, that the system is on its guard against those substances which tend to increase the coagulation of blood, must be made a matter for future investigation; but certain it is, that substances endowed with this property seem to have a great tendency to excite the inflammatory condition. It is a curious fact also, as connected with this point, that the coagulation of the blood becomes diminished under such circumstances, or in other words, that coagulation goes on more slowly in an inflamed state of the blood*.

QUACKERY IN FRANCE.

One Gardereau, a peasant of the Indre et-Loire, having been confined to his bed for six months, was induced by a neighbour to apply to a quack of the name of Gautier. On visiting his patient, the quack doctor announced that poison had done the mischief, but that he had a cure: he required, however, 11 francs to begin with, in order to propitiate the Evil one. Next day another visit was paid, when Gautier, having examined the urine, and made sundry grimaces, mentioned a remedy which would cost 30 francs a quart. The money was paid, and the potion delivered. It consisted of a many-coloured powder dissolved in a large quantity of water; on taking which the patient expired in excruciating agonies. Gautier was prosecuted, convicted of obtaining money under false pretences, and sentenced to imprisonment for five years. He appealed to the Court of Tours, where he pleaded his own cause, and stated that he was a horse-doctor: he had found, he said, the Caryophillata in a book of veterinary prescriptions, and he thought it would be good for

* Dublin Journal of Med. Science, Sept. 1835.

Gardereau. He produced in court a book of magic, in which he pretended to be an adept. The book was entitled *Enchiridion Leonis Papæ Serenissimo Imperatori Carolo Magno in munus datum*, professing to have been written by Leo the Third in the ninth century. Many curious recipes are contained in the work—such as for winning at play, escaping the conscription, discovering a thief, &c.: but Gautier found none in it to save him from the jail. The court confirmed his sentence.—*French Papers.*

FOWLS CLOVEN ALIVE—

AN OLD REMEDY *IN EXTREMIS.*

The *Essex Herald* tells us of a deed of "superstition and cruelty" just perpetrated in its neighbourhood. "A young man, not far from this town (Chelmsford) was last week in the agonies of death, when his father was induced to try the powers of a potent spell, which he was assured would restore the dying man to health and vigour. He accordingly procured a live pigeon, split it suddenly down the middle of the body with a sharp knife, and applied the severed parts, still moving with life, to the soles of the feet of the dying patient, fully expecting to behold its instantaneous effect. The son, however, was a corpse a short time after." About two centuries ago the "superstitious and cruel" remedy here mentioned would have been looked upon as a matter of course. It was one of the many methods tried in the last illness of Prince Henry, the son of James the First. "A cock cloven by the back" was applied to the soles of the Prince's feet, with the consent and advice of the eminent medical men—among them Mayerne, Butler, and Hammond—who had charge of the case. But it does not appear that any benefit was derived from it; the royal patient died on the second day after.

WESTERN DISPENSARY.

Dr. R. B. Todd has been elected Physician to the Western Dispensary, in the room of Dr. Clendinning, Physician to the Marylebone Infirmary, resigned.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

October 22d, 1835.

William Henry Cufaude, Norwich.
Charles Kingford Vacy, Cornwall.
William Simpson Lambert, Sunk Island.
John Spurrier, Northumberland.
John Griffin, Deddington, Oxfordshire.
George Gibson, Ulverstone.
George Parker, Upton House, Gloucestershire.
John Taylerson, Whitby.
James Tunstall, London.

NEW MEDICAL WORKS.

Edwin Lee on the Medical Institutions, &c. of France, Italy, and Germany. 8vo. 8s. bds.

Earl's Treatise on the Disorders of Neat Cattle. 12mo. 5s. cloth.

On Blood-letting. By J. Wardrop, M.D. Post 8vo. 4s. bds.

Dr. Venables' Interlinear Translation of the First Ten Chapters of Gregory's Conspectus. 12mo. 4s. 6d. cloth.

A Treatise on the Liver, and on the Treatment, &c. of Hepatic Disease in India. By W. E. E. Conwell, M.R.I.A. 8vo. 14s. bds.

Compendium of the Ligaments. By A. McNab. 12mo. 3s. 6d. cloth.

Practical Observations on the Diseases of the Heart, &c. By John Marshall, M.D. 8vo. 6s. 6d. bds.

The British Dissector. Part I. By M. W. Hilles. 8vo. 7s. bds.

Elements of Bedside Medicine and General Pathology. By J. S. Thornburn, M.D. 8vo. 14s. cloth.

LITERARY INTELLIGENCE.

Dr. J. L. Bardsley is preparing for publication a second volume of his Hospital Facts and Observations.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Oct.* 20, 1835.

Abscess	1	Heart, diseased	4
Age and Debility	30	Hooping Cough	4
Apoplexy	5	Inflammation	15
Asthma	9	Bowels & Stomach	9
Childbirth	3	Brain	2
Consumption	48	Lungs and Pleura	6
Convulsions	28	Insanity	2
Croup	1	Liver, diseased	2
Dentition or Teething	9	Measles	12
Dropsy	15	Mortification	1
Dropsy on the Brain	12	Paralysis	2
Dropsy on the Chest	1	Small-Pox	24
Erysipelas	1	Sore Throat and Quinsey	1
Fever	4	Spasms	1
Fever, Scarlet	15	Unknown Causes	5
Fever, Typhus	2		
Gout	1		
Hæmorrhage	1	Stillborn	16

Decrease of Burials, as compared with the preceding week . . . 9

METEOROLOGICAL JOURNAL.

Oct. 1835.	Thermometer.		Barometer.	
Thursday . 15	from 50 to 59		30·28 to 30·31	
Friday . . 16	48	57	30·31	30·27
Saturday . 17	47	57	30·22	30·20
Sunday . . 18	44	52	30·22	30·25
Monday . . 19	28	52	30·21	30·02
Tuesday . . 20	27	50	29·87	29·74
Wednesday 21	35	48	29·74	29·72

Prevailing wind, N.E. and S.W.
Except the 19th, generally cloudy: a shower of rain on the evening of the 20th.
Rain fallen, ·175 of an inch.

Charles Henry Adams.

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SATURDAY, OCTOBER 31, 1835.

LECTURES
ON
MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

By Jon. Pereira, Esq., F.L.S.

Lecture V.

COUNTER-IRRITATION.

I propose in this lecture to examine that mode or principle of cure usually denominated *Counter-irritation*, and which consists in exciting an artificial or secondary disease, with the view of relieving a primary one. It is a method of treatment derived from observation of the influence which maladies mutually exert over each other. For example, it has been frequently noticed, if a diarrhœa come on during the progress of some internal diseases, the latter are often ameliorated, or perhaps they rapidly disappear, apparently in consequence of this secondary affection. The result of observations of this kind would naturally be the employment of alvine evacuants in other analogous cases where diarrhœa did not spontaneously take place: and this practice is frequently attended with beneficial results. The appearance of a cutaneous eruption is sometimes a signal for the disappearance of an internal affection; and *vice versâ*, the disappearance of a cutaneous disease is sometimes followed by disorder of internal organs. Here, again, we have another remedy suggested, namely, the production of an artificial disease of the skin, as by blisters, an ointment containing tartar emetic, or other irritant applications;—a suggestion the advantage of which experience has frequently verified. I might bring forward numerous other examples to prove the fact (which, however, is so well known as to require little proof), that action in one part will often cease in consequence of action taking place in another. Diseases, then, appear to have what Dr. Pring calls a *curative relation* with respect to each other; and we shall find that the greater part of our most valuable and certain remedies operate on the principle of counter-irritation; that is, they produce a secondary disease which is related to the primary one. Let us offer a few examples: — vomiting is a powerful means of relief in bubo, and also in swelled testicle. Mr. John Hunter says he has seen bubo cured by a vomit. I have frequently seen the progress of swelled testicle in gonorrhœa stopped by the exhibition of full doses of tartar emetic. Now it is very improbable that the benefit arises from the mere evacuation of the contents of the stomach. The only plausible explanation that we can offer is, that the emetic sets up a new action in the system which is incompatible with that going on in the groin or the testicle. If this notion be correct, emetics act in these cases as counter-irritants. The efficacy of purgatives, in affections of the head, is best accounted for by supposing that they act on the principle of counter-irritation. Blisters, cauteries, issues, moxa, and other remedies of this kind, are universally admitted to have a similar mode of operation.

Even the efficacy of bloodletting, in inflammatory affections, is better explained by assuming that this agent induces some new action incompatible with the morbid action, than that it is merely a debilitant. The immediate effect sometimes produced on disease, by this remedy, is so remarkable as hardly to admit of the supposition of its acting as a mere weakening agent. One

full bloodletting will sometimes put an immediate stop to ophthalmia; and I have sometimes seen, even while the blood was flowing, the vascularity of the eye diminish, and from that time the disease progressively declined. When to this fact we add that the same disease is often successfully treated by other different, and even opposite remedies, such as mercury, and local stimulant applications, we find a difficulty in explaining their common beneficial agency, except by supposing that they influence disease by some relation common to all of them. This view of the counter-irritant operation of bloodletting is supported by Dr. Clutterbuck, Dr. Pring, and others. The term *counter-irritant* is, however, objectionable, since literally it expresses that the secondary disease should be a state of irritation,—a term hardly applicable to the condition caused by bloodletting. But this, as well as other remedial agents (mental impressions, for example), agrees with the counter-irritants, commonly so called (blisters, &c.), in influencing diseases only by an indirect relation; it would be better, therefore, either to extend the meaning of the term counter-irritant, or to employ some other, such as *counter-morbific*. The older writers employed two terms, *revulsion* and *derivation*; the first was applied to those cases in which the secondary disease occurred in a part remote from the seat of the primary affection; the second was, on the contrary, confined to those instances in which the secondary was produced in the neighbourhood of the primary disease. For example, leeches or blisters applied to the feet in apoplexy were called revulsives, but the same applications to the head, in the same disease, would be derivatives. There is, however, no real distinction between them, their operation being similar, for revulsion was even in their own sense of the word only derivation at a distant part.

Topical applications are frequently counter-irritants. Thus we see stimulant washes, applied to the eye, cure ophthalmia; and they operate, apparently, by altering the morbid action, and substituting a milder and more easily cured disease for the one previously existing.

Using the term, therefore, in its most extended sense, we see our list of counter-irritants is a most extensive one. It comprehends emetics, purgatives, diffusible stimulants, mercury, blisters, cauteries, issues, setons, moxa, bloodletting (including arteriotomy, venesection, cupping, and leeches), irritating lavements, frictions, sinapisms, rubefacients, the hot and cold baths, and even mental impressions. That is, all these agents excite some action in the system which has a relation (oftentimes beneficial) with the morbid action: to use Dr. Parry's words, these agents cure disease by *conversion*.

Let us, in the next place, examine that most unsatisfactory part of our subject, the theory or hypothesis of the manner in which the mutual relations of diseased actions are effected. Dr. Parry presumes most diseases consist in local determinations of blood, and that it is a law of the human constitution that excessive morbid determination to two different parts shall not exist in the same person at the same time. Neither of these assumptions, however, is quite correct; but if both were true, they still leave untouched the question how determination of blood to one organ is cured by producing a determination to another. To account for it, some assume that the system can produce only a certain quantity of nervous energy, and that, as in every disease, there is an undue or preternatural distribution of nervous energy, so the production of an artificial disease in one part must, by consuming the nervous energy, diminish the disease in another. But the whole hypothesis is grounded on assumptions perfectly gratuitous and incapable of proof. As Dr. Pring justly observes, were this hypothesis true, it would lead us to employ not bleeding, purgatives, blisters, and all indirect remedies in hepatitis or consumption, but the exercise of the tread-mill for a few hours; so that a patient labouring under phrenitis or pneumonia should be made to walk fifteen or twenty miles a-day, by which it would be presumed so much nervous energy would be consumed in the arms and legs, that there could not possibly be any preponderance or excess in any other seat.

Let us, then, discard absurd hypotheses of this kind; and for the present be content with a knowledge of the fact that one disease, whether artificially or spontaneously generated, will often supersede another. You may, if you please, say that the mutual relation of two diseases is effected by sympathy; but recollect that this mode of expression explains nothing, and gives no information as to the mode by which the relation it effected, —for the word sympathy is applied to those relations only, the causes of which are not understood.

OF THE PARTS TO WHICH MEDICINES ARE APPLIED.

As the effects of medicines vary according to the part of the body to which these agents are applied, and as in practice we are compelled frequently to vary the locality of our application, so it is necessary for

me to examine, in a general way, the peculiarities attending each place of application.

Medicines may be applied—1stly, to the common integument or skin; 2dly, to a mucous membrane; 3dly, to a serous membrane; 4thly, to wounds, ulcers, or abscesses; and 5thly, we may inject our remedial agents into the blood-vessels.

1st. *Of medicines applied to the common integument.* — Applications are frequently made to the skin in order to excite local effects, as in the case of blisters, cataplasms, fomentations, lotions, embrocations, &c.; and occasionally to affect remote parts of the system, as by the use of mercury. Most, if not all medicines, which influence distant organs by application to the skin, do so in consequence of their absorption; and as the cuticle offers a mechanical impediment to this process, we generally either make use of friction, or remove the epidermis in order to overcome this obstruction.

There are three methods of applying medicines to the skin, namely, the *enepidermic*, the *iatraleptic*, and the *endermic*.

(*a*). The *enepidermic method* consists in applying medicines to the skin, unassisted by friction; as when we employ plaisters, poultices, lotions, fomentations, baths, &c.

Gases or vapours are sometimes applied to the skin, either as local agents, or as means of affecting the constitution. Thus, baths of sulphurous acid gas are employed in itch; chlorine gas is recommended as an application to the skin in liver complaints; vapours of various mercurial preparations have been employed to effect salivation. The vapour of hot water, holding in solution the volatile matters of vegetables, has been employed in the treatment of many diseases, under the name of medicated vapour baths; though doubtless the greatest part of their efficacy is referrible to the influence of the hot aqueous vapour.

(*b*). The *iatraleptic method* (which has been so called from ἰατρευω, to cure or heal, and αλείφω, to anoint), consists in the application of medicines to the skin, aided by friction. It has been termed the *epidermic method*, — sometimes *anatripsologia* (from ἀνατρίβω, to rub in, and λογος, a discourse), and also *espnoïc medicine*. It was employed by Hippocrates and other old writers, but fell into disuse until attention was again drawn to it by Brera, Chiarenti, Chrestien, and others. Among the substances which have been employed in this way, I may mention camphor, digitalis, squills, cantharides, sulphate of quinia, veratria, colocynth, rhubarb, opium, belladonna, mercury, chloride of gold, &c. The doses of medicines require to be considerably larger than when these substances are given by the stomach: generally two or three, often as much as ten, and in some cases even twenty times the customary dose; but no absolute rule can be laid down. The objections to this plan of treatment are the uncertainty of results, the time required to put the system under the influence of medicines, the frequently unpleasant nature of the process (as when mercurial inunctions are employed), and the local irritation sometimes produced by the friction. Notwithstanding these, however, it may be resorted to occasionally with advantage, as where the patient cannot or will not swallow, or where the alimentary canal is very irritable or insensible to the action of the medicine.

(*c*). The *endermic* or *emplastro-endermic* method has been recently proposed by Lembert and Lesieur. It consists in the application of medicinal agents, incorporated with cerate or gelatine, to the cutis vera, the epidermis being previously removed by a blister or by heat. Morphia, quinia, nux vomica, musk, digitalis, sugar of lead, and many other agents, have been tried, and found to produce the same effects as when taken into the stomach. The operation is in general very quick, and in some cases more rapid than when the same substances are swallowed. If the gastric membrane be inflamed, or if the patient cannot or will not swallow, and the case be urgent, this is an admirable method of putting the system under the influence of a medicine. When it is desirable to produce speedy vesication, we may effect our purpose either by hot water or by a heated metallic plate.

2ndly. *Medicines applied to the mucous membranes.*—We have two mucous membranes to the different parts of each of which we make application of medicines: the first is the gastro-pulmonary membrane, the second the urino genital.

(*a*). *Gastro-pulmonary membrane.*

1. Ocular mucous membrane (conjunctiva).
2. Nasal or pituitary membrane.
3. Bucco-guttural membrane.
4. Eustachian membrane.
5. Aërian or tracheo-bronchial membrane.
6. Gastro-intestinal membrane.
7. Recto-colic membrane.

(*b*). *Urino-genital membrane.*

8. Urethro-vesical membrane.
9. Vagino-uterine membrane.

1. *Ocular mucous membrane* or *conjunctiva*. —Medicines are applied to the *conjunctiva*, under the name of *collyria*, to excite local effects only, though we might employ this part for other purposes, since remote effects may be obtained by it. Thus a

drop of hydrocyanic acid applied to the conjunctiva of a dog produces immediate death.

2. *Nasal* or *pituitary membrane.*—We seldom apply medicines to the *pituitary* membrane except in local diseases, or in affections of neighbouring organs. Sometimes they are applied to irritate this part, and excite a discharge: these are then called *errhines;* but when employed to excite sneezing, as when foreign bodies are in the nasal cavities, they are termed *sternutatories* or *ptarmics.*

3. *Bucco-guttural mucous membrane.*—Very rarely medicines are applied to the *mouth* and *throat*, except for local purposes. However, it has been proposed to excite salivation by rubbing calomel into the gums. Solids used in the mouth are termed *masticatories;* liquids are called *collutoria* or *gargarismata.*

4. *Eustachian membrane.*— Aurists now and then apply washes to the *Eustachian tubes* in local affections; but the occasions for this practice are rare, and the operation difficult, except in practised hands.

5. *Aërian* or *tracheo-bronchial membrane.*—Accidental observation, as well as experiments, have shown that medicines produce very powerful effects on the membrane lining the *trachea* and *bronchial tubes.* For the most part, applications here are made use of for local purposes, as in asthma, chronic bronchitis, phthisis, &c., though occasionally to affect the brain, the blood, the heart, &c. Dr. Myddleton has advocated the inhalation of substances (as cinchona, sulphate of iron, myrrh, &c.) reduced to an impalpable powder, in pulmonary diseases. The fumes (*suffitus*) of tar, balsam, resins, and other burning bodies, have also been employed in these cases. Sir Alexander Crichton has strongly recommended tar vapour; the method of using which is the following:—The tar employed should be that used in the cordage of ships; to every pound of which half an ounce of subcarbonate of potass must be added, in order to neutralize the pyroligneous acid generally found mixed with the tar, the presence of which will necessarily excite coughing. The tar thus prepared is to be placed in a suitable vessel over a lamp, and to be kept slowly boiling in the chamber during the night as well as the day. The vessel, however, ought to be cleansed and replenished every twenty-four hours, otherwise the residuum may be burned and decomposed,—a circumstance which will occasion increased cough and oppression on the chest.

The inhalation of aqueous vapour (*halitus*), either alone or with other substances, is oftentimes useful in various affections of the lungs and of the throat, &c. The apparatus for this purpose may be that proposed by Dr. Gairdner in the nineteenth volume of the Edinburgh Medical and Surgical Journal; or Dr. Mudge's inhaler; or in the absence of these, a teapot, or basin with an inverted funnel. In many asthmatic cases the difficulty of breathing is so great, that the patient cannot close the mouth around the tube, especially if the latter be small, without exciting a sense of impending suffocation. In such instances I have found the only easy and practicable method of enabling the patient to inhale aqueous vapour, is by holding the mouth over hot water contained in a basin or tea cup. Various narcotic and emollient herbs are sometimes added to the water, but I suspect without contributing in any way to its efficacy. The vapour of hot vinegar, of sulphuric æther, of iodine, of camphor, and of other volatile bodies, is occasionally employed in pulmonary diseases. The vapour of iodine may be conveniently inhaled by means of a double-necked glass bottle (fig. 21), into which we introduce about an inch of water, to which a few drops of the tincture of iodine have been added. Through one of the necks a straight glass tube passes, and dips under the surface of the water. The other neck has a short curved glass tube passing through it, by which the patient inhales. In the absence of a double-necked bottle we may use a common wide-mouthed bottle (fig. 22), the cork of which has two per-

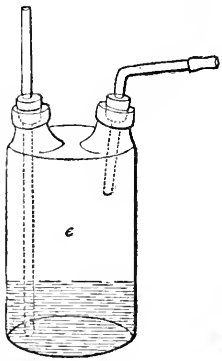

Fig. 21.

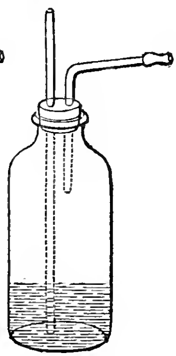

Fig. 22.

forations, through which pass the glass tubes. Chlorine gas may be inhaled in a similar manner, using a solution of the gas, or of chloride of lime, instead of the tincture of iodine. If oxygen, or nitrous oxide, be inhaled, the most easy and convenient mode of effecting it is from a bladder; but for other and more complete, though more costly methods, I must refer you to the works of the late Dr. Beddoes, and of the celebrated engineer, Mr. James Watt.

6. *Gastro-intestinal membrane.*—We em-

ploy both extremities of the *alimentary canal* for the exhibition of medicines; the upper, however, more frequently than the lower. This mode of employing medicines is called the *method by ingestion*. Of all parts of the body the gastro-intestinal surface is the most useful for the application of medicines. We presume that this arises from the great susceptibility, the active absorbing power, and the numerous relations, mechanical, functional, and sympathetic, which the stomach has with almost every part of the body. In many cases remote effects are more easily produced by this than by any other organ, as in the case of diffusible stimulants, and other agents which operate by sympathy. Medicines which act by absorption are more energetic when applied to some other parts, as the serous membranes, the bronchial membrane, the cellular tissue, &c. In some cases it is not only possible, but probable, that the stomach may digest, either partially or wholly, a medicine, and thus contribute to diminish the medicinal effect.

7. *Recto-colic membrane.* — Sometimes, though less frequently than the stomach, the *rectum* is employed for the application of medicines. It has been asserted that the general susceptibility of the rectum is only one-fifth of that of the stomach, and that medicines take five times as long to operate by the former as by the latter: hence it has been said that both the dose, and the interval between the doses, should be five times as great as when applied to the stomach. But this assertion is far from being correct universally, though it may be so occasionally. Orfila asserts that those agents operating by absorption, as opium and tobacco, are more active by the rectum than by the stomach; and he assigns as a reason the greater venous absorption of the rectum, and its less digestive power. But this statement is in direct opposition to the experience of almost every practitioner. Whenever I have had occasion to employ opium by way of enema, I always exhibit twice or three times the ordinary dose, without exciting any remarkable effects. Dr. Christison states that he has given two measured drachms of laudanum by injection, without producing more than usual somnolency, a quantity which, if Orfila's statement were correct, would probably prove fatal.

We apply medicines to the rectum sometimes with the view of alleviating disease of this or neighbouring organs (as of the uterus, bladder, prostate gland, &c.); at other times in order to irritate the rectum, and, on the principle of counter-irritation, to relieve distant parts (as the head); sometimes to produce alvine evacuations, or to dissolve hardened fæces; occasionally, also, when we are precluded from applying our remedies to the stomach, on account of their unpleasant taste and smell, the inability or indisposition of the patient to swallow, or the irritability of the stomach; and, lastly, in order to destroy the small thread-worm (*oxyuris vermicularis*).

When the substances applied to the rectum are solid, we name them *suppositories* (*suppositoria*, from *suppono*, to put under); but when they are of a fluid nature, we denominate them *clysters, lavements*, or *enemata*.

Formerly *suppositories* were usually either conical, or cylindrical like a candle, and of variable size,—sometimes one or two inches long. They are now usually made globular, and of small size. They are employed to evacuate the bowels; to irritate the rectum, and thereby to relieve affections of distant organs; but more commonly they are used as local agents in affections of the rectum, bladder, uterus, prostate gland, urethra, &c. I have frequently employed with great advantage a mixture of opium and soap, to prevent priapism during the night, in gonorrhœa.

Clysters or *lavements* require to be considered under several points of view: *first*, in reference to the material of which they are made, and which must vary with the object for which these remedies are employed; *secondly*, with respect to the quantity of liquid used, and which will depend on the age of the patient. The average quantity for an adult is about twelve or sixteen ounces; and I believe that it is rarely proper to use more than this. I am quite sure that the practice of introducing several pints of fluid into the large intestines, with the view of exciting alvine evacuations, is bad. In the first place it often provokes the contraction of the gut, by which the injection is immediately returned; and, secondly, repeated distention diminishes the susceptibility of the part, so that the ordinary accumulation of fæcal matter no longer acts as a sufficient stimulus. Mr. Salmon has related a case of this kind, where the patient had nearly lost all power of relieving the bowels, except by enemata or purgatives, and had produced dilatation of the rectum, in consequence of having been in the habit of introducing two quarts of gruel twice every day into the intestine. A newly-born infant requires about one fluid ounce; a child of one to five years, from three to four ounces; and a youth from ten to fifteen, from six to eight fluid ounces. *Thirdly*, the impulse with which the fluid is thrown up de-

serves attention. If too much force be used, the sudden dilatation of the gut may bring on spasmodic action of its lower part, by which the clyster will be returned. *Fourthly*, the instruments by which the injection is effected require notice. The common pipe and bladder are too well known to require description. I am inclined to think that the most convenient, safe, and useful apparatus, is the elastic bottle and tube. Any quantity of liquid, however small, may be thrown up with the greatest ease, and without any danger of the impulse being too great. Its application is exceedingly convenient; a lusty person, by placing one foot on a stool or chair, may easily apply it without assistance; and its price is very moderate. Another form of enema apparatus is a narrow water-proof tube, holding about a pint of liquid, about four feet long, narrower at one end, which is furnished with a common injecting pipe, and about two and a half inches in diameter at the other. The fluid being placed in the tube, the pipe is introduced into the rectum, and the apparatus held in a perpendicular direction, by which the fluid is propelled into the gut by its own gravity. This apparatus, although very simple, appears to me to be less convenient for common use than the elastic bottle, and not to be well adapted for the administration of small quantities of fluids. In the shops are sold syringes of various forms as enema apparatus, but they are more expensive than the common elastic bottle, without possessing any greater advantages or conveniences; indeed, they may become objectionable instruments, since their construction allows the liquid to be thrown up with great force.

Gaseous matters have been sometimes thrown into the rectum. Thus the injection of common air has been proposed in ileus; but I must refer you to the 16th volume of the Edinburgh Medical and Surgical Journal for further information on this point. Tobacco smoke has sometimes been employed in hernia: it is injected by a peculiarly constructed pair of bellows.

The *Urino-genital* mucous membrane, considered in reference to the application of medicines, includes the membrane lining the urethra and bladder, and that which lines the vagina and uterus.

8. *Urethro-vesical membrane.*—Applications to the *urethra* are made only for local purposes; either in a solid form, as caustic or medicated bougies, or in that of a liquid, as an injection: the latter is easily effected by a common syringe. Syringes of various kinds, for this purpose, are sold by Messrs. Maw, of Aldersgate-street.

Injections are sometimes thrown into the *bladder*, but always for local purposes. The operation is easily performed by attaching a catheter to an elastic bottle.

9. *Vagino-uterine membrane.* — Medicines are applied to the *vagina* and *uterus* to produce local effects only. Thus injections are thrown in, to relieve vaginal discharges, to excite the catamenia, &c. They are usually liquids, but the following case, told me by my friend Dr. Clutterbuck, proves that gases are sometimes employed. A lady, who had suffered a considerable time from some uterine affection, and had derived no relief from the treatment adopted, was advised to consult Dr. Rossi, an Italian physician. After he had examined the condition of the uterus, he assured her there was no organic disease, but merely a considerable degree of irritation; for which he proposed to apply carbonic acid, as a sedative agent. This was done by means of a pipe and tube, communicating with a gasometer situated in another room. The patient obtained immediate relief, and although she had been obliged to be carried to the Doctor's house on account of the pain experienced in walking, she left it in perfect ease. On her return to England she had a relapse of the complaint, and applied to Dr. Clutterbuck, to know whether she could have the same remedy applied in London, in order to save her the necessity of returning to Italy.

3dly. *Medicines applied to serous membranes.*—Irritating injections, such as wine and water, solutions of metallic salts, &c. are thrown into the cavity of the serous membrane of the testicle in hydrocele, in order to excite inflammation and the subsequent adhesion of the sides of the sac. Injections have also been made into the peritoneal sac in ascites, and in some cases with success. On this subject, however, I must refer you to the Philosophical Transactions for the year 1665.

4thly. *Medicines are applied to ulcers, wounds, and abscesses*, principally to excite local effects, and sometimes, though rarely, to produce a constitutional affection. Thus it has been proposed to apply corrosive sublimate to wounds, with the view of causing salivation.

5thly. *The injection of medicines into the veins* has been called *infusion of medicines.* The celebrated architect, Sir Christopher Wren, was the first person who undertook experimentally to determine the effects of medicines when thrown into the veins. His example was followed by Boyle, Clarke, Henshaw, Lower, and others. The partisans of this method of treatment asserted, that when medicines are administered by the stomach, their effects are much modified by the digestive process; and, therefore, by injecting our remedies at

once into the veins, we avoid this modification of their properties. But this statement is not accurate, since Drs. Christison and Coindet have shown that some substances are decomposed even in the blood, or at least they cannot be recognized in this fluid. Further, they have shown that the effects of medicines thus administered are of the same general nature as when their agents are applied to the skin or stomach; thus, tartar emetic vomits, senna purges, opium stupifies, and so on. There are, on the other hand, several objections to this practice; such as the danger of introducing air into the veins, or of throwing in too large a dose of the remedy (for a slight excess may prove fatal), or of the occurrence of phlebitis. These, then, are sufficient reasons for not resorting to this practice except on very urgent occasions; for example, to excite speedy vomiting when the patient is unable to swallow. Köhler preserved the life of a soldier, in whose throat a piece of beef tendon was sticking, by throwing a solution of six grains of tartar emetic into a vein of the arm: vomiting was induced, and the meat expelled. Knoph threw four grains of this substance, dissolved in an ounce and a half of warm water, into the veins of a man choked by a piece of beef: in one minute vomiting came on, and the beef was expelled. A similar case has been reported by Smucker.

In some obstinate and dangerous diseases this operation is sometimes admissible as a last resource; for example, in cases of poisoning, in hydrophobia, in malignant cholera, &c. As plethora appears to diminish absorption, it has been proposed to throw tepid water into the venous system in cases of narcotic poisoning, and thus to cause artificial plethora, in order to prevent the occurrence of the symptoms of poisoning by stopping absorption. Verniere found three grains of nux vomica produced no effect when applied to a wound in a dog into whose veins water had been thrown; and he asserts that by the early use of aqueous injections we may prevent the development of contagious diseases. Magendie has tried the effects of injecting tepid water into the veins in hydrophobia. The operation was first performed at the Hôtel Dieu, at Paris, in October 1823: the convulsions were stopped, but the patient died in a day or two afterwards. This operation has been several times repeated, and with the same results. In June 1832, I tried it on a patient (afflicted with this terrible disease) under the care of Mr. Bennett, of the Commercial Road. The patient was a boy, about nine years of age; he was nearly insensible at the time I performed the operation. I threw in about one quart of tepid water, without any obvious effect on the pulse: no convulsions were subsequently observed, but the patient died in a few hours. A solution of salt in water was injected into the veins in the malignant cholera, and often with apparent advantage. Purgatives, narcotics, &c. have been thrown into the veins by different physiologists, and in most cases the effects observed were of the same kind, though varying in degree, as when these substances were taken into the stomach. To this statement, however, the oils are an exception; for when injected into the veins they interrupt the circulation, and produce a kind of asphyxia.

Application of galvanism. — In the foregoing remarks on the different means of administering medicines, I have not noticed a recent proposal to introduce active medicinal agents (such as quinia and morphia) into the system by the aid of galvanism. *A priori* the idea seems feasible, and worthy of trial. We know that a variety of substances can be readily passed through animal membrane by means of the electrical agent; for example, if you place a solution of iodide of potassium in a glass tube (fig. 23), the bot-

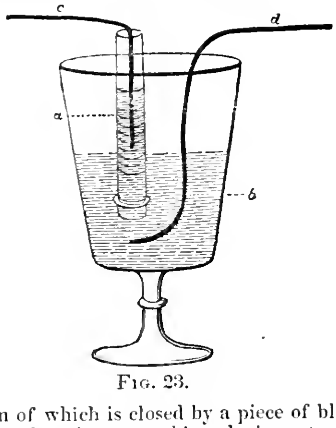

Fig. 23.

tom of which is closed by a piece of bladder; then immerse this tube in water (or rather a solution of common salt), containing some starch (letter *b*), and connect the solution of the iodide with the negative electrode, or pole (*c*), of a galvanic battery of fifty or one hundred pairs of plates, and the starch liquid with the positive electrode, or pole (*d*), you will observe in a few seconds that the starch is rendered blue by its combination with iodine, showing that this substance must have passed through the bladder to have come in contact with the starch. On examination not the least trace of perforation can be seen in this membrane. Now if we could on this principle introduce medicines into the system, or into tumors, it would be a con-

siderable practical improvement of our profession. In bronchocele we should have an easy method of conveying iodine into the swelling; in constipation a pleasant method of acting on the bowels. I throw out this hint for such of you as may feel disposed to follow up so interesting an inquiry.

ON THE

EFFICACY OF ICE IN DYSPEPTIC DISORDERS.

To the Editor of the Medical Gazette.

SIR,

I AM not conscious that any medical writer has noticed the use of ice and iced liquids in the treatment of stomach derangements. If not, you will perhaps give publicity to the following communication, the object of which is to propose another remedy for that distressing and widely-spread class of disorders, at once the offspring and the bane of highly-civilized and luxurious states of society. My attention, I confess, was first called to this subject by a lady, who, when in Italy, had derived great benefit from eating ice in a dyspeptic attack under which she then laboured. I had also very often remarked, as well as personally experienced, the refreshing influence of ice in languid states of the stomach, restoring the palled appetite, and re-invigorating the whole frame.

It occurred to me, besides, that I had not unfrequently seen persons of very delicate digestions, who durst not venture on a spoonful of cream at ordinary temperatures, eat of it largely in a frozen state, not only with impunity, but to the manifest advantage of their health. In regard to wine, also, it is notorious that the enfeebled stomach will endure much larger draughts of this intoxicating drink, without inconvenience, when it has previously undergone the process of icing.

Reflecting on these facts, it appeared to me highly probable that extreme cold, immediately applied to the secreting surface of the stomach, would be found a remedial agent of great value in the various affections of this sensitive and sympathizing organ. Without delay, therefore, I prescribed the ice freely in such gastric derangements as chanced to fall in my way, and quickly discovered that I had not formed an undue estimate of its powers. Relief almost immediate was the consequence in many cases of mere functional disturbance; and in those of a more fixed and structural character, it evinced a marked superiority over all other remedies in controlling the most distressing and refractory symptoms. To this practice, as opportunity offered, I have now adhered for years past; and so favourable has been the general result, that I feel more than ever anxious to recommend it to the consideration of my professional brethren.

I will not occupy your pages, sir, with a tedious recital of the numerous cases in which my confidence in the remedy here proposed is grounded, choosing rather, on the best authority, to illustrate my conclusion by an instance or two of general application*; and the first instance I shall adduce happened to a lady of delicate constitution, who had repeatedly suffered from functional derangements of stomach. On these occasions she experienced the usual symptoms of dyspepsia severely, attended sometimes by palpitations of the heart, and not without evidence of spinal irritation. Her recovery from such attacks was usually very slow under ordinary treatment. I therefore thought it advisable she should make trial of the (so to call it) "*refrigerant method.*" The ice was freely administered; the lady obtained almost immediate relief, and her recovery was rapid beyond all former example. Several times since she has resorted to the same plan with equal success; and now, by a timely application to it, she is enabled to ward off the severity of these attacks altogether.

The next and the only other instance to which I shall at present allude, occurred in a gentleman of the military profession, whose digestion had become impaired by long residence in hot and unhealthy climates, and which, continuing for some time after his return to England, at length terminated in a fixed

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* "Unum adhuc restat de quo monendus est lector, mihi in animo non esse, sequentes paginas infinito particularium observationum numero distendere, quibus methodo ibidem traditæ fidem adstruam: frustrà enim, et cum tædio lectoris, repeterentur ista singulatim, quæ in summas contraxi. Satis autem habui, ad calcem observationis cujuslibet generalis, particularem hic illic adnectere, qua methodi præcedentis medulla contineretur."—*Vide Sydenham, Præfat. v. finem.*

structural affection of the stomach. Besides other formidable symptoms, there was settled pain in epigastrio, increased on pressure, and especially on taking food, which was commonly returned almost as soon as swallowed, and along with it large quantities of thick, heavy, mucus. Of these matters, however, the stomach was wont to dispossess itself, not by the usual mode of retching and vomiting, but by a process of eructation or rumination. Continuing in this state some time, the body became weak and emaciated, and my advice was requested. I had the good fortune to allay, in some measure, the pain and irritability of stomach by the free application of leeches and blisters, followed up by anodyne and effervescent medicines; but the advantage was only transient. In a few days these distressing symptoms returned with redoubled violence, insomuch that the stomach would now literally retain nothing, the mucous discharge being at the same time more troublesome and copious than ever, and occasionally a little tinged with blood. At this difficult juncture I could think of no alternative but the refrigerant plan, as I have already called it. I therefore ordered a plentiful supply of iced milk. The patient ate it with avidity. It sat comfortably on his stomach, abating the pain and suspending the mucous discharges almost wholly during the short remainder of my attendance. This case, I need scarcely add, in the end proved fatal; and my only object in citing it, is to shew the extraordinary control of intense cold over some of the most intractable symptoms of gastric disorder. In thus extolling its efficacy, however, I beg to disclaim all intention of pronouncing it infallible, being persuaded that nothing can so effectually overthrow the reputation of any novel and untried remedy as the proclaiming it a panacea. Some instances, I doubt not, there are (perhaps many), in which it will prove unavailing; and others, as gout, &c., in which it might justly be deemed inexpedient, and even dangerous. Nevertheless, I confidently look to the experience of others for the confirmation of what I have myself deduced from pretty extensive observation, viz. *that intense cold, as applied to the secreting surface of the stomach, will generally be found a remedy of great value and avail in the disorders of that all-important organ.*

Leaving it, however, to time to establish or refute this opinion, it shall be my object, in the meanwhile, to inquire how far it is sanctioned by an appeal to acknowledged truths and rational principles. And in order to this, I shall advert to the well-known operation of cold on the surface of the body,—how it causes it to wrinkle and contract, and its blood-vessels to vanish from the sight. Often, indeed, does this cutaneous constriction proceed to the length of occasioning dangerous terminations to internal organs. Witness that almost endless catalogue of fevers and inflammations, whose common origin is exposure to cold. Again, in head affections that proceed from inordinate afflux of blood to that part, of what powerful avail are intensely cold applications! Then, as to internal surfaces, every one knows how effectual they are in restraining hæmorrhages, and in checking and resolving congestion and inflammation in various portions of the mucous system. With such analogies before us, is it not highly probable that the stomach also would, if similarly affected, experience similar benefit? And that it is, in the generality of its disorders, similarly affected,—that is, more or less in an irritative, congestive, or inflammatory condition, I gather from the pain and uneasiness so commonly felt in epigastrio in such cases, as well as from the known efficacy of topical evacuants in the removal of these symptoms. Other evidences there are to the same purport: but for the sake of argument, we will assume the truth of this opinion. We will suppose that in gastric diseases generally there is more or less of engorgement of the innumerable capillaries distributed to the interior of the great digestive organ. Ought we not to expect, as the necessary consequence of such a state, a proportional degree of pain or uneasiness in the epigastric region? Might we not also reasonably look for such an increase of sensibility in the part as would render the accustomed stimulus of alimentary matter almost intolerable? And would not this congestion or inflamed condition of the secreting surface cause it, after the example of other mucous surfaces (as in catarrh, dysentery, and the like) to pour forth its peculiar product in greater abundance, and so vitiated in quality as to create disturbances in the very function which

nature has assigned for its performance? Collect, then, these consequences; and is there any thing wanting to make a good general definition of gastric disorders? Assume the symptoms, and the engorgement plainly follows; admit the engorgement, and the symptoms are fairly deducible. Thus do these opposite hypotheses mutually serve to confirm and verify each other; and thus does it plainly appear, from an appeal to reason as well as fact, that, in the general run of affections of the stomach, the capillaries of that organ are in a greater or less degree of plethora and over-distention. This, perhaps, is their principal feature; and it is, I presume, only to be explained on the supposition of diminished tone or resistance in their coats, by virtue of which, blood, urged on by the ordinary force of circulation, rushes into them in undue proportion *. And, to ascend another link in the chain of causes, we cannot doubt but that this and every other instance of relaxation in capillary blood-vessels, must be referred to some impression previously made on the nerves with which they are in connexion, and under whose control they are, by the hand of nature, placed. Mysterious, indeed, is the process by which this physical change is effected, and so, perhaps, it will ever remain. To speculate about it, therefore, in the present state of our knowledge, would be but idle and unprofitable †.

But the fact itself is of unquestionable importance; for if that be admitted, the operation and efficacy of cold, in gastric diseases, is most obvious; there being no known remedy of greater power in restoring relaxed and weakened vessels to their proper tone and resistance.

Whether, therefore, we resort to analogical or hypothetical argument, or trust to the more sure guidance of experience and observation, still we arrive at the same conclusion. By the concurrence of every kind of proof of which the nature of the subject will admit, are we warranted in repeating our conviction, that ice and iced liquids will be found among the most valuable remedies for stomach complaints in general, whether functional or general.

It only remains that I thank you, sir, for the insertion of this communication in your excellent periodical, whose merits, as a record of authentic facts and useful information, are too well known and appreciated to need the commendation of your obliged correspondent,

F. Bailey, M.D.

Reading, Oct. 1835.

REMARKS ON

ANIMAL HEAT AND THE NERVOUS INFLUENCE,

In reference to Dr. Wilson Philip's Letter.

To the Editor of the Medical Gazette.

Sir,

I should be sorry to have misrepresented the views of Dr. Wilson Philip, or to have expressed too strongly any dissent from the conclusions of one to whom the science of physiology is under great obligations.

The passage to which Dr. Philip re-

* On this point I beg to be a little more explicit. Some years ago I inserted a paper in the Medical Repository, in which I endeavoured to shew (on the authority of Parry's experiments) that there reside in blood-vessels generally two opposing powers—the one, *elastic*, tending to increase, and the other *tonic* or *vital*, tending to diminish their capacity; and that, by a certain adjustment of these forces, the natural calibre or dimensions of such vessels would be determined. Supposing, therefore, the elastic property to remain invariable (which may in the present instance be fairly done), it is obvious, that whatever change should happen to any vessel in respect of its diameter or magnitude, must be the result of its vital contractility, or, in the language of Parry, its tonicity. That being *diminished*, the elastic power of the vessel would preponderate, and conspire with the *vis-a-tergo* to give it an augmented volume; in other words, to determine blood more freely into it. If, on the contrary, the vital contractility were to *increase* above its natural standard, it would prevail over its antagonist elasticity, and thereby lessen the size of the vessel and the quantity of its contents. It is to the former of these states, however, to which we here principally refer; it being that condition which we conceive to be essential to the existence of a great majority of diseases, and whose removal constitutes the cure.

† It may not, however, be amiss to notice the instantaneous manner in which this effect is sometimes produced. At the bidding of a moral impression, the cheek instantaneously becomes suffused, and its innumerable capillaries relaxed and overgorged with blood. Something analogous to this happens, I presume, to the stomach, whenever food enters it; especially if excessive, stimulant, or indigestible. If, then, to the injuries which this important organ is continually liable from these causes, we superadd such other as are of a mental, moral, or physical nature, how can it but be that the minute vessels of its interior lining should in the end become weakened and overloaded, and thus prove the fertile source of dyspeptic disorders, at which we have already hinted?

fers was written in 1823 *, and the expressions "animal heat, a secretion, or *tertium quid*, &c." were quoted from the early edition of Dr. Philip's "Experimental Inquiry," &c. If in his later edition he speaks of animal heat as something to be classed with the secretions, he still leaves in doubt what relation animal heat bears to them; whether it is a separate but connate effect of a common cause, or a secondary result, immediately caused by the chemical changes comprised in the formation of the secretions. In the recent additions to my paper, I have endeavoured to shew that the former of these alternatives (that animal heat is a *tertium quid* resulting from the action of the galvanic fluid on the blood), is unsupported by what we know of the properties of electricity as it can exist in the animal frame; and by pointing out the general law according to which heat must be chemically produced by the formation of the principles of the secretions, I venture to hope that I have rendered this latter alternative an explanation of the production of animal heat.

I admit that in my original paper the power of the spinal marrow to produce heat was too summarily denied; but I still think that its share in this process is only hypothetical. It is certain, that after the removal of the brain, some purely chemical changes do take place (viz. the absorption of oxygen and the production of carbonic acid, which also occur with blood out of the body), which, according to known laws, must produce heat; but that the spinal marrow, or any remaining part of the nervous system, has a similar power of producing heat, is a matter of mere speculation.

It would be well for the science of physiology if the nervous agency could be universally identified with electricity, and if its relations to secretions and muscular action, whether as a cause or only as an influencing agent, were fully understood. At present, although the researches of Dr. W. Philip and others, (especially the recent ones of M. Donné,) have proved the existence of electric currents in the body, yet this knowledge assists us little in the mysteries of vitality. As Dr. Alison has well remarked, electricity (as far as we at present know it) will afford no explanation of the diversity and peculiarity of the several secretions, or of a variety of the causes of muscular action; and whilst we admit the possibility of electricity having properties yet unknown, which may hereafter explain the various processes of organic life, we must for the present content ourselves with referring them to a cause of which we confess our ignorance, by calling it vitality.

I am, sir,
Your obedient servant,
CHAS. J. B. WILLIAMS.

Halfmoon-Street, Oct. 22, 1835.

* This passage was contained in the abstract published in the Transactions of the Medico-Chirurgical Society of Edinburgh, vol. ii.

A REMARKABLE CASE
OF
GALL-STONES CONNECTED WITH DELIRIUM TREMENS.

To the Editor of the Medical Gazette.

SIR,

IF you consider the following case worthy of notice, perhaps you will give it a place in your valuable journal.

I am, sir,
Your obedient servant,
H. S. CALDWELL, M.D.

Camberwell, Oct. 23, 1835.

Mr. B., who is about fifty years of age, was formerly an officer in the navy, and for some years past has been living upon a comfortable independency. His habits were generally considered sober, although not unfrequently he had recourse to spirits to aid digestion.

About the beginning of May last I was called up in the night to visit Mr. B. His countenance was then pallid, although agitated; tongue white, clammy, and tremulous; hands very unsteady; pulse and skin quite natural; the whole system free from pain. But what had produced the present alarm was a distressing delusion under which he had been labouring for several days, that his wife was endeavouring to wean from him the affections of his son and daughter by a former marriage. The absurdity of this was evident, and reasoning had a momentary effect of quieting his fears on the subject. But after a day or two he became so ungovernable that it was requisite to place him under restraint. For about two months he derived much benefit, under

the judicious care of Dr. Uwins, at Peckham Asylum; and his mind having become more composed, he was allowed to return home. After Mr. B. left the Asylum, I occasionally attended him; and his general health, which was greatly debilitated, seemed to improve, but there were at times symptoms about him which threatened a relapse.

On Sunday, August 30th, Mr. B. was seized, while at church, with severe pain in the epigastric region, which was relieved after taking some ol. ricini. On the following evening the pain returned. I then saw him rolling upon a sofa, in great agony. The pulse and skin were natural; the pain *not* increased by pressure. Anodynes and antispasmodics were administered, but produced no relief. In a short time the urine became exceedingly high-coloured and muddy; while the fæces, on the contrary, were remarkably pale. As some obstruction appeared to exist in the biliary ducts, Mr. B. was treated accordingly. Previous to the 6th September there was no fever. The pulse then rose to above a hundred, and an acute pain was now felt under the edges of the ribs in both sides, as also under the scapulæ. After the application of leeches the pain subsided, and the pulse fell to eighty. After this, the white of the eyes became yellow, and gradually, in succession, the whole surface of the body. The tongue, as usual, was clammy and tremulous; but except in the commencement of the attack, there was neither nausea nor sickness; and during the intervals of pain the patient relished his food as usual.

On the 8th, while under the influence of medicine, something was heard to rattle in the night-pan, which proved to be *nine* gall-stones, the colour of verdigris. Next day he passed *five* more, and on the following day *one*.

A favourable change now took place in Mr. B.'s appearance and feelings, which was very evident to all who knew him. An aperient mixture, containing hydriodas potassæ, as previously prescribed, was taken daily, and all went on well until the 29th, when the pain again returned, and continued violently for six hours without intermission. Next day he passed *ten* more gall-stones; on the following day, October 1st, *three;* and on the 2d, *two:* in all, twenty-nine calculi, which are at present in my possession. Ten of them are as large as a middle-sized hazelnut; the others somewhat smaller, and nearly of a triangular form; the whole weighing 163 grains.

After passing these calculi, the excretions gradually became natural, and a very remarkable improvement has since taken place in Mr. B.'s health, both in body and mind. His complexion is now clearer than it has been for years past; and his temper, instead of being irritable and captious, has, to the great comfort of his excellent wife, become mild and agreeable.

P.S. This case is also well known to —— Phillips, Esq., Surgeon, Albion-Street, Hyde-Park Terrace.

M. BAUDELOCQUE'S

IMPROVEMENTS IN PRACTICAL MIDWIFERY.

To the Editor of the Medical Gazette.

SIR

As you did me the favour of inserting my former communication on the instrument invented by M. Baudelocque, I have been induced to send you the following account of his other suggestions and improvements in obstetrical practice, hoping that you will deem it of sufficient importance to be laid before your readers

I may here mention that the lateral curvature perceptible in the engraving given in the No. of your journal for Oct. 3, does not exist in the instrument itself, it being occasioned by the paper slipping whilst under the pantograph.

I am, sir,
Your obedient servant,
JOHN CHAS. COOKE,
Member of the Royal College of Surgeons, Edinburgh, and Licentiate of the Society of Apothecaries, London.

Coventry, Oct. 21, 1835.

In those rare cases in which, after the head has been crushed by the cephalotribe, the shoulders form the obstacle to delivery, M. Baudelocque has recourse to another instrument, which he terms a "double crochet mousse à lame cachée." I regret much that I have not an engraving of this instrument, as I fear it will be very difficult to convey a clear or distinct idea of it by verbal description alone.

It consists of a blunt hook, grooved on

its internal or concave surface, and of a steel rod inclosed in a canula, which is made to fit the groove: to the end of the rod a knife is attached, resembling very much in form that used by cheesemongers; it is made cutting on its convex edge and angles. The mode of using the instrument is as follows: the hook is first introduced in the usual manner, and the parts to be divided securely embraced by it. The operator then slides the canula, containing the steel rod furnished with the knife, along the groove, until he feels the edge of the knife in contact with the child's body. He now makes the canula firm in the groove, by means of a screw, and then, by turning another screw, he causes the blade to perform the section of the parts comprised between its edge and the groove in the concavity of the hook; the hook being kept steady by an assistant.

I confess that this instrument does not appear to me to possess the security from danger which the cephalotribe does; as, although the blade cannot slip out of the groove, and consequently can do no mischief with its convex edge, yet it is quite possible, that before it is wholly included in the curvature of the hook, the angle of the blade which is not in contact with the groove may wound the sides of the uterus or vagina. M. Baudelocque, however, confidently asserts that this will never be found to be the case.

He also recommends this instrument to be used in those cases of arm-presentation in which the child cannot be turned, and is indisputably dead.

A description of this instrument will be found in a thesis sustained at the School of Medicine in Paris by M. Boytier, entitled "De la section du tronc du fœtus mort." In this thesis there is also an engraving of the instrument. I do not know whether M. Baudelocque has ever found it necessary to have recourse to it in any of the cases in which he considers it to be applicable. I have, however, frequently been present when he has shewn that it is capable of cutting a child's body in pieces, out of the pelvis.

Another of M. Baudelocque's improvements in obstetrics is the compression of the abdominal aorta, in order instantaneously to arrest hæmorrhage occurring in the lower extremities or pelvis, whatever may be the cause. He first applied it exclusively to uterine hæmorrhage, but now considers it applicable to bleeding dependent upon wounds of the femoral artery, or its branches. It is done with the greatest facility when the abdominal parietes are lax and flaccid, as is the case immediately after delivery. Its advantages over the ordinary methods of restraining uterine hæmorrhage are, its facility of application, and the absolute command it gives us over the supply of blood to the abdomen and inferior extremities. M. Baudelocque has long been in the habit of instructing his pupils in the mode of compressing this vessel: the directions he usually gives are, to bring all the fingers of the right hand to a level with each other by placing the points of them on a board, or other flat surface, so as to ensure an equal degree of pressure being maintained on all parts of the calibre of the artery. The fingers being reduced to a level, they are to be applied about an inch above the umbilicus, and gradually, but firmly, pressed down until the artery is felt under them. If the fingers of the left hand be now applied to the femoral vessels, their pulsation will be no longer perceptible. Any one desirous of testing the value of this mode of compression may satisfy themselves by trying it on muscular individuals: they will meet with but little difficulty, even in them, and none at all in those reduced by disease, or exhausted by hæmorrhage. I believe M. Velpeau has published a case which peculiarly shews the value of this mode of compression:—A butcher's boy in Paris divided the femoral artery, immediately after it emerges from under Poupart's ligament. A pupil of M. Baudelocque, who was present at the time of the accident, had presence of mind enough to compress the aorta until M. Velpeau arrived, and to maintain it until the latter had secured the external iliac: the hæmorrhage was arrested immediately the compressfon was applied, and the boy recovered.

If the case be one of uterine hæmorrhage, M. Baudelocque also stimulates the uterus to contract by small doses of ergot of rye, repeated at short intervals: he has also invented a spring bandage, by which the compression may be maintained for any length of time which may be required.

The last of M. Baudelocque's sug-

gestions which I shall notice, is that of passing a hollow tube into the mouth of children, when the head cannot be instantly extracted, and pulsation can no longer be felt in the funis. I have not seen any case in which this has been necessary, but in the No. of the Medical Gazette for Oct. 10, 1835, I find two cases recorded in which M. Baudelocque has succeeded in preserving the life of the child under these circumstances.

I have occupied a much larger space than I could have wished, but I feel convinced that these inventions and suggestions of Baudelocque deserve to be better known in this country than I have reason to believe is the case.

REPORT OF FRACTURES

TREATED IN THE LONDON HOSPITAL DURING SEPTEMBER.

To the Editor of the Medical Gazette.

SIR,

THE following is the statistical account of the fractures admitted at the London Hospital during the month of September. I have endeavoured more accurately to ascertain the side of the body on which the fractures of particular bones more frequently occur, in order that we may not too hastily draw our conclusions: I have therefore been more careful in my arrangement. The fractures amount to fifty in number, and bear the following proportions:—

		Right.	Left.	Unknown.	
Cranium	1	..	..	..	
Cervical vertebræ	1	..	..	..	
Pelvis	1	..	..	..	
Scapula (acromion)....	1	..	1	..	
Clavicle	7	2	4	1	
Ribs	7	1	6	..	
Humerus	3	1	2	..	
Forearm (both bones) ..	1	..	1	..	
Ulna	4	1	3	..	
Radius	4	1	3	..	
Metacarpus	2	1	1	..	
Finger	1	1	..	..	
Femur	6	2	4	..	2 of the cervix.
Leg (both bones)	5	2	2	1	
Tibia	1	..	..	1	
Fibula	3	..	3	..	
Metatarsus	1	1	..	..	
Toe	1	..	..	1	
	50	13	30		

I shall take the liberty of detailing the case of fractured pelvis, as it presents points of considerable interest.

September 19th.—A boy, 15 years of age, was brought in, having been knocked down and run over. The wheel of a cart had passed up between his legs, and had traversed the right side of the pelvis obliquely upwards. A wound was produced under the scrotum of about an inch in extent, through which a quantity of apparently venous blood passed. The right lower extremity seemed to the eye shorter than the left, but on accurately measuring from the spine of the ilium to the patella no difference was observable; the shortening was therefore attributed to some displacement of the bones of the pelvis. A rupture of the urethra being suspected, an elastic catheter was passed down the urethra, but could not be directed into the bladder; the fore-finger introduced into the wound readily felt the extremity of the catheter. After some unsuccessful attempts to pass the instrument, I dilated the wound of the perineum, and, after considerable difficulty, observed the lower portion of the ruptured urethra surrounded by the corpus spongiosum; the end of the elastic catheter was now directed along the

urethra into the bladder, where it was retained in such a manner that the water might dribble through it. Although the pelvis on depressing the ilia alternately, possesses more mobility than natural, no crepitation could be perceived. The boy was sick, and complained of the sudden inflation of his abdomen, which was tender on pressure; the urine was bloody, and did not pass freely through the catheter. These signs distinctly indicated a rupture of the bladder. A large opiate by the mouth, and in the form of an enema, procured him some sleep, but he sank the next day.

Sectio Cadaveris. — The peritoneal covering of the intestines exhibited marks of inflammation. The fundus of the bladder was ruptured through the peritoneal covering, and the mucous membrane of this viscus slightly protruded through the edges of the wounded aperture. Slight ecchymosis of the coats of the bladder. The right pubis was fractured near the symphysis, and the fracture extended across the ala of the ilium of the same side, near the sacro-iliac symphysis. The cellular tissue of the pelvis was extensively ecchymosed.—I am, sir,

Your obedient servant,

JOHN ADAMS.

New Broad-Street, Oct. 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Experiments and Observations on the Gastric Juice, and the Physiology of Digestion. By WILLIAM BEAUMONT, Surgeon in the U. S. Army.

"I MAKE no claim to originality in my opinions, as it respects the existence and operation of the gastric juice. My experiments confirm the doctrines (with some modifications) taught by Spallanzani, and many of the most enlightened physiological writers. They are experiments made in the true spirit of inquiry, suggested by the very extraordinary case which gave me an opportunity of making them. I had no particular hypothesis to support; and I have therefore honestly recorded the result of each experiment exactly as it occurred.

"The reader will perceive some slight seeming discrepancies, which he may find it difficult to reconcile; but he will recollect that the human machine is endowed with a vitality which modifies its movements in different states of the system, and probably produces some diversity of effects from the same causes.

"I had opportunities for the examination of the interior of the stomach, and its secretions, which has never before been so fully offered to any one. This most important organ, its secretions and its operations, have been submitted to my observation in a very extraordinary manner, in a state of perfect health, and for years in succession. I have availed myself of the opportunity afforded by a concurrence of circumstances which probably can never again occur, with a zeal and perseverance proceeding from motives which my conscience approves."

Nothing can be more modest and single-minded than this: it is conceived and expressed in the spirit of philosophy and truth; and for ourselves we yield a ready credence to all the statements and most of the deductions. Sure are we that the first are sincere, and the second, if not demonstrable, are certainly not yet disproved. The subject of the observations and experiments, ALEXIS ST. MARTIN, a Canadian, 18 years of age, robust and healthy, was, on the 6th of June, 1822, accidentally wounded by the discharge of a musket. The following is the description, when Dr. Beaumont first saw him:—

"The charge, consisting of powder and duck shot, was received in the left side of the youth, he being at a distance of not more than one yard from the muzzle of the gun. The contents entered posteriorly, and in an oblique direction, forward and inward, literally blowing off integuments and muscles of the size of a man's hand, fracturing and carrying away the anterior half of the sixth rib, fracturing the fifth, lacerating the lower portion of the left lobe of the lungs, the diaphragm, and perforating the stomach.

"The whole mass of materials forced from the musket, together with fragments of clothing and pieces of frac-

tured ribs, were driven into the muscles and cavity of the chest."

This remarkable patient went through some of the severest vicissitudes incident to accidental disease. In June, 1823, twelve months from the date of the accident, he was recovered sufficiently to perform light work; the injured parts were all cicatrized except the aperture of the stomach; thus described:—

"At the point where the lacerated edges of the muscular coat of the stomach and intercostal muscles met and united with the cutis vera, the cuticle of the external surface and the mucous membrane of the stomach approached each other very nearly. They did not unite, like those of the lips, nose, &c., but left an intermediate marginal space, of appreciable breadth, completely surrounding the aperture. This space is about a line wide; and the cutis and nervous papillæ are unprotected, as sensible and irritable as a blistered surface abraded of the cuticle. This condition of the aperture still continues, and constitutes the principal and almost only cause of pain or distress experienced from the continuance of the aperture, the introduction of instruments, &c. in the experiments, or the exudation of fluids from the gastric cavity."

In May, 1825, the experiments related in this work were commenced at Fort Mackinac, Michigan Territory. Part of these experiments were published in 1826, in the 29th number of the Philadelphia Medical Recorder. The patient remained in Canada four years, married, and begat two children, which he worked hard to maintain until 1829, when the wound was in the same condition as in 1825. In the service of Dr. Beaumont he laboured constantly, procreated "more children, and enjoyed as good health and bodily vigour as men in general." In 1831 he left the Doctor's service, went to, and remained in, Canada during the devastations of the cholera, himself going unscathed. In November, 1832, he re-engaged himself to his kind and philosophical master, for the express purpose of submitting to another series of experiments. During the whole of this term, from 1824, he has enjoyed general good health; "and he *now* enjoys the most perfect health and constitutional soundness, with every function of the system in full force and vigour." It is not necessary to detail the mode of obtaining the gastric juice, but it may be mentioned that "*bright yellow bile sometimes flows through the tube introduced into the stomach.*"

"The chymous fluids are easily taken out by depressing the valve within the aperture, laying the hand over the lower part of the stomach, shaking a little, and pressing upwards. In this manner any quantity necessary for examination and experiment can be obtained.

"*Valve.*—The valve mentioned above is formed by a slightly inverted portion of the inner coats of the stomach, fitted exactly to fill the aperture. Its principal and most external attachment is at the upper and posterior edge of the opening. Its free portion hangs pendulous, and fills the aperture when the stomach is full, and plays up and down, simultaneously with the respiratory muscles, when empty.

"On pressing down the valve when the stomach is full, the contents flow out copiously. When the stomach is nearly empty, and quiescent, the interior of the cavity may be examined to the depth of five or six inches, if kept distended by artificial means; and the food and drinks may be seen entering it, if swallowed at this time, through the ring of the œsophagus. The perforation through the walls of the stomach is about three inches to the left of the cardia, near the left superior termination of the great curvature. When entirely empty the stomach contracts upon itself, and sometimes forces the valve through the orifice, together with an additional portion of the mucous membrane, which becomes completely inverted, and forms a tumor as large as a hen's egg. After lying on the left side and sleeping a few hours, a still larger portion protrudes, and spreads out over the external integuments, five or six inches in circumference, fairly exhibiting the natural rugæ, villous membrane, and mucous coat, lining the gastric cavity. This appearance is almost invariably exhibited in the morning, before rising from his bed."

The remainder of the volume relates *every variety* of experiment practicable, and which could suggest itself to an ingenious and zealous seeker after truth. A selection of the experiments were in-

vidious, and certainly uninstructive if partially related; therefore as our limits exclude the whole, we must content ourselves and our readers by presenting merely the corollaries; they will furnish interest to all, and excite some to the prosecution of the subject, without, however, the same advantages as were furnished to Dr. Beaumont by the misfortunes of Alexis St. Martin.

"*Inferences from the foregoing Experiments and Observations.*

"1. That animal and farinaceous aliments are more easy of digestion than vegetable.

"2. That the susceptibility of digestion does not, however, depend altogether upon natural or chemical distinctions.

"3. That digestion is facilitated by minuteness of division and tenderness of fibre, and retarded by opposite qualities.

"4. That the ultimate principles of aliment are always the same, from whatever food they may be obtained.

"5. That the action of the stomach and its fluids are the same on all kinds of diet.

"6. That the digestibility of aliment does not depend upon the quantity of nutrient principles that it contains.

"7. That the quantity of food generally taken is more than the wants of the system require; and that such excess, if persevered in, generally produces, not only functional aberration, but disease of the coats of the stomach.

"8. That bulk, as well as nutriment, is necessary to the articles of diet.

"9. That oily food is difficult of digestion, though it contains a large proportion of the nutrient principles.

"10. That the time required for the digestion of food is various, depending upon the quantity and quality of the food, state of the stomach, &c.; but that the time ordinarily required for the disposal of a moderate meal of the fibrous parts of meat, with bread, &c. is from three to three and a half hours.

"11. That solid food of a certain texture is easier of digestion than fluid.

"12. That stimulating condiments are injurious to the healthy stomach.

"13. That the use of ardent spirits always produces disease of the stomach, if persevered in.

"14. That hunger is the effect of distention of the vessels that secrete the gastric juice.

"15. That the processes of mastication, insalivation, and deglutition, in an abstract point of view, do not, in any way, affect the digestion of food; or, in other words, when food is introduced directly into the stomach, in a finely divided state, without these previous steps, it is as readily and as perfectly digested as when they have been taken.

"16. That saliva does not possess the properties of an alimentary solvent.

"17. That the first stage of digestion is effected in the stomach.

"18. That the natural temperature of the stomach is 100° Fahrenheit.

"19. That the temperature is not elevated by the ingestion of food.

"20. That exercise elevates the temperature; and that sleep or rest, in a recumbent position, depresses it.

"21. That the agent of chymification is the gastric juice.

"22. That it acts as a solvent of food, and alters its properties.

"23. That its action is facilitated by the warmth and motions of the stomach.

"24. That it contains free muriatic acid, and some other active chemical principles.

"25. That it is never found free in the gastric cavity; but is always excited to discharge itself by the introduction of food, or other irritants.

"26. That it is secreted from vessels distinct from the mucous follicles.

"27. That it is seldom obtained pure, but is generally mixed with mucus, and sometimes with saliva: when pure it is capable of being kept for months, and perhaps for years*.

"28. That it coagulates albumen, and afterwards dissolves the coagulæ.

"29. That it checks the progress of putrefaction.

"30. That the pure gastric juice is fluid, clear, and transparent; without odour; a little salt, and perceptibly acid.

"31. That like other chemical agents, it commences its action on food, as soon as it comes in contact with it.

"32. That it is capable of combining with a certain and fixed quantity of

* I have now (Nov. 1, 1833, in my possession some clear gastric juice, possessing all its original properties, unchanged and undiminished, which was taken from the stomach in December 1832, about eleven months ago, and has been kept tightly corked in phials.

food, and when more aliment is presented for its action than it will dissolve, disturbance of the stomach, or 'indigestion,' will ensue.

"33. That it becomes intimately mixed and blended with the ingestæ in the stomach, by the motions of that organ.

"34. That it is invariably the same substance, modified only by admixture with other fluids.

"35. That gentle exercise facilitates the digestion of food.

"36. That bile is not ordinarily found in the stomach, and is not commonly necessary for the digestion of food; but

"37. That, when oily food has been used, it assists its digestion.

"38. That chyme is homogeneous, but variable in its colour and consistence.

"39. That towards the latter stages of chymification, it becomes more acid and stimulating, and passes more rapidly from the stomach.

"40. That water, ardent spirits, and most other fluids, are not affected by the gastric juice, but pass from the stomach soon after they have been received.

"41. That the inner coat of the stomach is of a pale pink colour, varying in its hues, according to its full or empty state.

"42. That, in health, it is constantly sheathed with a mucous coat.

"43. That the gastric juice and mucus are dissimilar in their physical and chemical properties.

"44. That the appearance of the interior of the stomach in disease, is essentially different from that of its healthy state.

"45. That the motions of the stomach produce a constant churning of its contents, and admixture of food and gastric juice.

"46. That these motions are in two directions; transversely and longitudinally.

"47. That the expulsion of the chyme is assisted by a transverse band, &c.

"48. That chyle is formed in the duodenum and small intestines, by the action of bile and pancreatic juice on the chyme.

"49. That crude chyle is a semi-transparent whey-coloured fluid.

"50. That it is further changed by the action of the lacteals and mesenteric glands. This is only an inference from the other facts. It has not been the subject of experiment.

"51. That no other fluid produces the same effect on food that gastric juice does; and that it is the only solvent of aliment."

MEDICAL GAZETTE:

Saturday, October 31, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo venlendi in publicum sit, dicendi periculum non recuso."

CICERO.

ACCIDENTS IN MINES.

DR. FIFE, of Newcastle, has published a pamphlet * which we are sure will interest many readers, and, we doubt not, far more than interest, if it do not alarm, others who hear — not without a shudder — the lamentable accidents which happen, from time to time, in our mining districts. The object of Dr. Fife is praiseworthy: he wishes to awaken the public attention to the deplorable sacrifice and loss of life which takes place so frequently in the coal mines, and to excite, if possible, the energies of those who have the power, as it is to be hoped they have the will, to remedy so shocking an evil.

Few things are more wretchedly mismanaged by our government than those which belong to a sound medical police. It is true that in a few instances we have indications that such objects as those to which we allude are not overlooked: we have our medical inspectors of prisons, of lunatic asylums, and professional commissioners to take cognizance of the due apportioning of labour in the factories. But there are numerous other respects in which an attention to the public safety is not sufficiently, if at all,

* A Critical Analysis of the Evidence adduced at the Inquest occasioned by the late Explosion at Wallsend Colliery, &c. &c. By George Fife, M.D., &c. &c.

given: the State seems to take no account of the loss of its subjects, or of the dreadful mode in which their lives are squandered. There are no medical commissioners whose duty it is to inspect the condition of the coal mines. Is this right—is it expedient—where the consequences are attended with such awful calamities?

Our readers must have learned from the newspapers the particulars of the catastrophe which occurred at Wallsend colliery in June last. An inquest was held on the bodies of the sufferers, and several startling facts were elicited in evidence. To Dr. Fife we are indebted for preserving the minutes of some of the most important circumstances disclosed, as well as for some valuable remarks which he has appended,—coming from him with peculiar propriety, as he has himself served the office of Coroner, and is at present a lecturer on Forensic Medicine at the Newcastle Medical School.

The Bensham seam, in which the explosion took place, is admitted on all hands to have always been a most dangerous locality; and Mr. Buddle, the eminent mining engineer who was examined at the inquest, says it required the utmost care to keep it in a workable state: this was chiefly managed by adopting what was supposed to be the most efficient system of ventilation.

But it is quite clear from the evidence, as well as from the principles laid down by Mr. Buddle himself, in his account of the explosion which took place at the Jarrow colliery, that sufficient care was not observed in ventilating the Bensham seam. To a question proposed by the Coroner, as to whether the latter pit did not give out a greater quantity of inflammable air than any other, Mr. Buddle replied, "It is a difficult question to answer; I must confess I have not seen any pit where extreme precaution was more necessary." From a further examination of the same gentleman, it appears that "the Bensham seam *abounds* in inflammable air. I shall take," continued Mr. Buddle, "for example, the quantity which is now in a state of combustion night and day at the top of the gas-pipe, at the coal-pit. This stream of gas issues from a detached portion of the workings, about five acres in area. The quantity, as gauged, issuing from that pipe is *eleven hogsheads per minute;* and supposing this volume of gas was suffered to pass through the workings, instead of being discharged at the pipe, *it would require from* 150 *to* 160 *hogsheads per minute of atmospheric air to dilute it below the firing point*, so as to admit of its being safely carried through the fire of the furnace."

Of the nicety with which the proper supply of atmospheric air requires to be adjusted for the purposes of safety, we may have some idea from the following extract, taken from Dr. Fife's pamphlet. Let it be kept in mind, that various quantities of inflammable air are evolved in mines at different times, and that there is often no possibility of predicting the moment when an enormous quantity may issue forth to the destruction of human life:—

"In Mr. Buddle's account of the explosion which took place at Jarrow colliery, there are some excellent remarks on the effect exerted by the atmosphere over the discharge of the inflammable air in coal mines. He observes, 'If the ordinary ventilation of a pit's workings should only be just adequate to the dilution of the quantity of inflammable air evolved, below the firing point, when the barometer stands at 30 inches, it would become quite inadequate whenever the barometer falls;'—'and, on the contrary, if the barometer should rise, the evolvement of inflammable air would be diminished.' It will be seen that this accords with a remark made by me in a former part of this pamphlet, in reference to the adequacy of the means for ventilating

'Wallsend colliery.' Mr. Buddle then says, 'Hence it is that we frequently find some of our coal mines in an explosive state when the barometer is low, while the presence of inflammable air is scarcely perceptible when the atmosphere is in a very dense state." Mr. Buddle then states that the *proximate* causes of explosions are three in number. 'First,—By a change in the density of the atmosphere, as indicated by the barometer.' Mr. B. states further, that he has known the 'whole circulating current of a pit's workings rendered explosive in the course of fifteen or twenty minutes.' This was the cause of the explosion at Wallsend in the year 1821, when *fifty-two lives* were lost. Mr. Buddle ascribes the accident to the '*too great* confidence of the overman in his own judgment, and that he had not given the usual alarm, 'Put out the lows!' in due time.' At the time of the accident the barometer stood at 28·8 inches. The second cause mentioned by Mr. Buddle is—'By the accumulation of the inflammable air in the 'threads' and fissures of the roof and pavement.' Mr. Buddle states that Backworth colliery was very subject to this cause of danger. Third—'By cavities, or large fissures, which do not seem to have any outlet.' 'This may be considered the most dangerous case, as it frequently happens that *a large quantity* of gas bursts out from those cavities, which occur in the roof and pavement as well as in the coal.' 'The great danger in this case arises from the *suddenness and great force* of the eruption, without giving sufficient warning for the escape of the colliers, or for allowing the persons in charge of them time to adopt the necessary measures of precaution.' This is what the miners call a 'bag of foulness.' This was the cause of the accident at Washington colliery in 1828. The explosion was occasioned by a 'bag of foulness' breaking down from the roof of the air course 'board,' behind the workmen, which at the same time, stopped the ventilation, so that a double cause operated in this instance to occasion the explosion: the 'stoppage of the ventilation, and an enormous discharge of inflammable air occurring at the same instant.' The accident at Jarrow was occasioned in a similar manner—viz. by the 'bursting out of a bag of foulness from a cavity in the coal.' Does any proof exist that the accident at Wallsend was not caused by a similar occurrence?"

This, however, constitutes by no means the whole of the risk incurred in the working of mines. We believe that the operations of nature owe a great proportion of their fatality to the unaccountable recklessness of the workmen and their overseers. The arrangements in this very Bensham seam must strike every one who inquires about them as being the most stupid and mischievously silly that can be conceived. In one part of the pit davies were used, and sometimes suffered to become red-hot before the alarm was taken; in another, not two hundred yards off, and with a free communication between the localities, *candles* were employed, and the beds of coal exploded by gunpowder. A naked oil-lamp, too, it appears, was suspended at no great distance from the dangerous part of the workings: and all this in the foolish trust that the actual ventilation would be sufficient to obviate all danger, and that the carburetted hydrogen *would not* drift to those situations where flame was nakedly exposed! Nay, this was not all; there were awful warnings for some time about the Bensham seam; and if its general character were not enough to inculcate caution, one should have thought that the occurrences of the 17th would have in some measure prepared any rational being for what so dreadfully took place on the 18th of June. But no; the hardihood of the miner seems to have no bounds. The operatives in our collieries are clearly persons not to be entrusted with the care of their own safety, and it is precisely for this reason that we think it the duty of the State to look to the matter.

We should lay some further extracts from the pamphlet before our readers, but that the evidence being of so complicated a nature makes it difficult to

select, without entailing the necessity of selecting other passages at the same time, which would go far towards neutralizing those first chosen. The truth is, that after all, the evidence adduced by the author of the pamphlet is vague, and in great measure inconclusive. After an attentive perusal, we confess ourselves at a loss to decide whether the calamity was owing simply to gross negligence, to accident, or to *design*. Our impression, however, is, that to the two former it must mainly be attributed. One thing ought not to be lost sight of in most cases of this kind—namely, that what would be the most valuable evidence is generally wanting: we mean that of the sufferers; but they rarely survive; in the present instance they all perished.

Dr. Fife, among the preventives which he suggests, lays great stress on the value of well-constructed Davy-lamps, properly furnished and attended to. He evidently thinks this of paramount importance, and challenges the statement of any well-authenticated case tending to invalidate his opinion. We believe him to be quite too sanguine on this point; for independently of the fact of its being now put beyond all doubt that the Davy-lamp is *not* secure,—that it explodes in currents of inflammable air—it has come to our knowledge that there have been cases where fire-damp in coal-pits has been exploded with terrible consequences by the lamp, and that —where no fault could fairly be imputed to those who had the adjusting of those supposed, but, as we think, too much trusted, guardians of safety.

SOCIETY OF APOTHECARIES—REGISTRATIONS.

THE number of pupils registered this season has been larger than ever before. *One thousand and fifty-five* entered their names at Apothecaries' Hall, up to the 21st instant.

LECTURES ON SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

By Dr. Latham.

Medical investigation conversant exclusively with Matters of Fact—What Medical Facts are, and what they are not—The Observation and Collection of Facts—Their Arrangement, according to Analogy or Resemblance; according to the Relation of Cause and Effect—What is meant by the Relation of Cause and Effect between Medical Facts—Peculiar Difficulties in the way of ascertaining that Relation—The Task of ascertaining it necessary to our knowledge of the Sources of Disease; of the Influence of Remedies; and of the Connexion between the Disease and its Symptoms—The Sources of Disease, how easily and quickly ascertained in some instances; how tardily and difficultly in others—The Influence of Remedies, how liable to deception, from a vicious credulity on the one hand, and a vicious incredulity on the other—The Nature of General Principles in Medicine, and how they are reached.

CERTAIN observations which I formerly made * must be considered to comprise the *method* only in which the particulars of a case may be most conveniently surveyed: but more remains to be considered than the mere form of case-taking.

You may adopt this mode of case-taking, or any other which your own experience may make most convenient to you: but no method of recording the particular facts will be of any use to you, unless you have right notions concerning the *facts* themselves. The facts, in truth, *are* the case; and it is only for the sake of getting *sure* possession of them that we adopt any form of (what is technically called) case-taking.

Now the whole business of your lives will be the business of taking cases; not necessarily with pen, ink, and paper, or after any technical form. But still it will be the business of your lives really and essentially, and in the justest sense; for each case is made up of its own facts, and the facts alone must teach you the nature of the disease, and suggest the remedy in every patient you see. Therefore, as long as you live, you will be evermore conversant with facts, learning and collecting them, arranging and combining and separating them, tracing their relations, and through them arriving at general principles.

* Vide Med. Gazette, vol. xi. p. 193.

There are peculiar causes which will ever prevent medicine from arriving at the certainty of purely physical science. But in so far as it is *certain*; in so far as it has taken the form of a science at all; it is built upon the same foundation with all other sciences; namely, upon facts: and in so far as it is uncertain, beyond what, in its own nature, it ever need to have been; in so far as it has not deserved the name of a science; it is raised upon a foundation which never would have been deemed sufficient for any other department of human knowledge.

It is important, then, to the right judgment and the right treatment of every individual case that we see; it is important for the sake of preserving to medicine whatever claim it may have to the name of a science, and (I will add) for the sake of our own credit and satisfaction, that we should utterly reject all other foundation of professional knowledge except matters of fact.

I desire to convince you of this, by shewing you, first of all, what matters of fact in medicine really are, and what they are not.

Every man's notion concerning any department of knowledge is the popular notion, until it is rectified by further inquiry. The popular notion concerning medicine is, that diseases are separate essences, and that an idea can be formed of them apart from the living being in whom they occur; that a fever, or a pleurisy, would still be something real, although there were no living beings in whom they could manifest themselves.

But diseases are not abstractions: they are modes of acting different from the ordinary and healthy modes—modes of disorganizing, modes of suffering, and modes of dying: and there must be a living, moving, sentient body for all this.

Endeavour to understand this truth; and at the very entrance of your professional studies get rid of all abstractions, that you may never record them and use them (as they are and have been recorded and used) as matters of fact.

You may record that this man has a hot skin, a dry tongue, and a frequent pulse, and call his disease a fever; that this man has a pain in his side, difficult breathing, and a hard pulse, and call his disease a pleurisy; but beware of taking this fever or this pleurisy for more than they really are. The fever is nothing, and the pleurisy is nothing, but the complex of the several facts which you have recorded under the head of each: separate from them, they are mere names. The fever has no treatment, and the pleurisy has no treatment, but what is suggested by the facts included under each. You prescribe for the hot skin, the dry tongue, and the frequent pulse, and you bring back their condition to what is natural, and so cure the fever; and you prescribe for the pain in the side, the difficult breathing, and the hard pulse, and so cure the pleurisy. But if, without regard to the facts apparent in the individual patients, you pretend to address your remedies to the fever *itself*, or the pleurisy *itself*, you take aim at an absolute phantom.

Bear in mind, then, that abstractions are *not facts*; and next bear in mind that *opinions* are not facts. To record that a patient is *better* to day and *worse* to-morrow; that he is at one time doing *well* and at another doing *ill*; is to give a *summary opinion* upon the facts, not the *facts* themselves.

I do not mean to say that such summary opinions should not be formed, or that they should not be announced. We always *do* form them, and the patient, or the patient's friends, always require us to communicate them; and most justly. But to ourselves, and in communication with other medical men, either for the sake of marking the exact state of the disease or suggesting the remedy, they are utterly useless.

If, in going round the wards of the hospital, I had it punctually recorded that this, that, and the other patient, had a peritonitis, a nephritis, or a hepatitis; and that day after day, this, that, and the other patient were better, worse, or just the same; and if, day after day, I were to order bleeding, blistering, or purging, as the case might require, and thus you were to witness numerous instances of recovery; you would not reap the slightest benefit from me and my pretended instruction, although you went round with me for a twelvemonth. For this would be to keep industriously from your notice every thing in the shape of a *fact* by which you could estimate the nature and progress of the disease, or the operation and effect of the remedy.

In medical science, the only materials of our knowledge are those things which are referable to our sensations and perceptions: matters of fact. Such are the temperature of the skin; the number and qualities of the pulse; the quantity and qualities of secretion; functions and modes of action in the several parts and organs of the body; and all their cognizable deviations from what is natural: also pain; for although pain, as to its actual occurrence in a particular case, must be taken upon the testimony of the sufferer, each man's own experience must some time or other have convinced him that pain is a *fact*.

Such, too, are the conditions of parts discoverable after death; their increased or

diminished bulk; the changes of structure and injuries they have undergone, and the morbid products to which they have given origin. These are the sort of facts with which we have to do, if we would know diseases and how to treat them.

Such being the materials of our knowledge, it becomes important to consider how we can use them best and abuse them least; for the materials of our knowledge require to be hewn and squared, and fitted to their place, with much care, and skill, and diligence; otherwise, what might have been a seemly edifice, may chance to be no better than a heap of rubbish.

First, then, as to the *simple reception* of medical facts, there is a good deal to be learnt.

All facts are not of equal value: some are trivial and accidental, some important and essential to the subject. In your inquiries at the bed-side, you will have to select and to reject cautiously and discriminately. Patients themselves are apt to press upon their medical attendants symptoms (generally consisting of strange sensations) which are irrelevant to their present disease. Again: in your examinations by dissection, you will have to use much care, lest you should admit appearances as indicative of disease which are accidental, or may have arisen during the process of dissolution, or from the position of the body after death.

But in this sifting and separating of facts, be cautious of throwing away any thing that is really valuable. Beware of rejecting facts of which you do not, perhaps, comprehend the import, and because you do not comprehend it; but rather reject those which you *do* comprehend, and know them to be trivial. Be careful especially of not allowing their due weight to facts which appear contradictory to each other; rather examine them more scrupulously because they are so. For instance, a patient may have a severe pain in the side and a frightful dyspnœa, and the pain and the dyspnœa may have arisen suddenly. Such symptoms at once carry with them the suspicion of active inflammation, and our thoughts may immediately run upon copious bleeding. But the same patient may, in the meantime, have his skin cool, and his pulse soft and tranquil, and not more frequent than the pulse of health. Cases of this and of the like kind are not uncommon, in which symptoms of a contrary character neutralize the import of each other, disprove the suspected existence of a dangerous disease, and forbid the needless adoption of a severe remedy.

The more you exercise yourselves in the observation of medical facts, the more you will understand the sources of error to be avoided in the reception of them. Time and diligence, and constant intercourse with the sick, if you have but *an impartial and honest* mind, will enable you to lay up a large and useful store of genuine facts, and draw from it as the treasury of your future knowledge.

I say an *impartial* and an *honest* mind, because it is remarkable how apt some little favourite theory is to get early possession of the student's imagination, rendering him dishonest (perhaps unconsciously) in the simple reception of facts. It is like some little favourite sin in our moral nature, which taints the character of the whole man.

A premature desire to generalize, an eagerness to arrive at conclusions, and a readiness to rest in them, are very common infirmities, and they offer a very serious hindrance to the right acquisition of facts. For if the early habit of theorising does not often estrange the mind of the student from the wish to observe altogether, it may yet pervert the faculty itself in its very use and exercise; and then, be his wish what it may, he cannot observe honestly. He gives an undue weight to the facts which accord with his assumed principle, and no weight at all to those that conflict with it: habit forces him to do so, and he cannot help it.

A very good and wise man has explained this matter by an illustration, which is so beautiful and so true, that I must recite it to you:—"A watchmaker told me that a gentleman put an exquisite watch into his hands that went irregularly. It was as perfect a piece of work as was ever made. He took it to pieces and put it together again twenty times. No manner of defect was to be discovered, and yet the watch went intolerably. At last it struck him that the balance-wheel might have been near a magnet. On applying a needle to it, he found his suspicions true: here was all the mischief. The steel-work in the other parts of the watch had a perpetual influence on its motions; and the watch went as well as possible with a new wheel. If the soundest mind be *magnetized* by any predilection, it must act irregularly *."

In the reception of facts, then, it is essential, first, that they be fully ascertained—i. e. upon sufficient observation; 2dly, that they be fairly and honestly represented, without disguise, modification, or omission, to make them suit particular theories; 3dly, that they be important and essential to the subject—not trivial and merely incidental to it.

Next comes the arrangement of facts.

* Cecil's Remains—on judging justly.

Now, facts may be arranged according to certain characters of agreement observable between them. There is an agreement of analogy and an agreement of resemblance. *Analogy* is a loose sort of resemblance, in which points of agreement and points of difference are mixed up together. By separating twenty points in which they differ, and retaining the two or three in which they agree, multitudes of things may be brought into this loose kind of resemblance and classed together. Thus, *analogy* is enough to bring diseases together in the same nosological order, as Fevers. Then, again, by calculating what individuals of the general order have most points of agreement, and distinguishing them accordingly, we come to divide orders into genera; and by still further selecting the individuals of the genus which have most points of agreement, we find the species; and by dealing with the species in like manner, we may divide it again and again, until thus, from the relation of mere analogy, we reach that of a tolerably strict resemblance. This is the scheme upon which nosologies are constructed; and nosologies have their use, until we can arrive at something better. Better to arrange facts according to the relation of analogies and resemblances, than not at all.

But it is far better (if it were possible) to arrange them according to their natural sequences, i. e. according to the relation of cause and effect. Now, "our knowledge of cause and effect, in reference to any two particular events, is founded entirely upon the observation of a uniform sequence of the events; or of the one following the other in a uniform manner in a great number of instances. The greater the number of instances is in which the sequence has taken place, with the greater confidence do we expect it to take place again under similar circumstances; and every single instance in which it does not occur, weakens our confidence, unless we can discover some adequate cause by which the sequence was interrupted. The result of this confidence is, that when we observe the first of two such events, we expect the second to follow it; and that when we observe the second, we conclude the first has preceded it. The first we call cause, the second effect *."

The philosophical physician is evermore studying how, upon adequate grounds, he can assign to medical facts this relation. But he knows in how delicate and difficult a task he is engaged. He is obliged to wait upon experience, and to attend to phenomena as they happen to occur. He cannot bring them together at will, and vary and transpose them as he likes, so as to learn their connexion. He envies the ease with which the chemist can bring any substance within the sphere and influence of as many others as he pleases; and the accuracy with which he can then ascertain the degrees of affinity it bears severally to each,—an accuracy so precise, that he can express them by numbers.

Further: when the physician has ventured to draw such conclusions as his long observation seems to warrant, he cannot test their truth by any simple experiment. He has no litmus or turmeric paper to tell him whether the blood-vessels or the nerves are the prime agents in producing a certain form of fever; but what his long observation seems to have taught him, he must still wait for the same long observation to confirm or to confute.

Unfortunately for us, the nature of medical causation is such, that it takes as much time and trouble to rectify an error as to establish a truth. Thus it may require the experience of one man's life to arrive at some plausible theory, and the counter-experience of another man's life to shew that it is false.

The vast experience required to establish a uniform sequence of events, and the impossibility of applying any summary test to the truth of our conclusions, are difficulties inseparable from the nature of medical causation. They lie at the very root of the matter.

And beside these, there are others, from sources impossible to enumerate, which perpetually beset and waylay our path of inquiry, breaking in upon the uniform sequence of events, and disappointing the best conclusions of experience. Such are the influences of places, and seasons, and climates, and the wills, feelings, and propensities, and all that is understood by the constitution, corporeal and mental, of human beings.

But however hard the task may be, we must still try to know the true relation of the things which concern the ordinary practice of our profession: we *must* trace the influence of external agents as causes of disease; otherwise we can do nothing for its prevention. We *must* trace the influence of external agents as remedies; otherwise we can do nothing for its cure. We *must* trace, too, the connexion between certain symptoms and certain morbid processes going on within; otherwise we can adopt no rational treatment of individual cases.

Respecting the external causes of disease, you must make them your study as opportunities may present themselves to you. But there are some so important,

* Abercrombie; Intellectual Powers, 390.

that their investigation has become of itself a department of medical education;—I mean what is called "forensic medicine." And, indeed, I know no department of public teaching which, if it be entrusted to good hands, (and I am sure it is so in this hospital), promises more benefit to medical science and medical practice than this. It undertakes to illustrate the modes in which injury, disease, or death, arrive from those external agents or accidents that are most signally hurtful to animal life; from poisons of every kind; from lightning; from hanging and drowning; from corruption of the air; and from every method of simple violence. Thus forensic medicine is conversant with all the highest points of physiology and pathology; and its very purpose requires the greatest exactness in the nature and display of its proofs. It requires, in truth, that they should be so made out as to be obvious to such understandings as ordinary men are accustomed to bring with them into a jury-box.

Here, then, is provision made, within a large and interesting field, for demonstrating to you the effects of external agents as the causes of disease; and all *matters of fact*.

The display of the fatal effects resulting from the causes enumerated to several vital parts and organs, furnishes so many demonstrations of the possible ways in which the same parts and organs are capable of suffering from causes less hurtful. Not long ago, poisoning was an affair of the utmost darkness and mystery; but now the rationale of poisoning in its several kinds is so well made out, that I am able to refer to it for the best instances which pathology affords of cause and effect in the manifest influence of external agents and accidents for the production of disease.

Poisoning, and the severer injuries, are a sort of pathological experiments. They produce upon this or that organ all the phenomena which any conceivable disease can exhibit; and they produce them in the greatest simplicity, because the subject is often previously in a state of health. Thus it is that they furnish an admirable introduction to the study of (what is called) spontaneous disease in the same organs.

Very acute inflammations arising suddenly in a healthy body afford the *next best* instances of the effects of external agents as causes of disease. I am accustomed to regard them in the light of medical accidents: for although the inflammation come from no injury in the ordinary sense, yet it will generally be found to have followed the exposure of the body to influences which act with a kind of violence—such as sudden changes of temperature, or the use of meats and drinks which, from their preposterous kind or quality, have become in a manner poisonous. Thus, the external cause productive of inflammation, and the time of its application, are sometimes as accurately determined as a fall or a blow.

In proportion as diseases are more chronic, they are (upon the whole) less easily assigned to their external exciting causes. With respect to them, a longer experience and inquiry are needed to establish a uniform sequence of the events. If you are attentive observers in the wards of this hospital, it will require a brief period only to convince you that the sudden impression of cold is among the undoubted causes of acute inflammation in various vital organs. But you cannot know that the habitual indulgence in spirituous liquors is undoubtedly productive of congestion of the liver until after years of experience. For the present you must be content to take this fact upon trust from the testimony of others.

The questions of malaria, of contagion, and of animal poisons; the certainty of their influence as causes of disease; and the mode, the sphere, and condition of their operation, are all questions of the highest importance, and involving facts of great interest. But I would advise you not to meddle with these questions at present; for *at present* you can only do it *speculatively*: therefore it is better that you should wait until a sufficient number of facts have fallen under your notice, either to enable you to form some conclusions of your own upon these subjects, or to have a sound judgment of the conclusions which have been formed by others.

With respect to the influence of external agents as remedies, I would recommend you to be most jealously observant of every circumstance connected with the treatment of individual cases. A *mere sequence* of events is not a *necessary* sequence. The remedy may be administered, and the disease may cease; and yet the treatment and the cure may not be cause and effect.

The remedy may have been really *inert*; and the spontaneous powers of reparation in the constitution or in the part may have been enough to surmount the disease; or the remedy may have been *active* in the wrong direction, and the powers of reparation more than enough to surmount the disease,—enough both to surmount the disease and to render the remedy harmless.

Your only safeguard against such deception lies in the most jealous and scrupulous observation at the bed-side. Turn your attention first of all to well-marked

instances of disease which is acute and rapidly progressive, where the remedies must be of equal force with the disease, and must operate with equal rapidity, and from which, if a curative impression follows, it must be a *sensible* impression. Watch the treatment of inflammation in various vital organs by the different modes of bleeding, by calomel, by tartar emetic, by colchicum; and the treatment of certain convulsive and painful disorders, and certain forms of delirium, by opium.

Beware of mistaking the nature of the disease, and then believing that the remedy has cured what in fact never existed. I lately read a book in which a certain remedy was recommended as infallible for diseases by dozen: tic douloureux, ovarian dropsy, disorganization of the heart, paralysis, and I dont know what besides, were all cured by it. Of these diseases no description was given, but only the name; so that there was not the slightest evidence that any such diseases existed as those that were said to be cured.

In like manner there have been remedies for cancer, remedies for consumption, remedies for stone,—all owing their reputation to the nature of the disease being mistaken.

This is the field in which ignorance and imposture reap their golden harvests. Not that it is impossible for those of good intention and good information to mistake the character of a disease, and so ascribe an efficacy to a remedy which is not its own. But those have the best security against this error who have taken the most pains to acquire a habit of faithful and jealous observation.

But there is an opposite infirmity not uncommon among medical men, which is just as much to be deprecated as the easy credulity of which we have been speaking, a scepticism in regard to the influence of medicine—a stubborn reluctance to admit the relation of cause and effect between remedy and cure. Surely it is not at all less hurtful to take up a notion that a number of diseases, from their nature or seat, are beyond the reach of all remedies, than to believe that any or every disease that gets well after the use of any remedy is necessarily cured by it. In the one case things are linked together as cause and effect, which bear no such relation; and, in the other, things which really bear that relation, are perversely dissevered.

Many men pride themselves upon this vicious scepticism, and wish to be thought to exercise a philosophical caution. Voltaire, who was upon the watch for every ludicrous infirmity of human character, was sharp enough to discern this in physicians, and has made excellent sport of it. He makes a physician of renown come from Memphis to cure Zadig of a wound in his *left* eye. The physician, however, affirms it to be incurable, and predicts the very day on which Zadig is to lose his sight, regretting at the same time the accident had not befallen the *right* instead of the left eye, for *then* he would have promised his cure; for that wounds of the *left* eye were in their very nature incurable. But Zadig gets well; and the physician writes a book to prove that he ought nevertheless to have lost his sight.

But much better than the fiction of Voltaire, and very much more to the purpose, is what really happened, and stands authenticated in a Scotch law book. Three physicians and two surgeons made oath in a court of justice, that, "by the rules of their prognostics," the wounds received by one James Houston were mortal wounds. But James Houston was still alive, and, to the honour of the three physicians, the two surgeons, and "the rules of their prognostics," was plaintiff in the very process where they had so memorably deponed*.

With respect to symptoms and their connexion with morbid processes going on within, the subject is too extensive to allow me to enter upon it at present. Indeed, it is so extensive and so important, and so full of its own difficulties, and yet wherein right views are so expedient, and wrong views so perilous, that I have thought it would not be altogether a profitless task both to you and to me, if we were to undertake together a patient and comprehensive examination of this very subject. Therefore, in connexion with the proper business of the wards, and the observation of cases, and (as I trust) for your help and my own, and for our mutual guidance at every step of our practical studies, I will endeavour, in a series of lectures, to give what illustration I am able to the doctrine of symptoms.

It remains that we notice another, and (philosophically considered) a higher purpose, which concerns us in the contemplation of medical facts—viz. the discovery of general principles from them.

A principle (as the name imports) is a beginning. A principle is some matter of fact to which numerous other matters of fact are traced as to a common source; and when we speak of discovering a principle, we mean the business of analysing or decomposing compound matters of fact into those which are simpler, until we come to one which is simpler still, and more general and elementary; and, being unable to go beyond it, we regard this as

* Edin. Med. and Surg. Journal, vol. i. p. 339.

an ultimate fact, or a principle. A principle is an ultimate fact, and a universal fact, and true, without a single contradictory instance.

Now it must be confessed that there is no fact in medicine (*i. e.* no fact respecting the animal body, its actions upon itself, or its obedience to other influences) which has the same character of universality with certain facts respecting the external world. In medicine we have no fact so universal as that all bodies unsupported fall to the ground, no principle so sure or irrefragable as gravitation.

Nevertheless, in medicine we talk of principles, and we are continually striving after them; but, in the strict philosophical sense, have we ever really compassed them?

We have reached *forms* of principles (if I may so say) rather than principles themselves. By *forms* of principles I do not mean things fictitious, or things purely imaginative, but facts tested by observation, and carefully analysed, and very comprehensive, but not universal: facts true in a vast number of instances, but not true in all.

In medicine there have, indeed, been facts, which for a time have passed for universal, and for a time have held the place of principles; but larger experience has shewn that their title to it was not a just one.

There is a certain order of symptoms constituting what is called "angina pectoris;" and angina pectoris was for years, by the common consent of medical men, drawn from extensive observation and dissection, universally ascribed to ossification of the coronary arteries of the heart. But more enlarged experience has found angina pectoris to exist where there has been no such change of structure, but another form of disease, viz. dilatation of the origin of the aorta; and *still* more enlarged experience has found it, where there has been neither one nor the other.

The disease produced by the vaccine virus gave proof, by instances almost innumerable, of imparting to the constitution a protective power against small-pox. Accordingly, this power was believed to be absolute and universal. At length contradictory instances arose and multiplied; and the protective power of vaccination was now no longer a law or a principle. In the present state of our experience we still ascribe to vaccination a protective power against small-pox, but one which is *only highly probable*, not certain. We ascribe to it, moreover, when its protective power fails, a mitigating power; but this, too, is only highly probable, not certain; for assuredly both its protective and its mitigating influence often fail, and small-pox after vaccination goes through all its stages *unaltered*, and is in all respects the *same small-pox*, as when no vaccination has taken place.

After the history of vaccination we know not what number of concurrent instances is enough in medicine to prove a fact universal, or when we can ever be safe against the intervention of contradictory instances, and venture to rely upon any fact as a doctrine or a principle.

But we are not, therefore, to abandon our search after principles; and the same method which in other departments of natural knowledge has alone led to their discovery, we must still employ in ours; for although in our hands a less eminent success has hitherto attended this method, no success whatever has attended any other. This method imperatively requires, that the principle sought be a matter of fact.

If, while we properly restrict ourselves to matters of fact in every other stage of our investigations, we yet take a fiction for our principle, medicine will never improve as a science in our hands, and ancient errors and follies will only give way to new ones.

The fault of physicians has not so much been, that they have shown a general disregard of matters of fact, as that they have lost sight of them just when they ought especially to have kept them in view—when they were concerned with principles. Hence the mischiefs that have arisen to physic in the shape of so many renowned theories, either shown to be false, or not shown to be true. Take any of these false or unverified theories you please, and you will always find it derived from some principle gratuitously assumed, a principle which is either no matter of fact at all, or incapable of being shown to be a matter of fact.

The principle assumed may be some physical process or property, such as a spasm of the extreme vessels, or a peccant matter in the blood; things which possibly may be, but which are entirely without proof, and even too subtle to admit of any; possible facts, but facts quite unascertained and gratuitous.

Or the principle may be neither process nor property, nor any thing that has a physical existence, real or possible, but a mere figment of the mind. The Zoonomia of Dr. Darwin abounds in principles of this kind. Even Mr. Hunter, with all his wariness and penetration in search of truth, admits what he calls "the stimulus of necessity," as a principle or element engaged in the production of diseases before they are yet apparent in their phenomena.

This is to escape from physical inquiry into the region of pure fancy.

What has been said of principles in medicine, whether erroneously or legitimately pursued, may seem to offer small encouragement to physicians to engage further in the search after them. Many of the most celebrated that have given renown to schools and universities, have been abandoned altogether—abandoned because they have been erroneously pursued, and have not possessed the essential character of matter of fact. Many, again, being real matters of fact, and legitimately sought and accepted as principles for a time, have at length been abandoned, because they have been found not to possess the essential character of universality.

But we must still concern ourselves with principles; we cannot help it; all men do it in some sort or other; for the mind is not able constantly to keep in view all the particulars of its own experience. It must needs reduce them within a narrower compass, and contemplate them (so to speak) in some representative. Thus a law or a principle must be set up, right or wrong. Some forge a maxim, and some forge a fact, and soon find it conspicuously illustrated in every instance of disease they meet with. They find *irritation* in every thing, or *spasm* in every thing, or *bile* in every thing. While others, after having gone on observing and collecting facts, and cautiously arranging them according to their natural relations, venture at length to rest in one which seems to have every characteristic of a principle, and yet in process of time may turn out to be no principle at all: witness the protective power of vaccination.

The truth is, something must be conceded to physicians in respect of the very nature of the subject on which they are engaged. Let the principle be ever so legitimately reached, we are only answerable for it as a law explanatory of the facts *already known*. But in medicine new facts are continually presenting themselves. These may be still comprehended within the same principle, or they may not. If they are, they furnish a stronger attestation to its truth. If they are not, they weaken or destroy the principle altogether. But it is no disparagement to us that our principle has failed. We are only concerned that the method by which we reached it is the right one, and then, though it fail, we are at liberty to arrive at a new principle, if we are able, by the same method; that is, to find some other matter of fact comprehensive of the newly discovered particulars, and to concede to it the character of a principle.

Such is the nature of medicine, that things which we have laid up in our minds as settled truths, often require to be modified by our future experience, and come at last to be rated many degrees below the value at which we originally prized them.

Nevertheless, we do not claim for medicine a liberty to transgress any of those landmarks which philosophy has set up to indicate the path of truth. Let it have no other principles, and no other method of arriving at them, but such as philosophy approves; only let no disparagement fall upon it (considering the nature of the things with which it is conversant), if, for just reasons, it be sometimes dissatisfied with principles which it once embraced, and seek to discover new ones.

Finally, then, as to general principles in medicine, let it be remembered that the mind must always seek to arrive at a matter of fact, and *there* only be content to take its rest. But it need not settle there longer than until the clear discernment of some other fact, more general and elementary, opens the way to a safe progression beyond it. Then the fact last discovered becomes the principle, and the other is only one of the several stages conducting to it.

In this mode of proceeding our knowledge may be *incomplete*, but it is never erroneous. The mind advances from fact to fact, resting on one as the stepping-stone to another, and feeling safe in the possession of the truth, although it may not be *all the truth* that is capable of being ascertained.

Every fact from which another fact is derived is in some sort a principle. To us it is a *first* principle as long as we are obliged to rest in it; but as soon as another fact is discovered which is prior to it, it loses its character of a *first* principle; and if it be a principle at all, it is only an *intermediate* one, the first being always that to which we know nothing prior.

ST. GEORGE'S HOSPITAL.

Two Cases of Abscess within the Cranium, discharging through the Ear.

CASE I.—Matthew Palin, ætat. 28, admitted August 24th, 1835, under the care of Mr. Hawkins, and giving the following history of his complaints. Three weeks ago he received a blow on the chest, in a quarrel, and, in attempting to retaliate, he struck his head against the edge of an open door. The blow caused a small wound on the right side of the forehead, with much ecchymosis, though with very

little external hæmorrhage. This caused intense pain in the seat of the blow, which in a few days became more general over the whole forehead. A week after the injury he became delirious, and made repeated attempts to destroy himself. It appeared also, from his wife's account, that he was habitually very low-spirited. Pulse rather quick and small; tongue white, but tolerably moist; slightly delirious, but easily roused from it. Complains of want of sleep, and frowns much, with an expression of much anxiety. It was with some difficulty that he was persuaded to remain in the hospital, on account of the apparently urgent nature of his symptoms.

V. S. ad ℥xii. statim.

Hydrarg. Submur. gr. iss.; Opii, gr. $\frac{1}{4}$. 6tis horis. Tinct. Opii, ♏xv. h. s. s. ex Aquæ Cyatho.

25th.—Blood not inflamed; pulse frequent, with less jerk, and weaker. Pain in the head not at all abated. Slept badly; slight wandering, and great restlessness.

It now appeared, from his own account, that he had been subject, for *some years*, to a discharge from his left ear (which is not, however, now perceptible), on the cessation of which he becomes quite deaf. It could not, however, be ascertained that he had had more pain on this side than on the other since the blow, but still Mr. Hawkins thought it very probable that there was an abscess in the brain, discharging by the ear, as the symptoms were different from those usually produced by suppuration following a recent blow.

℞ Acet. Morphiæ, gr. $\frac{1}{2}$; Carbon. Ammon. gr. v.; Mist. Camph. ℥iss. M. 3tis horis sumend. donec cessaverit dolor. Appl. Emp. Lyttæ Nuchæ. Beef-tea and arrow-root.

26th.—He has passed a better night, and though he says he is not free from pain, yet it is not so great as yesterday. Pulse more perceptible, but labouring. Seems a good deal under the influence of the morphia, of which three doses were given before the pain was relieved.

Cont. Acet. Morphiæ, gr. ss. p. r. n.

27th.—He was restless during the early part of the night, getting out of bed twice, to go to the water-closet; after this he became to a certain extent comatose, and died at 10 o'clock in the morning.

On examination of the seat of the injury there was found no mark of any mischief, either in the bone or its coverings; nor was there any extravasation in the brain. There was no unusual vascularity, and the quantity of fluid in the ventricles was not unusual. On carefully raising the left hemisphere of the cerebrum, it was found to be soft and discoloured, easily lacerable, and partly adherent to the middle fossa at the base of the cranium. In the interior of the middle and anterior lobes was a large abscess, containing about three ounces of foul pus; and the parietes were to some extent soft and green, as if nearly gangrenous. At the bottom of the abscess the brain adhered to the dura mater, covering the thin roof of the tympanum, in the centre of which a small opening in the dura mater allowed a probe to strike on dead bone, about the size of a sixpence; the dura mater to this extent being separate from the bone. With some difficulty a very small aperture was discovered in the dead bone, which allowed a lachrymal probe to enter the cavity of the tympanum, the membrane of which was partially destroyed.

It was thus manifest that he must have had the disease in the brain for a considerable time, the discharge from the abscess occasionally ceasing, as is usual in such cases, producing in him only deafness. The utmost that the blow could have done was to hasten his death, by exciting the abscess already existing. The man with whom he was quarrelling when he ran his own head against the door, is, however, to be tried as the cause of his death, though the only blow which he would seem to have given the deceased was, not on the head, but on the chest.

Case II.—October 2d.—Charles Young, aged 32, cabinet-maker, admitted under the care of Dr. Macleod. Complains of pain in the forehead, particularly above the right eye, extending to the ear and occiput, in which last situation it is extremely severe; has a pretty copious fœtid, thin, puriform discharge from the right meatus auditorius. Pulse 120, very feeble; skin cool and damp; tongue furred, clammy. Bowels much purged (probably from medicine taken before his admission); countenance collapsed; extreme prostration. Ill a month, and attributes the present attack to having sat in a draught after having had his hair cut.

Habeat Haust. Ammoniæ Effervescentem, 4tâ quâque horâ.

℞ Hydrargyri c. Cretâ, gr. v.; Pulv. Ipecac. Comp. gr. iij. M. sum. 8vâ quâque horâ. Broth diet.

3d.—Has rallied considerably; the purging has ceased, and his expression, though still anxious, does not indicate the same excessive exhaustion. Pulse 98, with more

power, but still very compressible. Discharge from the ear continues, and it now appears, from the testimony of his wife, that he has had it occasionally for about two years.

Dr. Macleod stated it to be his opinion that the discharge from the external ear originated in an abscess within the cranium, and that the patient would die.

Emplast. Lyttæ pone aurem dextram.
Rep. Hydrarg. c. Creta. Omitt. alia.

5th. — Pain of head considerably relieved, which he attributes to the blister. Has been rather drowsy this morning, but says this is owing to his being comparatively free from pain. Pulse 84, soft; tongue retains its fur, and the bowels did not act yesterday, nor have they done so to day.

Sumatur Haust. Sennæ quamprimum et repetatur quartâ quâque horâ donec responderit alvus; dein repetatur Hydrarg. c. Creta, ut antea.

6th.—Bowels freely opened by the first draught. Drowsiness continued during the whole of yesterday. In the evening he was attacked with pain, extending all over the right side of the head; for which ten leeches were applied to the temple with some relief. Gums sore.

Omitt. Hydrar. c. Creta. Habeat. Haust. Salin. 4tis horis.

7th. — Still complains of considerable pain in occiput and over right eye; is in a state of partial stupor; brows knit, and expression anxious; right eye particularly dull, but he sees distinctly with it; pulse 84, large; tongue dry and furred; urine scanty, and voided with difficulty.

Hirudines xiv. tempori dextro. Rep. Haustus.

8th.—Stupor increases; pulse 64, large and soft; bowels sluggish; urine still rather scanty; ptyalism receding; discharge from ear less abundant.

Haust. Sennæ, quamprimum; Calomel, gr. ii. 4ta quâque horâ. Emplast. Lyttæ Nuchæ.

9th. — Comatose; pulse 58; bowels have not acted; gums again becoming sore.

℞ Ol. Terebinth. ℥i.; Enematis Domestici, ℥vi. M. fiat Enem. quamprimum injiciendum.

Rep. Haust. Sennæ et Calomelas.

11th. — Gradually sank, and died at 1 P.M.

Autopsy, October 12th.—*Head.* Cerebrum and its investing membranes perfectly healthy; about an ounce of colourless fluid in the ventricles. Cerebellum, at the lower and anterior surface on right side, exhibited a spot of greenish colour, and size of a sixpence; in its centre was an aperture, sufficient to admit a small probe. This perforated the dura mater, and led into an abscess in the cerebellum, which, being laid open by a horizontal section, was found to be about the size of a very large walnut flattened; it contained dirty-looking, greenish, fœtid pus; and the parietes were of a similar appearance, but greater consistence, about a line in thickness, and gradually blending with the surrounding substance of the cerebellum. In the vicinity of the abscess were a few minute spots of extravasated blood. The aperture in the dura mater presented opposite to a corresponding perforation in the pars petrosa of the temporal bone, and leading into the internal ear, which was disorganised, and allowed the matter to flow by the external meatus. No other diseased appearance was found.

The cerebellum and corresponding portion of bone have been preserved in the museum of the hospital, and a drawing made of them by Dr. Hope.

Instances like the above, in which abscesses within the cranium communicate with the ear, are by no means very uncommon; and it is therefore of importance that we should keep in mind the necessity of a guarded prognosis in such cases. It has generally been assumed that the disease originates in the ear, and spreads to the parts within the cranium; but this is by no means well ascertained, and some of the circumstances would rather lead us to believe that the disease travelled in an opposite direction. We allude particularly to the fact that those cases in which the ear is unequivocally the primary seat of disease (as from cold, from the inflammation spreading along the Eustachian tube in scarlatina, or from foreign bodies irritating the external meatus), we very seldom hear of the brain becoming affected in this manner, although the internal ear may be so much disorganised as to produce permanent deafness.

ST. BARTHOLOMEW'S HOSPITAL.

Fibrous Tumor connected with the lower Jaw—Removal of the left half of the Jaw.

The operation of removing the left half of the lower jaw was performed by Mr. Stanley, on Saturday, the 10th instant. The following is a report of the case at the time of admission.

A. B., a girl aged 15. She states that the disease has been twelve months in progress, and that it has not been attended with pain, or disturbance of the general health. A tumor now occupies the whole of the left side of the jaw, from the angle to within half an inch of the symphysis. The length of the tumor, therefore, corresponds with this extent of the jaw, and in its broadest part it measures a full inch and a half. The tumor is but little apparent on the outside of the face; its principal growth having taken place towards the cavity of the mouth: it is smooth, and does not yield to the firmest pressure; its upper surface is slightly ulcerated. The absorbent glands and other parts in its neighbourhood are healthy. In an early stage of the disease, with the idea that the tumor contained fluid, a lancet was plunged into it; this was followed by a considerable flow of blood, but had no other effect. With the gradual increase of the tumor, the fangs of the teeth were successively imbedded in it. Mr. Stanley had removed several of these loosened teeth, and by introducing a probe into the centre of the tumor, had ascertained it to be a growth from the cancellous texture of the jaw. Before the admission of the patient into the hospital, Mr. Stanley had tried iodine freely for a fortnight, both as a local application and internally, but it had no influence upon the disease, which was still slowly increasing, and accordingly its removal was undertaken.

An incision commencing an inch in front of the extremity of the lobulus of the ear was continued in a semicircular line downwards and forwards over the base of the jaw, and then upwards upon the chin to within about an inch of the orifice of the mouth. The division of the cheek was followed by the detachment of all the contiguous parts from the tumor, and by the exposure of the jaw beyond it sufficiently to permit the division of the sound part of the bone. With a small saw a groove was made in the jaw close to the symphysis, and with the large cutting forceps the division of the bone was here completed. With the forceps alone the division of the ramus of the jaw was easily effected a little above the angle; it now only remained to divide the mucous membrane of the mouth where it was continued upon the inner side of the tumor. Very little hæmorrhage had occurred, only two arteries requiring to be tied, the facial and one of its branches. The divided surfaces of the cheek were retained in contact by hare-lip pins and fine silk ligatures. Scarcely any constitutional derangement followed the operation. The whole tract of the wound united by the first intention; and on the eighth day the patient, in good health, had left her bed, and was moving about the ward. The cicatrix being a mere line, occasions but the slightest disfigurement of the face. There is no falling inwards of the cheek. The articulation is perfect, and already the muscles can act upon the remaining half of the jaw with so much force, that with a little more use of the parts, she will be able to masticate the firmest substances.

Upon examination, the tumor was found to have originated in the cancellous texture of the jaw, and in its progress, to have caused, first the expansion and then the partial absorption of the walls of the bone, some portions of which still contribute to form the boundaries of the tumor. The section of the tumor shewed that it consisted throughout of a dense greyish substance, irregularly intersected by white lines. In its colour and texture, this specimen of morbid growth from the jaw closely resembles the common fibrous tumor of the uterus.

Mr. Stanley stated that every circumstance in the history of the disease indicates that it is not of a malignant nature.

ELECTION TO THE COUNCIL OF THE COLLEGE OF SURGEONS.

APPEAL OF MR. KINGDON.

To the Editor of the Medical Gazette.

Sir,

Will you have the goodness to grant insertion of the accompanying letter in your journal of next Saturday, and oblige,

Sir,

Your very obedient servant,

W. Kingdon.

2, New Bank Buildings,
Oct. 26, 1835.

To the Medical Profession.

Gentlemen,—Having of late been treated with injustice by the *majority* of the Council of the College of Surgeons, and having learnt that he who tacitly submits to injustice becomes an instrument to his own degradation, I take this, I believe the most proper, method to protest against the measure of exclusion which has been practised towards me. Thinking that such a body would not willingly treat any one with injustice, I suspected there must have been, unknown to me, some disparaging report abroad, and therefore addressed the

President and Council, to inquire if, to the belief of any of them, such report existed.

After some time, I received an answer, avoiding the question; and thus, in effect, admitting that if they allowed me an unsullied reputation they damnified their own conduct. On the same showing, however, their conduct was calculated to damnify my fair fame, more valued and estimated by me, as more essential than life itself to the well-doing, both present and future, of myself and family. Without power to question this conduct, shielded as it is by charter, granted (we may presume) by a gracious Sovereign, for the benefit, and not the oppression, of his subjects, I still have the power possessed by every English gentleman, to see that my reputation do not suffer by the selfish or partial conduct of others; and it is for this reason that I now address my professional brethren. After more than twenty years of public and private practice as a surgeon in London, I must be known to some of you, and I address you to entreat that if any of you know, or think you know, any thing disparaging to my character, you will have the goodness to inform me, and thus confer the greatest favour that it is possible for man to confer on man; for I am conscious that any such disparagement need only be made known, to be proved as resting on misapprehenson or founded on falsehood. Having avoided all legal ineligibility to a seat at the Council of my College, the exclusion would seem to imply that I have by some act, either professional or otherwise, rendered myself unfit for election.

Unless erroneous information respecting me, given to the Parliamentary Committee by the then President of the College (which he afterwards made all endeavours to correct that gentlemanly feeling could prompt), has been deemed a sufficient cause to throw me out of my fair professional course,—or unless my not having an interest in common with surgeons of hospitals, or a mind easily led to surrender its own views, and see things as others wish,—be deemed a sufficient cause, (and neither of these can be supposed to influence the minds of honourable men), I must remain under the stigma of exclusion for my own demerit, did I not make this appeal to my professional brethren, so as to make known to them that on the majority of the Council rests the responsibility of having used their power unjustly. Such conduct needs only to be canvassed for the reprobation of the right-thinking to fall on those who practise it; and I have felt it my duty to offer my character for the strictest investigation, in order that the *majority* of the Council may have the benefit, if any thing can be found against it, as an excuse for their unprofessional and unjust conduct. It is the conduct of such men that drives the quiet and the peace-lover to desire and enforce change. It is the conduct of such men that renders futile the best efforts of the honourable and the able to place the affairs of our country on a footing of fairness and stability.

I am, gentlemen,
Very faithfully yours,
W. Kingdon.

2, New Bank Buildings,
Oct. 26, 1835.

ROYAL COLLEGE OF SURGEONS.

Mr. J. H. Green has been elected a member of the Council of the College, in the room of Sir W. Blizard, deceased.

TIEDEMANN.

This very distinguished physiologist, after a sojourn of about two months in this country, left London on Saturday last to return to Heidelberg. We are glad to learn that previous to his departure he sat for his portrait to an eminent artist, and that a striking likeness of him has been taken. Mr. Schloss, we understand, will shortly publish a lithographic copy of the portrait.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *Oct.* 27, 1835.

Abscess	4	Bowels & Stomach	11
Age and Debility	51	Brain	5
Apoplexy	12	Lungs and Pleura	2
Asthma	16	Insanity	2
Cancer	2	Liver, diseased	4
Childbirth	1	Measles	9
Consumption	91	Mortification	7
Convulsions	32	Paralysis	4
Croup	3	Small-Pox	18
Dentition or Teething	22	Sore Throat and Quinsey	1
Dropsy	21	Spasms	2
Dropsy on the Brain	19	Stone and Gravel	1
Dropsy on the Chest	1	Stricture	1
Erysipelas	1	Thrush	2
Fever, Scarlet	6	Tumor	2
Fever, Typhus	1	Worms	1
Gout	1	Unknown Causes	32
Hæmorrhage	4		
Heart, diseased	5	Stillborn	26
Hooping Cough	5		
Inflammation	35		

Increase of Burials, as compared with the preceding week . . . 176

METEOROLOGICAL JOURNAL.

(Not come to hand.)

ERRATUM.

In Dr. Rigby's report last week, page 124 (in part of our impression), for *Double pregnancy*, read *Doubtful pregnancy*.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 7, 1835.

LECTURES
ON
MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE VI.

GENTLEMEN,—The subject of this day's lecture is pharmacological classification.

Writers on medical botany frequently follow no order in treating of plants used in medicine. I may refer you to the works of Woodville, of Stephenson and Churchill, of Bigelow, and of Barton, as examples. The difficulty of procuring botanical specimens in regular succession is the only excuse that can be offered for this mode of proceeding.

The substances employed in the treatment of diseases being very numerous, I conceive any kind of arrangement of them is better than none. The principles on which authors have proceeded in effecting pharmacological classifications are very different: some have arranged medicines alphabetically; others according to their physical, natural-historical, chemical, or medicinal qualities. We may very conveniently make two classes of these arrangements:—

Class I.—Including those which we may term *empirical.*

Class II.—Those which we may, in contradistinction, call *rational.*

CLASS I.—By *empirical* arrangements I mean those founded on circumstances which have no real relation or connexion with the substances to be arranged, and which, therefore, are independent of the nature of these bodies. For example, an *alphabetical* order is empirical, since it is founded on names, which are arbitrary, and have no real relation to the substances they are used to designate. A considerable number of pharmacologists, both English and Foreign, have adopted this method, by which it was supposed two advantages were gained—namely, a ready reference to any particular subject; and secondly, an avoidance of errors committed by all writers who adopt other methods.

But I think it will not be very difficult to demonstrate that no peculiar advantage is gained by it. In the first place, the ready reference is more imaginary than real; for every substance is known by more than one name, and hence an index becomes as necessary to this as to any other arrangement; in proof of which I may refer to the Dispensatories of Duncan and Thomson, both of which, though arranged alphabetically, have their indexes. Now an index gives us every advantage of an alphabetical arrangement, and therefore, if appended to any other modes of classification, renders the latter as convenient for reference as the former. Secondly, the alphabetical order has also its disadvantages. The most important are, that it brings together substances of the most incongruous natures, and separates those which accord in most of their properties; in addition to which, it is totally unfit for an elementary work, since, by its want of order, it distracts the attention of the student.

The following are some of the authors who have adopted it:—

1. *In England.*—Lewis, Paris, Brande, Ainslie, A. T. Thomson (Dispensatory), and Duncan.

2. *In France.*—Bodard, Hipp. Cloquet (Faune), Chevallier, Richard and Guillemin (Dict.), Martinet, Ratier, and Merat and De Lens (Dict.)

3. *In Germany.*—Jahn, Horn, Schmidts, Hahnemann, Segniss, Hartlaubs and Trinks, and Dulk.

Class II.—By *rational arrangements* I mean those having some connexion with the bodies to which they are applied, and which are founded on some of their properties; they are the classifications properly so called. Being, then, founded on the qualities of the bodies to be arranged, they must necessarily be as various as there are classes of properties. Now a medicine has sensible properties (such as colour, taste, and smell); it has natural-historical properties (external form and structure); chemical properties; and lastly, it excites certain alterations in the functions of the body, for which we employ it medicinally. Rational arrangements, therefore, may be physical or sensible, natural-historical, chemical, or physiological. Let us, then, take a general view of each of these.

I. *Classifications founded on the sensible properties of the materia medica* are to a certain extent useful, though I have already shown you that these properties give us very little information regarding the effects which different substances produce on the system. The latest and best arrangements of this kind are, one by Dr. Osborne, in the fifth volume of the Transactions of the Association of the Fellows and Licentiates of the King and Queen's College of Physicians; and one by Mr. A. F. A. Greeves, to which the prize was awarded in the class of materia medica in the University of Edinburgh; it was published by the late Dr. Duncan, in the Supplement to the Edinburgh New Dispensatory, and is certainly the best of the kind I have seen. The following is a tabular view of it:—

Mr. Greeve's Classification.

Classes.	Families.	Orders.
I. Inodorous and insipid	1. Liquid	1.
	2. Soft	1. Pulverescent. 2. Unctuous.
	3. Hard	1. Tough. 2. Brittle.
II. Inodorous and sapid	1. Sweets	1. Saccharineæ. 2. Amylaceous. 3. Mucous or Unctuous. 4. Faint. 5. Frugous.
	2. Bitters	1. Mawkish. 2. Astringent. 3. Pure bitter. 4. Austere. 5. Styptic. 6. Acrid. 7. Salino-amaræ.
	3. Alkalines	1.
	4. Acids	1. Pure acid. 2. Saccharo acid.
	5. Salines	1. Pure salt.
III. Odorous and insipid	1. Fragrant	1. Sweet. 2. Aromatic.
IV. Odorous and sapid	1. Sweets	1. Saccharineæ. 2. Faint. 3. Sweet spicy.
	2. Bitters	1. Mawkish. 2. Subastringent. 3. Bitter spicy. 4. Sharp bitter. 5. Austere. 6. Subacrid. 7. Acrid.
	3. Aciduous	1.
	4. Camphreous	1. Camphreous aromatics. 2. Savoury. 3. Terebinthinatæ. 4. Camphreous.
	5. Spirituous	1. Vinous. 2.

II. *Natural-historical classifications* are much more useful than those already mentioned; yet they are open to several objections, of which the principal are the following:—In some instances substances of dissimilar operation are brought together, while others are separated whose effects are very analogous; and, as the ultimate object of all our inquiries into the materia medica is to obtain a knowledge of the medicinal effects, a natural-historical arrangement, it may be said, offers inferior advantages when compared with a classification founded on the operation of medicines. Notwithstanding these objections, I think it is by far the best method for lectures, and is the one which I shall adopt. Without asserting, or pretending to assert, that this is without real and solid objections, yet I cannot help thinking there are much more weighty reasons against other modes of arrangement than against this. I believe, also, that the principles on which natural-historical classifications are founded are such, that these arrangements have attained a greater degree of perfection. Added to which, other reasons render this mode preferable in lectures, but which, perhaps, do not equally apply to writings: I allude to the variety of interesting points of natural history which it allows us to introduce; added to which, we avoid the inconvenience arising from having to speak of the same substance several times under different heads. These circumstances have led me to adopt the natural method in these lectures. I shall, therefore, on the present occasion, enter into no details respecting it, as the principles of this arrangement will be fully discussed hereafter. I regret that, with the exception of a little work by Dr. Johnstone, of Birmingham, all the books to which I can refer you wherein this method has been followed are Foreign. The following are some of the best:—

1. *French.*—The works of De Candolle, Fee, De Smyttère, and Richard.

2. *Germans.*—The publications of J. A. Murray, Gmelin, Tode, Cassel, Kapp, Nees von Esenbeck, Dierbach, and Brandt and Ratzburg.

III. I must next direct your attention to *chemical classifications*, which are employed by some of the German pharmacologists; for, with the exception of Donald Monro's work, I know of no systematic work, either French or English, in which this method has been followed. In fact, these arrangements, although of value to the chemist, are of little use to the medical practitioner. This, I believe, principally arises from the difficulties and uncertainties experienced in the analysis of organic substances; for, as I have already remarked, the chemical properties of bodies give us very little insight into their physiological effects. No one, for example, could suspect, from chemical analysis, the poisonous property of the virus of the viper, or of the mad dog. I conceive, also, the establishment of the doctrine of *isomerism*, that identity of composition is not always attended with identity of properties, will diminish still more the value of chemical classifications. The following are some of the German writers who adopt this mode of arrangement:—Klose, Pfaff, Gren, Voigtel, Hufeland, Hecker, Schwartz, Richter, Hoffman, and Bischoff.

As an example of a chemical arrangement, I shall select that adopted by Schwartze, in his *Pharmacologischen Tabellen.*

Schwartze's Classification.

DIV.

1. Aqua communis.
2. Gummosa, mucilaginosa.
3. Farinosa, amylacea.
4. Gelatinosa.
5. Albuminosa.
6. Saccharina.
7. Pinguia—Oleosa.
8. Extractiva amara.
9. Adstringentia seu Tannica.
10.Ætherea—Oleosa.
11. Resinosa.
12. Narcotica.
13. Spirituosa.
14. Acida.
15. Alcalina.
16. Salina.
17. Metallica.
18. Corpora simplicia, solida, non metallica.
19. Kalia sulphurata.
20. Sapones.

You will observe that the author has not always founded his divisions on the chemical properties of medicines; some of them refer partly or wholly to the effects produced by these agents on the body. The nomenclature is not always perfect: thus, his seventeenth class is called "metallica," as if it alone contained metallic substances; whereas division sixteen and seventeen also contain them. Again, some of the divisions, for example "resinosa," contain substances whose effects are most dissimilar; while substances of analogous operation are placed in separate divisions.

One of the latest writers on the materia medica among the Germans, is Dr. C. H. E. Bischoff, who has published an extensive work, entitled "*Die Lehre von den Chemischen Heilmitteln oder Handbuch der Arzneimittellehre.*" He has arranged the articles according to their supposed chemical, or rather electro chemical, properties. The following is a tabular view of his arrangement:—

Class I.—Negatively electrical medicines of basic quality.

Orders.

1. Gas hydrogenium.
2. Oleum æthereum animale.
3. Ætheres et acida dulcificata.
4. Alkohol.
5. Ammonium.
6. Ammonium sulphuratum.
7. Gas hydrogenium sulphuratum.
8. Gas azoticum oxydulatum.
9. Oleum æthereum vegetabile.
10. Camphora et anemonin.
11. Oleum æthereum empyreumaticum animale.

Class II.—Indifferently electrical medicines of neutral quality.

Orders.

1. Acidum hydrocyanicum.
2. Narcotica fixa.
3. Acrid.
4. Sulphur.
5. Metalla cyanata, sulphurata, oxygenata, atque salita.
6. Iodeum, iodeum hydrogenatum, salia hydriodica et cyanatum.
7. Pinguedo, [*Fett.*]
8. Resina.
9. Gelatina.
10. Albumen.
11. Mucus, [*Schleim.*]
12. Farina et fecula farinæ.
13. Saccharum.

Class III.—Positively electrical medicines of acid quality.

Orders.

1. Acidum aromaticum.
2. Materies extractiva.
3. Tannicum.
4. Carbo.
5. Acida composita (vegetabilia) et salia neutra.
6. Aquæ soteriæ acidulæ.
7. Acida mineralia.
8. Medicamina halogenia eorumque acida et salia cum aquis soteriis muriaticis.
9. Aquæ soteriæ martiales, ferrum et plumbago.
10. Oxygenium.

This classification is objectionable even on chemical grounds. Sulphuretted hydrogen is not a *basic*, but an acid body; hydrocyanic acid is not *neutral*. The orders 2 and 3 of class the second are not founded on the chemical properties of the substances. These are only a few of the errors pervading this arrangement.

IV. I now come to *physiological classifications*, which, in theory, I hold to be the best of all; but in practice, far inferior to the natural-historical: that is, I think we have at present too limited an acquaintance with the effects of medicines, to enable us to form any classification founded on them which can be either correct or of much value. Every writer who has attempted to form one, has been obliged to introduce theory; and, therefore, we find the so-called physiological arrangements are really founded on the prevailing doctrines of the day, to all the errors and fallacies of which they are liable. Thus, Dr. John Murray, Oberreich, Chortet, Müller, Schöne, and others, adopt the Brunonian doctrine, that all medicines are stimulants; and hence the very basis of their arrangement is hypothetical. Another class of writers assume that there are two, and only two, classes of medicines—stimulants and contra-stimulants; or stimulants and asthenics. Here, again, we have hypothesis. By some writers—Barbier, for example—the operation of all medicines is assumed to depend on their chemical properties; by others, the very reverse assumption is made, and a class of substances is admitted under the name of vital agents, supposed to act independently of their chemical properties. Barbier asserts that laxatives relax (as their name implies); while the followers of Broussais assert they irritate. At one period medicines were presumed to act both on the solids and the fluids. Subsequently this was denied, and the operation of all was referred to the solids. Yet, as I have before stated, we have just as good grounds for believing that some agents primarily alter the quality of the blood, as to suppose that they act on the solids. Thus, then, you see there are many points yet unsettled which require to be known ere a correct physiological classification can be expected.

In a course of lectures, I conceive other objections exist to the adoption of an arrangement of this kind. Most medicines produce several effects, and we should be obliged to speak of the same remedy under several heads; for example, squills being emetic, diuretic, and expectorant, would require to be brought three distinct times under examination, which, to say the least of it, would be an inconvenience. Again, substances are often associated to form a class, in consequence of their agreeing in one or more effects, whose general operation, however, is very different. Thus, foxglove, squills, urea, and nitre, are all diuretics, yet their general action on the system is very different.

The following are some of the writers who have adopted this mode of arrangement:—

1. *In England*—Cullen, Pearson, Young, Murray, Duncan, A. T. Thomson, and Johnstone.

2. *In France*—Vitet, Chortet, Menard, Schwilgué, Barbier, Alibert, Edwards and Vavasseur, and Foy.
3. *In Germany*—Sift, Gesenius, Monch, Frank, Oberreich, Müller, Neurohr, Schöne, Hartmann, Arnemann, Hermann, Pachur, Sundelin, Vogt, Wendt, &c.
4. *In America*—Eberle, and Chapman.

There are two modes of classifying medicines according to their effects; the first is to place together all those substances which concur in affecting a particular organ or structure; the second is to group medicines according to the quality of their effects. For example, when we make a class of "substances acting on the nervous system," we adopt the first method; whereas, if we form a class of "narcotics," we follow the second. The classification of Alibert belongs to the first method.

Alibert's Classification.

I. Medicines acting in a special manner on the vital properties of the digestive passages.

1. Medicines acting in a special manner on the tonicity or fibrillary contractility of the system of the digestive passages. [*This includes tonics.*]
2. Medicines acting in a special manner on motility or muscular contractility of the system of the digestive passages. [*Here are placed emetics and purgatives.*]
3. Medicines proper for combating the alterations of the vital forces which result from the presence of worms, or of poisons in the digestive passages in the stomach or in the intestinal canal. [*Anthelmintics and antidotes.*]
4. Medicines acting in a special manner on the vital properties of the large intestines. [*Enemata.*]

II.—Medicines acting on the vital properties of the urinary passages. [*Including diuretics.*]

1. Medicines acting in a direct or special manner on the vital properties of the urinary passages.
2. Medicines acting in an indirect or sympathetic manner on the vital properties of the urinary passages.

III.—Medicines acting in a special manner on the vital properties of the system of respiration.

1. Medicines acting in a special manner on the vital properties of the system of respiration, to free it from the superabundant matters which oppress it. [*Expectorants.*]
2. Medicines acting in a special manner on the vital properties of the system of respiration, by the aid of pneumatic apparatus.
3. Medicines acting in a special manner on the vital properties of the system of respiration, when their exercise is suspended by the phenomena of asphyxia.
4. Medicines acting in a special manner on the vital properties of the system of respiration, to moderate the excess of animal heat. [*Refrigerants.*]

IV.—Curative means specially directed to the vital properties of the system of circulation.

1. Curative means specially directed to the vital properties of the circulation of black blood. [*Phlebotomy, leeches, and scarifications.*]
2. Curative means specially directed to the vital properties of the circulation of red blood. [*Arteriotomy.*]

V.—Medicines acting in a special manner to the vital properties of the brain and nervous system. [*Narcotics.*]

VI.—Medicines acting in a special manner on the vital properties of the sensitive organs.

1. Of the organ of vision.
2. Of the organ of hearing.
3. Of the organ of smell. [*Errhines and sternutatories.*]
4. Of the organ of taste. [*Sialogogues and masticatories.*]

VII.—Medicines acting in a special manner on the vital properties of the tegumentary system.

1. Considered as an organ of absorption. [*Iatraleptic medicine.*]
2. Considered as an exhalent organ. [*Diaphoretica.*]
3. Considered as a sensible organ. [*Epispastics, electricity, animal magnetism, acupuncture, baths, &c.*]

VIII.—Medicines acting in a special manner on the vital properties of the productive system.

1. Of the male genital apparatus. [*Aphrodisiacs.*]
2. Of the female genital apparatus. [*Emmenagogues.*]

Dr. Osann has given an example of a similar mode of classification in the "*Encyclopädisches Wörterbuch der Medicinischen Wissenschaften.*"

In 1822, Dr. Granville proposed a new classification of the Materia Medica; the primary divisions being founded on the tendency which medicines have to affect particular organs or systems of structure, while the subdivisions are formed from the *quality* of the effect. This plan formed the ground-work of the classification adopted by Dr. Eberle, an American writer, in his "*Treatise of the Materia Medica,*" published in 1824.

Eberle's Classification.

A.—Medicines that act specifically on the intestinal canal, or upon morbific matter lodged in it	I. Medicines that excite discharges from the alimentary canal	Emetics. Cathartics.
	II. Medicines calculated to destroy or counteract the influence of morbific substances lodged in the alimentary canal	Anthelmintics. Antacids.
B.—Medicines whose action is principally directed to the muscular system	I. Medicines calculated to correct certain morbid conditions of the system, by acting on the tonicity of the muscular fibre	Tonics.
	II. Medicines calculated to correct certain morbid states of the system, by acting on the contractility of the muscular fibre	Astringents.
C.—Medicines that act specifically on the uterine system	I. Medicines calculated to promote the menstrual discharge	Emmenagogues.
	II. Medicines calculated to increase the parturient efforts of the womb	Abortiva.
D.—Medicines that act specifically on the uterine system	I. Medicines that lessen the sensibility and irritability of the nervous system	Narcotics.
	II. Medicines that inecase and equalize the nervous energy	Antispasmodics.
E.—Medicines whose action is principally manifested in the circulatory system	I. Medicines that increase the action of the heart and arteries	Stimulants.
F.—Medicines acting specifically upon the organs of secretion	I. Medicines that act on the cutaneous exhalents .. General ..	Diaphoretics.
	I. Medicines that act on the cutaneous exhalents .. Topical ..	Epispastics. Errhines. Emollients.
	II. Medicines that increase the action of the urinary organs	Diuretics.
	III. Medicines that alter the state of the urinary secretion	Antilithics.
	IV. Medicines that promote the secretory action of the salivary glands	Sialagogues.
G.—Medicines that act specifically upon the respiratory organs	I. Medicines calculated to increase the mucous secretion in the bronchia, and to promote its discharge	Expectorants. Inhalations.
	II. Medicines whose action is truly topical	Emollients. Escharotics.

Notwithstanding that several of its details are objectionable, the plan of this classification appears to me an exceedingly good one.

I proceed, in the next place, to examine those arrangements, the basis of which depends on the quality of the effects produced: locality of operation being a secondary consideration. The number of these which have been proposed within the last few years is so great, that were I to attempt to take a general review of all of them, I conceive I should be very unprofitably consuming your time; as I should have to devote a considerable portion of this course of lectures to the subject. I propose, therefore, to confine my remarks to one or two of the leading classifications of England, France, and Germany; beginning with that of the late Dr. John Murray, which, with a few alterations, might, I conceive, still be unrivalled for its practical value and simplicity.

Murray's Classification.

A. General Stimulants :—

- (a) *Diffusible* — Narcotics. Antispasmodics.
- (b) *Permanent* — Tonics. Astringents.

B. Local Stimulants :—

Emetics.
Cathartics.
Emmenagogues.
Diuretics.
Diaphoretics.
Expectorants.
Sialogogues.
Errhines.
Epispastics.

C. Chemical Remedies :—

Refrigerants.
Antacids.
Lithontriptics.
Escharotics.

D. Mechanical Remedies:—
Anthelmintics.
Demulcents.
Diluents.
Emollients.

The principal objections to this arrangement are the following:—1st. The general operation of medicines is assumed to be stimulant; of which we have no proof in the case of hydrocyanic acid, of foxglove, or of tobacco. 2dly. The distinction into general and local stimulants is bad; for narcotics are unquestionably in their primary operation, local agents; and drastic purgatives, emetics, &c. when given in full doses, affect the general system. 3dly. The terms antispasmodic, antacid, lithontriptic, and anthelmintic, refer to the therapeutic, and not to the physiological effects of medicine. 4thly. It is an assumption to say that refrigerants are chemical remedies. 5thly, emollients are not mere mechanical agents.

The classification proposed by the late Dr. Duncan is, in many respects, an improvement of that of Murray.

Duncan's Classification.

External agents act,

1. By nourishing the body—Alimenta.
   (*a*) Drink—*Potus.*
   When they act medicinally—*Diluentia.*
   (*b*) Food—*Cibi.*
   When they act medicinally—*Demulcentia.*
2. By evacuation—Evacuantia.
   (*a*) By the skin insensibly—*Diaphoretica.*
   ———— sensibly—*Sudorifica.*
   (*b*) By the mucous membrane of the nostrils—*Errhina.*
   By the mucous membrane of the lungs—*Expectorantia.*
   By the mucous membrane of the stomach—*Emetica.*
   By the mucous membrane of the intestines—*Cathartica.*
   By the mucous membrane of the uterus—*Emmenagoga.*
   (*c*) By glandular secretion:
   The kidneys—*Diuretica.*
   The salivary glands—*Sialogoga.*
3. By exciting the vital powers—Stimulantia.
   (*a*) Chiefly of the parts to which they are applied—Topica.
   Applied externally,
   Causing redness—*Rubefacientia.*
   ——— serous secretion — *Vesicantia.*
   ——— purulent secretion—*Suppurantia.*
   Administered internally,
   Condimenta, when alimentary.
   When acting medicinally—*Carminativa.*
   (*b*) Of the system generally—Generalia.
   (*α*) Obscurely, but more durably—Permanentia.
   Producing no immediate effect—*Tonica.*
   Constricting fibres, and coagulating fluids—*Astringentia.*
   (*β*) More evidently, but less durably—Transitoria.
   Acting on the organic functions —*Calefacientia.*
   Acting on the mental functions —*Inebrientia.*
4. By depressing the vital powers—Deprimentia.
   Acting on the organic fuctions—*Refrigerantia*
   Acting on the mental functions—*Narcotica.*
5. By chemical influence on the fluids—Chemica.
   Acidifying—*Acida.*
   Alkalizing—*Alkalia.*

Like every other physiological arrangement proposed, this has its objections. In the first place, a distinction of classes is sometimes made without much difference; for example, diaphoretica and sudorifica; also the rubefacientia, vesicantia, and suppurantia; and to these we may add the calefacientia and carminativa. Nux vomica cannot be said either to depress the general system or to act on the mental functions: yet Dr. Duncan classes it among the narcotica. Opium is sometimes used to inebriate, as by the Turks; at other times to cause sleep. In the first case it would, by this classification, be considered as an excitant, and placed among the inebriants; whereas, in the second case, it would be presumed to depress, and therefore be ranked among the narcotics.

Dr. A. T. Thomson has, in his "*Elements of Materia Medica*," offered a classification for the most part founded on that of Dr. Murray, but freed from some of the objections to which I have already stated the latter is liable. Thus, in place of making the two divisions, general and local stimulants, he makes only one; which he calls, after Dr. Young, "vital agents."

These are all the physiological classifications proposed in this country, which I think it necessary to notice.

Barbier, one of the leading pharmacological writers of France, has offered the following arrangement:—

Barbier's Classification.

		Classes.
Medicines	which strengthen the tissue of organs	1. Tonics.
	which stimulate the tissue of organs	2. Excitants. 3. Diffusibles.
	which relax the tissue of organs	4. Emollients.
	which moderate the too great activity of organs	5. Temperants.
	which diminish cerebral life	6. Narcotics.
	which irritate the internal surface of the intestines	7. Purgatives.
	which irritate especially the gastro-duodenal surface	8. Emetics.
	which disorder the natural movements of the intestines,	9. Laxatives.
	whose action is not well determined, or which cannot be referred to any of the preceding classes	10. *Incertæ sedis.*

There are several parts of this classification which appear to me objectionable. Some substances called narcotics do not, under all circumstances, diminish the activity of the cerebral functions; on the contrary, in moderate doses, they occasionally have an opposite effect. I do not think it expedient, or proper, to separate purgatives and laxatives; and I conceive that several of the substances inserted under the head of *incertæ sedis*, might, with great propriety, be placed in some of the preceding classes.

The next classification which I propose to bring under your notice is that of a German physician, Dr. Vogt, the author of an exceedingly good work, entitled *Lehrbuch der Pharmakodynamik*. I regret, however, that I cannot say much in favour of his classification. He makes three classes; the first including those agents which specially affect the *sensibility* of the body, the second containing those which alter the *irritability* of the system, and the third embracing those agents which influence what he calls the *vegetation* of the body—that is, the organic functions; namely, nutrition and reproduction. The following is a tabular view of the classes, and their principal subdivisions:—

Vogt's Classification.

	Orders.	Divisions.
Class 1. Medicines operating specially on the nervous system, and particularly used as nervous agents	1. Medicines which limit the vital manifestation of the nervous system (*Narcotica*)	1. Opium and its allies. 2. Nux vomica, and medicines similar to it. 3. Hydrocyanic acid, and vegetables allied to it. 4. Belladonna, and medicines similar to it.
	2. Medicines which exalt and strengthen the vital manifestations of the nervous system (*Nervina*)	1. Nervinia volatilia (ammonia, musk, &c.) 2. Nervino-alterantia antispasmodica (ipecacuanha, copper, zinc, bismuth, &c.)
Class 2. Medicines operating specially on irritable life [*auf das irritable Leben*]	1. Weakening (*Antiphlogistica*), as the neutral salts, cold, &c.	
	2. Medicines which heighten and strengthen the vital manifestations of the irritable system.	1. Excitantia volatilia (as camphor, mints, &c.) 2. Tonica. 3. Antiseptica (acids, chlorine, &c.)
Class 3. Medicines operating specially on the vegetative [organic] system, and which are particularly used in diseases of vegetation [nutrition and reproduction]	1. Medicines operating specially on the secreting and excreting systems	1. Heat. 2. Gummi—resinosa, balsamica, and resinosa. 3. Resolventia (acrids, mercury, antimony, sulphur, alkalies, iodine, &c.)
	2. Medicines which specially operate on the formative process.	1. Aromata (pepper, pyrethrum, nutmegs, &c.) 2. Nutrientia.

You will observe that the author does not confine his orders to those drawn from physiological effects, but constitutes some (antiphlogistica and antiseptica) which have a direct reference to disease.

These, however, are not the sole objec-

tion to this arrangement. When we come to examine the division of his orders, we feel astonished that an intelligent writer like Vogt should huddle substances of such different properties (as aconite, ergot of rye, croton oil, cantharides, squills, mercury, antimony, sulphur, alkalies, iodine, &c.) into one subdivision, under the very objectionable term *resolventia*. In its common acceptation this word signifies local agents applied to remove enlargements supposed to depend on obstruction. It refers, therefore, evidently to hypothetical conditions, and has no direct connexion with physiological effects.

On the other hand, we find substances whose effects are somewhat analogous, placed in different parts of the classication. Take the following as examples: belladonna and stramonium are placed under narcotica, while aconite and conium are classed among resolventia. Ammonia, musk, and castoreum, are called nervinia volatilia, and placed among substances exerting a specific influence over the nervous system; while camphor, alcohol, and æther, belong to the excitantia volatilia; rosemary is called narcotic; the mints volatile excitants.

I do not think it necessary, gentlemen, to bring before you any further examples of classifications of the articles of materia medica, founded on their operation on the body. The examples I have selected are from the best English, French, and German writers, and I have endeavoured to prove to you that not one of them can be regarded as unexceptionable. Our *positive* knowledge of the action of medicines is, in fact, too limited to allow us to form classifications of this kind independently of theory.

Physiological Classes, or Groups. — Certain associations or groups of medicines are by general consent admitted, though it is oftentimes difficult to define accurately the boundaries of each. In describing the effects of the individual articles of the materia medica, I shall, to avoid repetition, frequently employ terms which have reference to the common effect of each of these groups, or classes. Many of these terms require no explanation, as they are in common use, and their acceptation is generally agreed on; for example, emetic, purgative, &c. There are others, however, which require some explanation, since different authors are not agreed on the sense in which they are employed. I propose, therefore, in the next lecture, to offer a few explanatory remarks on these.

CASE OF

EXTRA-UTERINE PREGNANCY,

IN WHICH THE FŒTUS WAS REMOVED FROM THE ABDOMEN BY INCISION,

Fourteen Months after Conception.

To the Editor of the Medical Gazette.

Sir,

If the following case of removal of an extra uterine fœtus from the abdomen, should appear to you sufficiently interesting for insertion in your journal, you will oblige me by giving it publicity.—I am, sir,

Your obedient servant,

F. Hutchinson.

92, Farringdon-street,
Oct. 26, 1835.

Mrs. J——, æt. 28, living at 5, Butcher-hall Lane, of an active disposition and healthy constitution, has been married eleven years without having borne a child, and was always perfectly regular in regard to the catamenia until August 4, 1834, when that discharge appeared for the last time. In consequence of its suspension she considered herself pregnant, and was confirmed in that opinion as well by the declaration of the medical gentleman to whom she applied for advice, as by her own feelings. His statement was, "that she had every symptom attendant on such occasions." From the beginning of September, she states that the breasts enlarged considerably, and that a milky fluid was secreted in such quantity as to be readily squeezed out on the application of pressure; the areolæ around the nipples also were deeply shaded. Her person enlarged more rapidly than is usual in early pregnancy, so that by the middle of September she had acquired a considerable bulk. It was remarked that the abdomen was more prominent on the left side than on the right. She now became the subject of occasional spasmodic pains in the back and the region of the stomach; and so violent were these attacks as to produce syncope and deprive her of all consciousness, while they lasted. During the month of October she believed that she felt the movements of a child distinctly. Her increase in size was regularly progressive, but so quick that she was represented to have been as large by the end

of January as the generality of women are at the full period of gestation. After the month of December had passed, she felt the movements of the child continually, and it seemed to become daily stronger: the enlargement continued to be greatest on the left of the linea alba. At the end of April she was seized with periodical uterine pains, attended with much expulsive effort. She then considered herself in labour; especially since (according to her own calculation) the ninth month of her pregnancy had expired. The pains continued for nearly three weeks, accompanied with a discharge from the vagina of clear fluid, sometimes mixed with blood, and at other times assuming a yellowish appearance, amounting altogether, she believes, to about two quarts; and after the passage of two or three stringy substances, they gradually subsided. She suffered at intervals, during this period, great agony from the restlessness of the child; after which it ceased to move, and she has not felt it since. Still, however, her abdomen continued to enlarge, and her legs became œdematous. From the latter part of May she gradually wasted; her breasts and lower extremities shrunk to less than half their former dimensions; her appetite failed; and general debility ensued. The size of the abdomen, however, remained nearly stationary.

The preceding statement constitutes the history and symptoms of the case until the 14th June, 1835, when Mrs. J. first called at my house. The symptoms then present were as follows:—She was much emaciated, but her general aspect was not unhealthy. Pulse 100; tongue clean; bowels indolent, unless their action was excited by purgatives. Her sleep was sound, and appetite good, but she felt afraid of taking solid food in consequence of its producing pain in the stomach, accompanied with flatulence. She complained of pain in the lower part of the back when in the erect position, and a dreadful sense of suffocation when recumbent.

On an examination per vaginam, that canal was found to be shorter than natural, and its sides were compressed by the protrusion of a large tumor, occupying a portion of the pelvis between it and the rectum. The os uteri was indistinctly felt, conveying to the touch an impression that the fundus was turned downwards and backwards, and the mouth forced up behind the pubes.

I did not see the patient again till July 8th. I found her much altered for the worse, and considerably more reduced than when the former observations were made. The pulse was now 115; the respiration laborious; tongue dry and furred; there was a disagreeable taste in the mouth, and the bowels were constipated; there was also frequent vomiting of a white frothy fluid; pain in the back; constant desire to pass urine, which was voided in small quantities, and its evacuation afforded only temporary relief, from the painful sense of distention. She suffered frequent forcing and bearing down pains, with cramps in the legs; and her nights were disturbed and sleepless.

Dr. Ramsbotham visited her with me on July 9th, the symptoms being then as just described. After examining the tumor externally, and by the vagina, he was of opinion that the case was one of ovarian disease, and that paracentesis abdominis ought to be adopted.

Dr. F. H. Ramsbotham saw her, in company with his father, on the 12th; and from the distinct sense of fluctuation communicated to the hand on examining the abdomen, considered that there was ascites complicated with ovarian enlargement. He concurred in the propriety of letting out the fluid, and thought that as little time as possible should be lost before the operation was performed.

On the 15th, in the presence of Dr. F. H. Ramsbotham and Mr. Margetson, I introduced a large-sized trochar through the abdominal parietes, in the linea alba, about two inches above the umbilicus, where the tumor was most prominent. About six pints of a thick, sizy, chocolate-coloured fluid escaped, possessing an unpleasant, though not highly-fœtid, odour. This proceeding afforded considerable and almost instantaneous relief to the suffocating sensations the patient had previously experienced. On the discharge of the fluid Dr. F. H. Ramsbotham expressed surprise at its character, having been previously impressed with the conviction that it was contained in the peritoneal cavity. The aperture did not heal, and a fluid continued to be discharged daily, which at first resembled that let out by the trochar, but became by de-

grees more offensively fœtid, until a fortnight after the operation, when a small lock of fœtal hair was observed in it. From this date pieces of the same kind of hair passed repeatedly, as well as globules of oil and portions of putrid skin, with some membrane, which was described by the patient as resembling fœtal intestines, and which could be drawn out by the fingers to a considerable length. A hard circumscribed substance could now be distinctly felt through the abdominal parietes, in the left hypochondrium, evidently the head of a child, and no doubt remained in the mind of either Dr. F. H. Ramsbotham or myself as to the true nature of the case. She remained much in the same state throughout August; but before the middle of September all the symptoms became aggravated. She suffered much from constitutional irritation; the pulse rose to 120; there was great thirst, and her appetite was impaired, which she attributed to the offensive effluvia that arose from the putrid discharge.

On September the 21st Dr. F. H. Ramsbotham again met me, and I proposed making an incision sufficiently large to remove the child. To this measure he objected, thinking putrefaction had gone on to such an extent that the body could not be extracted entire, and that the frequent introduction of the hand would be required for the purpose of emptying the cyst in which it lay; but he thought it would be desirable to enlarge the aperture to the extent of an inch or two, that the fluid might have a more ready exit, and that an opportunity might be afforded us of examining more accurately the degree of decomposition that had taken place.

Accordingly, on the next day, in the presence of that gentleman and Mr. Margetson, by means of a director and bistoury, I made an incision about two inches in length downwards, below, and rather to the right of the umbilicus. A small artery was divided, the hæmorrhage from which, however, was easily checked by gentle pressure; a considerable quantity of fœtid matter escaped. The finger could be introduced to its whole extent into the cyst, upwards, downwards, and laterally; and it was ascertained that the fœtal body still retained much greater firmness and solidity than had been anticipated. We were yet not perfectly agreed as to the propriety of extending the opening, and it was determined to seek the advice of a consulting surgeon. On the day after, therefore, (Sept. 23d), we obtained the opinion of Mr. Mayo; and as he concurred with me, Dr. F. Ramsbotham withdrew his objection, and it was agreed that the body of the child should be removed without delay. Besides the gentlemen just named, Mr. Margetson was present, and his opinion was also favourable for the operation.

The grounds upon which we resolved to remove the dead fœtus were the following:—

1st. The body had so much consistence and firmness, that we conjectured it would probably be *several months* before it would be sufficiently softened to come away piecemeal in the discharge; but the state of the mother's health was such, that we could not expect her to hold out *many weeks* longer unrelieved.

2dly. The original risk arising from opening the cavity of a peritoneal ovum was already got over, as far as the irritation of puncturing and admitting air into such a cavity could be prejudicial; the effect had been tried, and had not injured this patient. A freer opening might get rid of the existing source of irritation in the abdomen, but did not seem likely to add another.

3dly. It was evident, from examination by the finger, that the cyst enclosing the fœtus lay before the bowels, and behind the abdominal parietes; and that the existing wound might be enlarged upwards and downwards in the direction of the linea alba, without endangering the opening of the free peritoneal cavity, or dividing any part of the reflected peritoneum, which was not adherent to the outer membranes of the ovum.

The operation was performed in the following manner:—

I enlarged the aperture, both above and below, to the extent of about five inches. Mr. Mayo then introduced his hand, and grasped the left upper extremity, which he brought out of the wound; but the transverse position of the fœtus prevented its being extracted by that member; the arm was therefore separated at the shoulder. The part that was next taken hold of was a foot; this was then drawn out, and the trunk afterwards extracted without difficulty; the head, however, was too bulky to come away entire, and on the suggestion of Dr. F. H. Ramsbotham.

the cranium was punctured with a scalpel in the lambdoidal suture; a quantity of offensive gas instantly escaped; the bones collapsed and passed readily.

The removal of the child was followed by a flow of the same kind of offensive, brown, putrid fluid, which we had before remarked. The funis was divided; a portion of it, with some membrane, was left hanging out of the wound. Upon gently pulling the maternal end of the funis, the placenta was felt to be still adherent: no attempt was therefore made to remove it. The wound was dressed with a strip or two of adhesive plaister, and poulticed, an opening being left at the lower part to allow the escape of the fluid still within the cyst.

The operation did not occupy much more than five minutes, and was borne by the patient with the greatest fortitude. No vessel was divided that required to be secured; but a slight faintness came on soon after its completion. The fœtus was as large as an ordinary one at full time, and the cuticle was perfect except on the scalp, whence it had entirely separated, and over the vertebræ and the joints of the fingers and toes, which were denuded.

Sept. 24th.—Met Mr. Mayo and Dr. F. H. Ramsbotham, the latter of whom has continued to attend with me three or four times a week from this period. She has passed a good night; expresses herself much relieved; the countenance is cheerful; pulse 115, but evidently excited by seeing us. There has been no rigor or sickness; no pain or tension on any part of the abdomen. The bladder has acted two or three times; one motion of a natural appearance has passed: a larger portion of membranes than was apparent yesterday is hanging out at the lower part of the wound: she complains of being hungry: the dressings were not disturbed. It was not thought necessary to administer any medicine; but she was allowed a little broth.

25th.—Another comfortable night; we found her eating some flounders. Pulse 100, no pain, and the bowels and bladder had acted. The fluid discharge since yesterday has been trifling; but a considerable part of the placenta is offering itself at the aperture. The whole of this organ was drawn away most easily by a pair of forceps; it was perfectly putrid: the cellular web having been entirely destroyed, and the vessels separated from each other, hung down like strings. It had the appearance, indeed, of having been a long time macerated in water. It was smaller than a uterine placenta. About a pint of fœtid fluid followed its extraction. Dressed as before.

From this time she continued improving, the discharge varying daily from about four to eight ounces, but still very fœtid in character. Not a single bad symptom appeared until Oct. 2, when she complained of slight gastric uneasiness, which she attributed to having eaten some turnips. To relieve this, a mild dose of the compound decoction of aloes was administered; it produced, however, so much relaxation, that it was thought necessary to order an astringent. The purging was soon checked, and she became again free from uneasiness. The wound is now very much contracted, and the edges granulating: she has sat up two or three times. She continued again to improve till the 8th, when she was attacked with constant pain on the right side of the abdomen, extending from the ribs to the spine of the ilium, and the least pressure caused an aggravation; there was also sympathetic fever. The discharge from the wound has become changed, both in colour and smell: it is yellowish, as though bile was mixed with it, and possesses a slight fœcal odour. Pressure on the right side of the abdomen produces an increase of discharge, and an evacuation of gas: the bowels continue to act naturally; the size of the aperture is much diminished. Fomentations relieved the pain, and saline medicines seemed to allay the fever. From this date, the 8th October, the general symptoms have remained much the same, although upon the whole the aspect of the countenance has improved; the discharge has been occasionally feculent. Upon our visit yesterday we found her in good spirits: pulse 98, rather small. She sleeps well; her appetite much improved. The bowels have acted daily once or twice. At the bottom, and on the edges of the wound, healthy granulations have sprung up, which have contracted the opening to the size of about half an inch. The discharge consists almost entirely of pus from the granulating surface, and that in moderate quantity. The flaccidity of the abdominal parietes, consequent

on the extraction of the child, has, Oct. 26, entirely disappeared; she has been able to sit up, dressed, for two or three hours every day, since Thursday last, without fatigue; can walk without pain, and she seems in a fair way to be eventually restored to health.

SOME OBSERVATIONS ON ANIMAL TEMPERATURE AND VITALITY,

Suggested by Dr. Williams's Letter in the last number of the Medical Gazette.

By A. P. W. Philip, M.D.

To the Editor of the Medical Gazette.

Sir,

I am sorry to see that Dr. Williams declines the proposal I made, because, till the functions of the sensorial and nervous powers are clearly distinguished, and the nature of the latter ascertained, all discussions respecting them must more or less be, as they have always hitherto been, involved in error and consequent confusion; a striking instance of which I am now to consider.

He will find, I think, on a more careful review of the subject, that his opinion of the cause of animal temperature is much too confined; and that there can be no greater error than that the spinal marrow does not contribute to its maintenance, the contrary of which is proved by direct facts. The confusion which prevails on the subject cannot, I think, be better illustrated than by the circumstance of a person so well acquainted with it as Dr. Williams supposing that animal temperature can in any degree be maintained by the functions of circulation and respiration after the influence of the nervous system is wholly withdrawn*.

It has been found by experiment, that in proportion as the function of the spinal marrow is impaired by destroying part of this organ, the temperature sinks, the sinking of the temperature being proportioned to the extent of the part destroyed (my Inquiry into the Laws of the Vital Functions, part ii. chap. vii. sect. 2, 3d edition.) The maintenance of animal temperature depending on functions which take place in every part of the system, and these, being, like the other assimilating processes, functions of the central parts of the nervous system, can only be generally influenced by causes which influence the functions of these parts, not by causes which prevent their influence from reaching particular parts of the body. Thus the stomach may have its nervous influence so impaired by so dividing and separating the divided ends of the eighth pair of nerves, as to destroy its functions, with little if any sensible decrease in the general temperature of the animal, because the whole power of the brain and spinal marrow remains, although the full influence of the former is prevented reaching that organ. But when the same effect on it was produced by destroying the lower half of the spinal marrow, the actual power of the brain and spinal marrow being thus reduced, there was, along with the loss of power in the stomach, such a diminution of the temperature, that in a few hours after the operation the animal shivered violently, which could only be prevented by keeping it in a very high temperature, which, indeed, seemed necessary to prevent its dying of cold, because here the general source of nervous influence was impaired.

The temperature was for a similar reason much impaired in Mr. Brodie's experiments, by which the general powers of the nervous system were enfeebled—that is, of the brain and spinal marrow—the only active parts of that system *. From mistaken views of the functions of the spinal marrow, succeeding writers have, in speaking of the effects observed in Mr. Brodie's experiments, not taken into account the effects they necessarily produced on this organ, as well as on the brain.

The functions of the spinal marrow are as essential to the maintenance of animal temperature, and indeed of all the assimilating functions, as those of the brain itself. Nor in these, as in the sensitive functions, does the brain act through the spinal marrow, in consequence of which, in the more perfect animals, the sensitive functions of the spinal marrow cease on the removal of the brain. The vital functions of the former organ, on the contrary, remain, and are not immediately impaired by

* Med. Gaz. October 10th, p. 50.

* See a paper in the Philosophical Transactions for 1833, republished in my treatise on Sleep and Death.

the removal of the brain. Hence it is that the temperature is, to a certain degree, maintained by artificial respiration after its removal.

When the nervous influence is wholly lost, the mechanical parts alone of circulation remain. It is impossible, under such circumstances, that any degree of animal temperature can be maintained, the chemical part of the vital functions wholly depending on that influence.

In the last sentence of Dr. Williams's last communication, an opinion is expressed, tending (as far as I am capable of judging) to divert the attention from the only principle on which correct physiological views can rest.

"Whilst," he observes, "we admit the possibility of electricity having properties yet unknewn, which may hereafter explain the various processes of organic life, we must for the present content ourselves with referring them to a cause, of which we confess our ignorance by calling it vitality."

What is meant by saying that we must confess our ignorance of vitality? If the nature of vitality be meant, the expression is correct. But is not the same true of the nature of all other principles of action? Take any of the most familiar instances. Do we know more of the nature of gravitation than of vitality? What do we mean by a knowledge of any such principle but a knowledge of its properties?

The apparent greater mystery of vitality arises, not from its being more mysterious than any other principle of action, the nature of all is equally so, but from the properties of vitality being more varied, less easily made the subject of inquiry, and bearing less analogy to the phenomena most frequently presented to us.

Both the chemical and mechanical powers of living animals are under the influence of their vitality, on the same principle as the electricity of the magnet is under the influence of magnetism; and we are no more in the one instance than in the other precluded from ascertaining by observation and experiment in what respects and to what extent the modification takes place.

Let us adopt a more correct language. Let us cease to talk of the obscurity of the vital principle,—it is no more obscure than all other principles,—but speak of the difficulties of ascertaining its properties; for the properties alone are all that we know of any principle of action,—difficulties which here depend on the variety of the phenomena, the complicated and varying nature of the subjects of our observation and experiments, and the little analogy which exists between the properties of vitality and those of other principles of action, which renders the former less familiar subjects of contemplation; for the striking analogy which exists among the phenomena of all the principles which operate in inanimate nature is greatly modified in many of the phenomena of the living animal, and wholly lost in those in which no inanimate agent interferes.

We thus speak a language which can at once be understood. To talk of the obscurity of the vital principle, implies that we know more of the nature of any other principle of action.

It is in no instance the nature, but the properties alone of any principle that can be ascertained by observation and experiment. They alone are the objects of our senses. A knowledge of the former is equally in all instances not merely beyond the limits, but the nature of our minds. The blind may as well attempt a knowledge of colours. And it will surely be admitted that the properties of the vital principle may be made the subject of observation and experiment as well, though not so easily, as those of any other principle of action. The greater difficulty, which arises from no other cause than the properties of vitality being both a more complicated and less familiar subject, lessens as we proceed, provided we steadily reject the influences of prepossession and imagination, so ready to intrude where the facts are at all complicated or obscure.

Nothing shows in a more striking manner the inaccuracy of our reasonings respecting the phenomena of life, than that writers who deservedly stand high, even Hunter, and others of hardly less name, have confounded together the vital principle and nervous influence, a mistake not less, and, as far as the nervous influence is modified by the vital principle, precisely of the same kind as if they had confounded the vital principle with muscular contractility, one of the properties bestowed by that principle with the principle itself.

In the living nervous and muscular systems, and the living blood, reside the

properties of the vital principle. The properties peculiar to them, therefore, we must study, if we wish to become acquainted with that principle, in the only way in which we can become acquainted with any principle of action; and is this study, although certainly beset with greater difficulties, at all more hopeless than the study of the properties of electricity or gravitation?

Let us, then, cease to talk of the hopelessness of a knowledge of vitality, a position originally the mere result of that indolence of mind which shrinks from difficulties, and maintained by the power of habit, and the circumstance of not giving ourselves time duly to consider the question.

We have precisely the same means of acquiring a knowledge of vitality as of all other principles of action.

Its properties are as open to observation and experiment as those of any other principle, and we know nothing of any principle but what is acquired by these means.

I abstain from farther observing on the subjects on which Dr. Williams touches, convinced that, till we are agreed respecting the question stated in my last communication, any more particular discussion of them would be little better than loss of time.—I am, sir,

Your obedient servant,

A. P. W. PHILIP.

Cavendish Square,
Nov. 2, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

On Bloodletting. An Account of the Curative Effects of the Abstraction of Blood; with Rules for employing both Local and General Blood-letting in the Treatment of Diseases. By JAMES WARDROP, M.D., Surgeon to the late King, &c. &c.

JAMES WARDROP, *M.D.*! Mr. Wardrop, who has all his life affected to despise physicians, now striving to add something to his respectability by engrafting his new claim to the doctorate on his old standing title of "Surgeon *to the late* King,"—a piece of Heidelberg lace upon a thread-bare court dress! Doubtless the worthy *Doctor* has seen the error of his ways, and now only desires that the public should learn to regard him as occupying a place among the *other* metropolitan physicians of the day.

The volume before us consists of seven short lectures, now called "discourses," which the author informs us have already been published *elsewhere*; but with a prudential reserve worthy of all praise, omitting to state the work in which they appeared, and avoiding the name of the *Lancet* by a process as circuitous as the circumlocution of the traveller who informed his readers that on a particular day he crossed a river, "the name of which (he adds), for brevity's sake, I shall here omit to mention." As to the *Doctor*, having passed the Rubicon when he became one of the contributors to the Lancet, he should have made up his mind to brazen it out boldly; for he may rest assured that *where* his "discourses" appeared can no more be consigned to oblivion by such omission, than the author of certain "intercepted letters" can be concealed by their being published without a signature.

The circumstance of the discourses having originally consisted of lectures gives to them an elementary character, and has led to the statement of many truisms, or, at least, of facts so familiar as to render it matter of some surprise that their formal enunciation should have been deemed necessary. Thus we are informed that "the blood which is contained in the heart, in the arteries, and in the veins, is kept in a continual motion, called its circulation;" that "when serum collects in a preternatural quantity, either in the cellular texture, or in any of the serous cavities, as those of the pleura, pericardium, or peritoneum, it produces a disease called *dropsy*," &c. &c. There are numberless other paragraphs of the same nature, so very true, and so very well known, that the author might safely have omitted them. Again, we meet with some passages between the clauses of which there is no connexion whatever; or none that we have ingenuity enough to discover. Thus, we are told "Dr. Tweedie once saw the buffy coat in blood taken from the temporal artery of a person labouring under pneumonia; and the great prevalence of the buffy coat in diabetes, first lead [led] Dr. Watt, of Glasgow, to treat that disease by frequent bleed-

ings." Now we are utterly at a loss to discover what connexion exists between Dr. Tweedie having seen the buffy coat form upon arterial blood once — or one hundred times — and Dr. Watt adopting the plan of bleeding in diabetes. It is a complete *non sequitur*. Dr. Watt was led to bleed in diabetes, not because Dr. Tweedie, or any other person, *once* saw the buffy coat in *arterial* blood in pneumonia, but because he himself very often saw it in *venous* blood taken from diabetic patients. Neither is the phenomenon of buff occasionally forming on arterial blood confined to the testimony of Dr. Tweedie, having been witnessed by the late Dr. Gordon, of Edinburgh, by M. Gendrin, and others.

The same ignorance of well-known facts is shewn in other points. Thus, in the very next sentence, the authority of Dr. Hamilton, of Edinburgh, is quoted to shew that the buffy coat is sometimes seen on the blood of persons who are under the influence of mercury. Could Mr. Wardrop not have stated this on his own authority?—or is he ignorant that in scurvy and various other cachectic conditions, the same phenomenon is occasionally witnessed?

But what, after all, does this book pretend to teach? What in the shape of original information is there given in it? We can hardly believe that it requires the revelation of a newly-dubbed *Dr.* to tell us that "he who is in the habit of prescribing the abstraction of particular quantities of blood, in like manner as he may be accustomed to write the prescription for a dose of medicine, must possess a very incompetent knowledge of all the advantages which are to be derived from the judicious employment of this most useful, and no less powerful, remedy."—Page 148. Nor, surely, does it need Dr. Wardrop's authority to inform us that "the state of fainting is an unerring criterion for estimating the extent to which blood should be removed in those cases where general bleeding is most expedient—such as inflammatory diseases attended with febrile disturbance, and in congestions affecting the vital organs."—P. 72.

Does Mr. Wardrop really flatter himself that the world is now, for the first time, informed of these matters; and that to him we are indebted for these novel hints regarding this "useful and no less powerful remedy?" If he does, we are amazed at his simplicity. We believe the credit of having first put forward, in a strong light, the practical utility of attending to these points, is eminently due to Dr. Marshall Hall; whose work on Blood-letting* we are surprised that Mr. Wardrop does not once mention from the beginning to the end of his little volume. We may take this opportunity of recommending Dr. Hall's valuable work to our readers; they will find in it also several other rules and observations of great importance, relative to blood-letting as a diagnostic of diseases.

In fact, the practical portion of the publication before us may be stated in a single sentence: — bleed, when your patient has an incompressible pulse, whatever be the name of the disease; and bleed till you subdue the pulse, whatever quantity may be necessary for this purpose. This, we think, is a fair representation of Dr. Wardrop's doctrine; and it is in the main a good one, though the manner in which it is illustrated and enforced shews great ignorance of what has often been said on the same subject by others. Thus, several pages are occupied in arguing that bleeding may be employed in the eruptive fevers, where the same symptoms are present as would indicate it in any other disease; as, for instance, it is said that in scarlatina, when the inflammation of the throat is "severe," relief is afforded by local bleeding; "but when any of the vital organs are affected, then venesection is preferable." Is there any man who has been a year in practice—any tyro who has passed his examination at Blackfriars—who imagines that in inflammation of a vital organ he is to refrain from venesection if his patient happen to have scarlatina or measles? We maintain that there is no novelty whatever in the general principle which Mr. —we beg his pardon—*Dr.* Wardrop labours to establish, just as if it were a great doctrine in physic peculiar to himself. It is true that by repeating the same thing many times, and by instancing many different diseases (though in a very superficial manner) he endeavours to convey the idea of his having had to

* Researches principally relative to the Morbid and Curative Effects of Loss of Blood; by Marshall Hall, M.D. F.R.S. London, 1830.

overcome numerous erroneous opinions which he assumes to exist. Still, when we strip what he says of its disguise, it always comes to this—that where there is violent general reaction, blood may safely and advantageously be abstracted till an impression be made upon the pulse; in which recommendation we humbly submit that, however true it may be, it possesses no novelty whatever.

In some few instances, instead of adhering to the safe general rule above mentioned, our author ventures upon an exclusive and unqualified injunction; thus we are informed, that "*whenever* the abstraction of blood is necessary in the treatment of erysipelas, general is to be preferred to local blood-letting." A more unfortunate selection for such an exclusive recommendation could not well have been made; and this is the more remarkable as the disease is surgical, and therefore one with which Mr. Wardrop might be expected to be familiar. We say, with all deference to our author, that there is no inflammation of equal severity in which local is so frequently preferable to general bleeding; particularly in the form of scarifications. Neither is it correct, as implied, that, in our public hospitals, erysipelas is treated by "as much bark as the stomach can receive." Mr. Wardrop here, as elsewhere, either is ignorant or affects ignorance of the actual state of medical practice. If he will take the trouble to visit the public hospitals (which, from his writing, we suspect he never does) he will find, that though some of the older practitioners still retain the idea of specific influence being possessed by cinchona over erysipelas, yet that the great majority treat the disease on general antiphlogistic principles; but he will also find that there is no form of inflammation which is so frequently attended by depression, and by so much tendency to sinking, or which so often requires the use of stimulants and tonics; particularly bark.

There is one position of the author's which we are quite certain is replete with *danger*. Mr. Wardrop observes, page 125,—"In the treatment of all *inflammations*" "the blood should be abstracted from the arm whilst the patient is in the recumbent position, until syncope be produced." However safe it may be to bleed to incipient syncope in the *erect* position, we are quite satisfied, from much experience, that blood-letting carried to syncope in the recumbent position is a measure at once unsafe and unnecessary. Nay, we would venture to assert, that it is quite as wrong to bleed in the *recumbent* position, in *any* case, as to *prescribe* the quantity of blood which may be withdrawn.

That we have been disappointed in the volume before us, must ere now have become evident to our readers; and we confess it does surprise us, that any thing which Mr. Wardrop has been "employed in collecting and revising many years," should be so superficial, or should have been given to the public in so crude a form. The only praiseworthy part of the volume is the "getting up" by the publisher—which is exceedingly good.

Medico-Chirurgical Transactions. Published by the Royal Medical and Chirurgical Society of London. Vol. XIX.

The principal feature of novelty in the present volume is that it appears as containing the Transactions of the *Royal* Medical and Chirurgical Society. A copy of the charter serves as a preface, and the papers which follow are numerous and important; but as we have already, at the time of their being read, given full and accurate reports of them, we do not find any thing to add or to correct. Dr. Robert Lee continues one of the most indefatigable contributors, having furnished not less than three papers. There is also an exceedingly elaborate essay on various affections of the brain, by Dr. Sims.

Although we thus express a very favourable opinion of the volume, we would nevertheless strongly urge the necessity of the cases being condensed in future volumes—many of those before us being detailed with the most tedious and unnecessary minuteness. The Council ought to look to this. That numerous cases have been read in detail, is no sufficient reason for their being published, except with the most rigid curtailment that is consistent with perspicuity; and even then we think that mere cases to illustrate the opinions adduced in a paper ought to be printed in a smaller type, without separation into paragraphs, with wide interstices—as in many of those before us.

MEDICAL GAZETTE.

Saturday, November 7, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MEDICAL ATTENDANCE ON THE COUNTRY POOR.

In our last number we had occasion to point out some instances in which the medical police (if it may be so called) of this country is lamentably mismanaged. Had we room at the time to insert Mr. Rumsey's circular (see page 190), we should have felt called upon to add some further remarks on the same subject. Since then our attention has been called to an able and well-written letter in the *Times*, which so clearly sets forth the inefficiency and paltry meanness of the system at present pursued by the Boards of Guardians throughout the country, under the auspices of the new Poor Law Commission, that we cannot refrain from bringing certain passages of it more immediately to the notice of our readers.

We pass over (inasmuch as we have ourselves treated the subject largely before) the writer's observations on the preposterously unequal duties imposed on the practitioners employed under the new system. Every body has heard of the superabundant pauper populations committed to the charge of the new comers; it is, however, of some importance that the facts should be vouched for by an authority so candid as Ruricola evidently is. Pass we on, therefore, to his remarks on the method of portioning out the sick poor into districts:—

"Again, the unions are divided into districts, and no doubt wisely so, for *relieving* officers, or for general purposes: but though these districts bear not the remotest regard to the practicability of medical attendance, yet are they peremptorily forced upon parochial surgeons, and their pauper patients, to the great injury of both.

To insist upon identity between districts for general relief, and districts for medical relief, is manifestly absurd. The fanciful divisions made by these new authorities cannot, in a rural population, suddenly alter conventional arrangements, the growth at least of a generation, and often of a century. Each village and parish in the country has long ago decided in what directions it is most suitable to send for medical advice; and the comfort and advantage of all parties depend upon the medical districts of the unions being founded upon these results of experience: any arbitrary attempt to force different distributions of this relief upon the country can only end in disappointment and additional expense, because the total amount of the labour of medical men within the district must be increased by it, and so ultimately must the cost of that labour be enhanced."

The consequence of this is, that the medical officer, be he of what *physique* he may, being utterly unable to fulfil his engagements single-handed, is obliged to procure assistants; and to these is eventually intrusted the irresponsible care of the unfortunate sick poor.

Let us take another example of the working of the system—by which we shall be enabled still further to judge of its fruits.

"The next point to which I beg to invite attention is the mode of giving orders for medical relief. This is in almost every case left to the relieving officer of the district; for although the 54th section of the Poor Law Amendment Act permits the magistrate to direct medical relief in urgent cases, yet this permission is, in fact, merely nominal. I have not yet heard of an instance of its being made use of. The relieving officers, having the power, are generally allowed to exercise it exclusively. Now, if these functionaries would confine their judgment in cases of sickness to the question of how far the patient's circumstances entitle him to parochial assistance, it might do well enough; but they usually think themselves competent also to decide *how far the pa-*

tient's illness may require medical attendance. I need not point out the hardship that such a discretionary power must inflict on the poor; indeed, I could bring forward numerous instances of the sick paupers having lingered for the want of medical aid—because, forsooth, the relieving officers thought their cases too trifling for attention * * * * Another hardship inflicted on the poor is the distance to which they are obliged frequently to send, in agricultural districts, both for orders from the relieving officer, and for attendance from the surgeon. In many of the new unions the medical officer resides more than ten miles from the patient. Now, those who are acquainted with the habits of the poor know well the difficulty they experience in merely sending for a medical attendant. Even if they can procure a messenger, a day's work must be lost by a journey to such a distance; and there is, of course, great uncertainty in meeting with a surgeon who is obliged to be almost constantly travelling about an extensive district. It is, therefore, not surprising that the length of time before assistance, either by a visit or by medicines, can be afforded, is often attended with most serious consequences."

It must be recollected, too, that the relieving officer is a man of many avocations,—that he has generally five or six parishes to attend to, and the distance of his residence may be as great as that of the medical officer's—perhaps, also, in an opposite direction. Can greater cruelty be devised than that which must in many instances ensue in consequence of these arrangements?

The arguments of Ruricola in favour of the local practitioners, and proving the inefficiency as well as the vexatiousness of introducing a new set,—a race of adventurers among them,—are strong, and well worthy of consideration; and we are particularly pleased with the light in which he places the "tender" system. But our limits will not allow us to make many more extracts; nor, perhaps, is it very necessary that we should, especially on this subject, as we have already on other occasions so fully expressed our own sentiments upon it.

Another grievance touched on by this well-informed writer is the mode of payment adopted by the so-called Guardians of the poor,—paying the practitioner a miserable pittance per case. The usual rate is 2*s.* 6*d.*—*half-a-crown* for attending a patient *through an illness* long or short, providing medicines, and travelling perhaps twenty miles on each visit! There is a slight increase in the fee, it seems, when the number of cases is inconsiderable during the year; but in no instance, says the writer, does it exceed *four shillings*.

"For this trifling sum the medical officer is to attend the most important illness. Fracture of the skull—or typhus fever, requiring visits daily, or twice and thrice a day, at the remotest point of one of these immense districts, with a due supply of apparatus, leeches, medicines, &c. is to be attended for any length of time, short of 12 months, for 2s. 6d. Perhaps such a preposterous payment will be defended in this way—'the slight cases are much more numerous, and the average will produce a very fair salary.' Supposing, for the sake of argument, that if all the slight cases were sent to the medical officer, a fair salary would result (which, however, I am prepared to disprove), what will be thought of it when the relieving officer is instructed not to give orders for relief in trifling cases, and is allowed to decide which are and which are not trifling,—a decision that might perplex the profoundest physician? The injustice is yet further increased, when in addition to this the time during which a sick pauper may linger for want of an order may convert any trifling ailment into a serious and fatal disorder. And although in a dense population containing many pauper patients within a limited extent a small sum like 3s. or 4s. on all cases, without reservation, might produce something like a fair salary, yet it is manifest that this must be absurdly insufficient for a small number of patients residing at a great distance from the medical officer, or dispersed over a great extent of country. Nevertheless,

on no account will the commissioners allow of any increase of charge according to the increased distance of the patients, whether the salary be by a payment per case, or by a fixed sum. Here, however, is not the end of these oppressive terms, for lest by any possibility (the occurrence of an epidemic, &c. for instance) an unusual number of paupers should fall ill, the cases for which this payment is to be made are limited to a certain number, all beyond which the medical man must attend gratuitously!"

Yet there are those in the profession who accept these terms, and thus countenance the commissioners in their insulting conduct! We cannot otherwise denominate that conduct—that mode of dealing with medical men, which has so much that is degrading and disgraceful connected with it. Threats have been unscrupulously thrown out to induce the resident practitioners to accede to the terms proposed; their motives have been aspersed, and attempts have been made to bias the public opinion against them. With all this, we say, persons have been found mean enough to submit to the degradation offered to them and their order! Is there no unanimity, no consistency, no *esprit de corps*, remaining among even the humblest members of the profession? Has the scanty allowance tendered by the *patronizing* hand of heartless overseers such temptation in it, as to subdue the manly and generous feeling of those professing to belong to a liberal vocation? But we cannot persuade ourselves to acquiesce in such an opinion. We are rather disposed to believe that the parties to whom we allude are tempted to pursue their present course, trusting to the impunity of obscurity. They hope to escape exposure (they must have had a stock of bad principles to begin with), and they are reckless of all that can operate on a well-conditioned mind.

They will fail, however, in this object, we hope and we trust: the truth must come forth. We have great expectations of the result of the circular issued by the Association, of which Mr. Rumsey, of Chesham, is the secretary; and we are convinced that, ere long, we shall be possessed of a mass of facts which must have great influence with the public mind. As it is, we can scarcely believe it possible that the government is duly apprized of the real state of things. The public at large cannot be aware of it; otherwise the illiberal and narrow-minded functionaries, the ungenerous hirelings, who bring disgrace upon the institutions of the country by the scandalous way in which they exercise the discretionary power committed to them, would sink beneath the weight of general indignation. It is provoking to witness the operation of a system like this, which adds insult to inefficiency, while it is profusely payed for out of the public purse.

EXTRAORDINARY SCENE AT ST. BARTHOLOMEW'S.

An account will be found at page 189, of a *scene* which took place at St. Bartholomew's last Saturday, and which we can scarcely suffer to pass without comment. There has hitherto been an understanding among the officers of the metropolitan hospitals, that the pupils at each of these establishments should be admitted to the others when any important operation, or similar opportunity of acquiring professional information, presented itself; but this accommodation has always been afforded as an act of courtesy and favour, and of course under such restriction as to prevent it from interfering with the prior claims of the regular pupils of the hospital. In this way many gentlemen entering to one of the London hospitals

have had opportunities of witnessing operations by nearly all the eminent metropolitan surgeons of their time; and there can be no doubt but that great advantage has thence accrued to the profession. To expect, however, that the pupils of any school should be be allowed to pre-occupy the benches of the operating theatre in another, whilst the proper pupils of the hospital are absent at lecture, is evidently quite unreasonable. It is very natural that gentlemen should be desirous to see as much practice, and to obtain as much information as they can—and it is doubly natural that those who at their own hospital want the requisite opportunities, should endeavour to make up for the deficiency by seeking them elsewhere—but a moment's reflection must convince them that this can only be done fairly when it does not interfere with the rights and privileges of others.

We have no doubt that the subject requires only to be noticed in order to prevent the repetition of such a scene, and to spare the necessity of enforcing that rigid system of exclusion to which it must otherwise unavoidably lead.

DEATH OF DR. JAMES HAMILTON, OF EDINBURGH.

THIS distinguished veteran died at his house in Edinburgh on the 27th ult., in the 87th year of his age. Dr. Hamilton formed a connecting link between the past and present race of physicians; and with him the last remains of the old school have completely passed away. Many of our readers, doubtless, remember him: who that has ever visited the Royal Infirmary of Edinburgh can ever forget him? Summer and winter, fair day and foul, was Dr. Hamilton to be seen stepping along, with his thin-soled shoes ornamented with large buckles, his black silk stockings, and those short inexpressible articles which cover the "nether bulk;" his formal, square-cut coat, and his redoubtable cocked hat—the whole in exquisite keeping with his upright elastic gait, and his expression of mingled shrewdness and eccentricity. He was the *beau ideal* of a physician of the last century, and with him the age of cocked hats, shoe-buckles, shorts and all, is clean gone by.

But Dr. Hamilton, though these be the points which attached him to the fancy, had stronger claims to our respectful notice; for to him we owe one of the most important improvements in the practice of modern times—a practice now universally adopted in this country; we mean that of administering purgative medicines in fever and many other diseases, in which, anterior to his time, the existence of debility was thought to prohibit their use. It is about thirty-five years since his work first appeared, and for nearly twenty years before, Dr. Hamilton had been in the habit of inculcating and illustrating his doctrines in the Royal Infirmary, which at that time was attended by students from all parts of the world, and by whom his doctrines were rapidly carried into every quarter of the globe.

Benjamin Bell, at that time enjoying the first practice in Edinburgh, married a sister of Dr. Hamilton, to which connexion the latter was much indebted for his early introduction into practice. He was appointed physician to the Infirmary when a very young man, and practised in Edinburgh for above sixty years. He is supposed to have left a large fortune, which goes to his family by a marriage privately contracted, and which was declared but a few years ago.

OFFICE OF APOTHECARY TO ST. BARTHOLOMEW'S HOSPITAL.

(From a Correspondent.)

THE apothecary of St. Bartholomew's, some time ago, determined to resign:

a circumstance of which a favoured individual was informed, and who made such good use of the information that he canvassed the Governors, addressing to them a circular printed a considerable time before the vacancy was declared. The result was, that on the resignation being made known, all those who might otherwise have come forward were deterred by finding the field completely pre-occupied.

We have good reason to believe that we have stated the facts simply as they occurred; and we may add, that from none of the said Governors did the candidate (for a vacancy which did not then exist) receive any check or remonstrance, but, in fact, that all, or a large majority, promised him their votes. Now all this is very discreditable to the hospital, and calculated to be to it of very serious injury. We trust, for the honour of the profession, that the medical officers are not implicated in the transaction; but we do think that they are imperatively called upon to free themselves from a suspicion of having connived at what is in every sense of the word a job.

The medical officers are not governors, and cannot, therefore, have a direct part in the novel kind of election about to take place;—we mean that they cannot vote. But that is not enough; the profession will expect to learn that they were not concerned, directly nor indirectly, with the tradesmen who govern St. Bartholomew's Hospital, in the perpetration of a job, and in getting up the mockery of an election without a choice.

There are some other points connected with this subject which would require more space to discuss than we can at present give to them. Here is a large hospital, with an income of 40,000*l.* per annum, in the management of which, and its vast funds, any tradesman may purchase a share for 50*l.*; and he who does so may become a contractor for the supply of goods, so as to put in his pocket many times the amount he has paid; and we know that this is not a supposititious case, but that some of those who have thus become governors are actually the tradesmen who supply the hospital—the consequences of which are too obvious to require that we should point them out. We shall probably return to this subject.

LECTURES ON SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

BY DR. LATHAM.

ON SYMPTOMS.

General Notion of Symptoms—How they differ from mere Signs—The Relation of Symptoms to Diseases not the same in all cases—Symptoms are direct or indirect—Character of each.

Direct Symptoms respect the Sensations, Functions, and Structure of the part affected:—

1. *Symptoms which respect Sensation—Pain, its Degrees, its Qualities—Amount of Information derived from Pain as a Symptom—Sources of Deception arising from it.*

2. *Symptoms which respect Function—Amount of Information derived from them, as compared with that derived from Sensation—Amount from both taken together.*

3. *Symptoms which respect Structure—The information derived from them limited to parts within reach of the sight and the touch, until Auscultation brought the diseases of certain organs within the scrutiny of the ear.*

IN going round the hospital my mind often reverts to the time when I was a mere beginner like yourselves; and I remember how strange and puzzling to me was every thing that I saw; how I thought I never should be able to distinguish diseases, one from another, as long as I lived; and, as to treating them, I could not look forward with the hope that my conscience would ever allow me to attempt any such thing.

Above all, I was perplexed with the number and variety, and (as I humbly thought) contradictory nature of symptoms. It seemed to me, that if I could ever succeed in learning them all, it would be to no profit; for the same symptoms appeared sometimes to import one thing, and sometimes another.

There was a patient, perhaps, suffering convulsions; and the physician evidently thought the case most grave and perilous, for he employed several remedies of the most gigantic power, and succeeded in saving him. But there was another patient suffering convulsions no less severe, and to my apprehension just of the same kind, yet so far was the physician from thinking seriously of this case, or treating it severely, that he just looked at the pa-

tient and smiled, ordered some cold water to be thrown in the face whenever the convulsions returned, and said that would cure her; and, sure enough, he was right. Moreover, many died who seemed to me to have little or nothing the matter with them, and many recovered whom I did not hesitate to condemn to death at first sight.

Thus health and sickness, and life and death, seemed the most mysterious things in the world; and the symptoms which were said to indicate them were to me a long while unintelligible.

These recollections, at this day so often present to my mind, enable me to place myself in your situation. They serve the good purpose of making me feel, that just the same difficulties which are *yours* now were once my own, and of making me wish to aid you by my little experience in removing them.

Yet the very objects which have puzzled me, and are, perhaps, now puzzling you, do in fact contain infinite instruction. It is by symptoms, and by symptoms only, that we can learn the existence, and seat, and nature, of diseases in the living body, or can direct and methodize their treatment. But, first of all, symptoms themselves must be understood, before we can make the proper use of them for gaining the instruction which they are calculated to convey.

It is important for us to understand that the symptoms or signs of diseases are never to be taken in the like sense with that in which the signs of external things are often regarded. The buoy floating in the river, which marks its navigable tract; the bell which, by striking, denotes the lapse of time; the stone by the way-side, which tells us how far we have come, and how far we have yet to go; these are most important *signs*, and of indispensable service to us all, but they have no *natural* connexion with the things they are made to indicate. They are mere expedients, of conventional meaning and use. Navigable rivers, and time and space, would still exist, though there were neither buoy, nor bell, nor mile-stone, in the universe.

There is nothing that we call the symptom of a disease which does not contain within itself much more than a mere sign. Heat, pain, redness, swelling, are called the signs of inflammation; but nature does not intend by them barely to intimate that inflammation exists; they are essentially connected with the processes she is carrying on.

Thus at early dawn we point to the first glimmering in the east, and call it a *sign* of the rising sun; but it is more; it is an emanation from his beams. We look at the cloud above our heads, and say it is a *sign* of rain; but it is the gathering of the waters themselves.

Concerning symptoms I would nevertheless remark (what is very important to be borne in mind), that they stand in different relations to the diseases to which they belong. They may flow out of the disease, so as, in idea at least, to be separable from it; and they may be involved in the disease, so as to be identical with it. The difficult respiration, the cough, the sputa, the emaciation, the hectic, are the symptoms of phthisis, and are distinguishable from the disease itself. They are the signs of something beyond themselves which we do not see—viz. tubercles of the lungs.

But the symptoms which denote an intermittent fever are the same which constitute the disease. We have no idea of an intermittent apart from the rigor, the heat, and perspiration. The same may be said of other fevers, and almost all diseases not organic, in which, if you seek to separate the symptoms from the disease, you must resort to theory for the purpose, and conceive an action of a certain kind prior to the *actions* which constitute the symptoms, and productive of them.

There are symptoms, then, which, in the plain and intelligible sense, are signs and tokens of the disease which exists separately and distinctly from them; and there are symptoms which, however they may be spoken of as signs merely, are, nevertheless, all *that we know* of the disease itself. The disease is the symptoms; and the symptoms are the disease.

True: but it can hardly be conceived that they are in reality the same. Yet it is better that they should be so regarded, than that we should go beyond our knowledge in attempting to distinguish them. Better to see in fevers only a certain combination of symptoms, than to run wild about a debility of the nerves, a spasm of the extreme vessels, or a peccant matter in the blood.

Again, I would remark that it is often most difficult to draw the line between what is disease, and what is symptom; and that the same conditions may, in different cases, become now one and now the other. A dropsy, or an hæmorrhage, are sometimes primarily and essentially *the disease*. Sometimes they are secondary, and incidental to *the real disease*, and are themselves only symptoms.

How impossible, then, must it be to give any definition of a symptom which shall be *philosophically* true, and at the same time satisfy every sense in which it is practically regarded!

From the imperfection of our knowledge the whole subject of semeiology is

beset with philosophical difficulties; and no advantage will be gained by conducting our inquiry concerning it by a stricter method than its own nature will bear.

Whatever it is that bespeaks the presence of diseases, or denotes their nature or their seat, or, moreover, whatever indicates the proper method of treating them, may be equally regarded in the character of a symptom. At present I shall restrict myself to the symptoms which denote the disease, reserving those which indicate the remedy for future consideration.

There are certain popular tokens of disease which all the world is acquainted with. A man is generally known by his friends to be ill before he submits his case to a physician. They judge, and judge rightly, from his complexion, his aspect, his voice, his gait, or something unusual and unsatisfactory in his whole form and behaviour. And these circumstances, which strike every body, are not unobserved by us. All can gather from them that disease is present somewhere; but *we* can often draw from them intimations of its very nature and seat.

But how much soever physicians may learn from what constitutes the physiognomy of diseases in its largest sense (and, indeed, they may learn a great deal), their more accurate knowledge is derived from symptoms which admit of a more exact analysis. Upon such I propose principally to dwell.

Of symptoms some belong immediately to the part affected. They proceed immediately from it, and are referred immediately to it. We will call these *direct symptoms.* Others, belonging originally to the part affected, declare themselves through the medium of other parts, or through the medium of the constitution at large. We will call these *indirect symptoms.*

To begin with direct symptoms, there are those which respect the sensations of the part, in whatever way they may differ from what is natural.

Now concerning unnatural sensations as evidences of disease, to what extent they exist, or whether they exist at all, we are not competent absolutely to determine. In matters of feeling we must depend entirely upon what our patient tells us. Every man smarts with his own pain; himself, and nobody else, can say how much. We must presume, therefore, that our patient has no disposition to deceive us; and, giving him credit for the correct expression of his own feelings, we must act upon his information concerning them.

Pain and uneasiness! These are general terms. But there is more to be learnt upon the subject of morbid sensations than what these terms are calculated to convey.

There are qualities and peculiarities of pain arising from parts which are disordered, diseased, or injured, as there are qualities and peculiarities of sensations arising from parts which are healthy.

Ask the man whose leg has just been amputated, and he will tell you that he suffered one kind of pain when the knife divided the skin, and another when it cut through the muscles; and that sawing through the bone gave him still a different pain. Ask the man who has just suffered the operation of a violent purgative, and he will tell you, that after taking it he first felt oppression and nausea; that presently the nausea became fainter and fainter, until it was exchanged for real pain; twinges and griping arose, and became more and more acute, until they were relieved by an evacuation from the bowels.

Here we see various peculiarities of uneasiness arising from the same mode of irritation applied in succession to the different parts of the intestinal canal, and the different structures which go to the formation of the leg.

Pain in vital parts is different from common pain; and the pain in one vital part is different from the pain in another. In the brain it is heavy and stupifying; in the heart and lungs, and contiguous structures, it is apt to be acute, and generally much circumscribed, and confined to a spot; in the liver, the uterus, and testicles, it is oppressive and sickening.

Pain varies also in parts not vital. In skin, cellular structure, and muscle, it rouses and excites; in tendon, ligament, and bone, it rather oppresses; in nerve, it is numbing, prickling, or intolerably acute, and often runs along a string (as it were) to a considerable distance. Witness tic douloureux; witness the sciatic affection.

But, besides the qualities and peculiarities of pain belonging to several parts, and denoting *generally* their unhealthy condition, there are those which belong to the same parts under different states of disease, and are thus expressive of *the kind* of morbid action which produces them. There is what is called the throbbing pain, in which the patient, simply by attending to the part affected, may count his own pulse: this throbbing pain is characteristic of inflammation just at the point when it is passing from the adhesive to the suppurative stage. There is a pain which is called lancinating, almost the constant concomitant of cancer, and very different from the pain which would attend common inflammation in the same parts of the body.

The sense of pain is in proportion to the magnitude of the disease, only within certain limits. The extremity of the disease may abate or even abolish the sense of pain altogether. Thus, there are circumstances in which it is in vain to seek to learn the existence of pain by interrogation merely: the sense of it must be awakened by hard pressure or rough handling of the part in which the disease is thought to reside; and sometimes even these are insufficient for the purpose; the sense of pain is irrecoverably gone. Surely there is a benevolent intention conspicuous in all this. The way of death is often smoother than the path of life; and great bodily anguish (there is reason to believe) does not often enter largely into the process of dissolution.

But I do not wish to insist upon pain, its quantity, and peculiarities, as infallible criteria by which to detect what is the seat of the disease, and what its kind. It is possible to speculate too curiously upon morbid sensations; to speculate even so far as to deceive ourselves respecting them. Besides, the patient himself, by an over anxious and over constant attention to what he feels, is liable to miscalculate the kind and quantity of his own sufferings, and thus mislead you by exaggerating every little ache into an intolerable grievance. Again, the patient himself, by habitual disregard of what he feels, is liable to miscalculate in the opposite way. He either has no pain, or he owns to none, where another would complain of a great deal; and thus he misleads you by extenuating a real grievance, or entirely passing it by. Neither of these can be safely trusted for a correct interpretation of their own sensations

It is an important practical truth, well worthy of being remembered, that diagnosis is capable of being greatly aided or greatly obstructed by the *personal character* of the patient. Education, and the better habits of civilized life, render men more rationally attentive to their internal sensations, and better able to describe them; whereas over refinement engenders such excessive care and regard of the feelings, that it contrives to sophisticate and spoil them; and barbarity acts so much in spite of them, that it blunts or nearly abolishes them altogether.

Plain sensible men, who feel just what they ought to do, and tell just what they feel, are the most agreeable patients to attend. You make out their complaints easily and satisfactorily; they have noticed the first *real* deviations from healthy sensation, and can describe them intelligibly; and they obtain from you an earlier and more certain relief. But soft, delicate, nervous persons, who feel extravagantly, and still exaggerate what they feel, are very troublesome to deal with. You are not certain that they do not deceive both you and themselves; and such a perplexity is cast over their complaints, that you can neither understand them nor treat them properly. Again, the stupid, half civilized, who are often literally *insensible* to their disease until it has endured a long time, and made a considerable progress, and done formidable injury to the parts concerned, can hardly give you any help to the knowledge of their complaints by their own description of their feelings.

I have often remarked, in the victims of extreme intemperance, that they have little or no consciousness of the pains and disordered sensations proper to the diseases which they suffer. This strange want of correspondence between the symptoms of disease derived from other sources and those derived from the sensations, is a subject of very curious speculation medically, and of very melancholy interest morally; for the chief cause of the anomaly I believe to be really that to which I have alluded. There are whole classes of society in London who are never actually sober for years together. The sensations proper to health and to disease are alike unknown to them. In health, the stimulus of spirits, renewed day by day and hour by hour, gives them feelings and excitement which are unnatural; and however they may be mistaken for those of health, do in truth not at all belong to it. They are better, perhaps, and more pleasurable than any that health has to give; and they have superseded them. In disease (disease which it has itself produced), the stimulus of spirits gives them feelings and excitement which are still unnatural, and disguise or supersede the sensations which they then ought to have.

People are frequently brought into this hospital just ready to perish of complicated visceral disease, yet declaring that they never suffered ache or pain in their lives until a few weeks ago. Their liver, spleen, kidney, and heart, and blood-vessels, are all disorganized. They are breathing, perhaps, with one lung; and the cellular structure, and some cavities of the body, are distended with fluid. Here is disease which must have been the growth of years; yet true it is, as they say, that they have felt neither ache nor pain until within a few weeks. Spirits—spirits more or more recklessly taken, have sustained, and excited, and cheated them, with false strength and false feelings, till fluid has gushed out every where, and vital organs have been suddenly oppressed, and down they have sunk at once, and irretrievably.

Nevertheless, the inquiry into morbid sensations is most interesting in itself, and most proper and necessary to be pursued for pathological and practical purposes.

There are complaints of sensation, and sensation merely. People feel burning heat and pinching cold, in opposition to the indications of the thermometer, and in opposition to the perceptions of the physician, who applies his hand to the person of his patient, without being able to confirm the fact by his own feelings. People will complain of severe pain upon some external surface, which exhibits no visible mark of disease.

These complaints of sensation, and sensation merely, often occur in those whom you cannot suspect of an intention to deceive you. They are often real diseases, and being such, are generally most difficult to cure. But these complaints of sensation, and sensation merely, are those which people most frequently counterfeit when they have an interest in being believed to be ill; and they often counterfeit them successfully, owing to the extreme difficulty of detecting the deceit. Yet even here the physician would make the probability of deception less, if he were acquainted with the kind of pain which is usually felt in that part to which the patient refers it.

But, although we may learn from this order of symptoms all that they are in their own nature capable of teaching us, yet, in almost every case we meet with, we shall find a necessity of inquiring into other symptoms, if we would know the real condition of the part which falls under suspicion of disease. The majority of complaints are not such as declare themselves by *this* or by any one order of symptoms only. Diseases of *mere* sensation are very few.

Now there are other direct symptoms (symptoms immediately referable to the part affected) besides those which respect its sensations; and these are in truth more important, on account of the more certain information which they are calculated to convey. These other direct symptoms respect the functions of the part.

If the patient own to pain in a part, we suspect that part to be the seat of some morbid affection; but we are not satisfied that it is so, nor can we tell what the morbid affection is, until we have made further inquiry.

Suppose a man complains of pain in the head. It may be a mere nervous pain; it may be a sick headache; or it may be a symptom of inflammation of the brain. But we cannot tell what it is, and (what is worse) we cannot prescribe with any reasonable chance of procuring relief, until we have ascertained many more particulars concerning it.

Or if the patient own to no pain, yet, if a part has fallen under a suspicion of disease, we cannot be satisfied that it is healthy until we have made other inquiries. I have known people die of diseases of the brain, of the lungs, of the heart, who have suffered no pain whatever.

It appears, then, that whether we learn much or little, or nothing, respecting the sensations of a part, there is always a necessity for further inquiry, if we would know the nature of its complaint.

Our further inquiry is still into direct symptoms, viz. those which respect the functions of the part.

The symptoms which respect function are of much more practical value than those which respect sensation; and for this consideration especially, that the knowledge which they convey is less fallible in itself, being the result, not of what we ask and another tells us, but of what we see and note for ourselves. In obtaining it, we depend not at all upon the representations of the patient, but entirely upon our own observation and reasoning.

But, concerning the direct symptoms which respect the functions of parts, and which consist in the various deviations of those functions from their healthy state, let this especially be borne in mind, that, valuable as they are in themselves, to *us* they will be of much, or little, or no use, according as we take much, or little, or no care, to prepare and capacitate ourselves for understanding them. *Every body* cannot tell when and how the functions deviate from what is natural. A competent acquaintance with physiology must precede and prepare us for such knowledge. We must begin with what is natural and healthy, and afterwards inquire into what is unhealthy and disordered; and thus learn the latter by comparing and contrasting it with the former.

As the anatomy of healthy structure must always be the beginning and foundation of morbid anatomy, so must the physiology of healthy function be always the beginning and foundation of morbid physiology; for by this name of morbid physiology I will venture to call the knowledge of all the various ways in which the functions of the living body and its several parts are capable of being perverted and deranged.

Some interruption or derangement of their ordinary functions probably always attends disease or injury in every part and structure of the body; and such interruptions or derangements, being discovered,

are the symptoms which bring us home to the seat of complaint more surely than any other.

But there are parts in which they are not discoverable; namely, those of which the ordinary functions are unknown, as the spleen. And there are parts in which I will not say that they are not discoverable, but only that they are never discovered—namely, those whose functions are so mixed and blended with the functions of other parts, that it is impossible to determine how much belong to them and how much not, either in health or in disease. Who shall say when the pancreatic secretion is redundant or defective, or of an unhealthy quality?

But the brain and the nerves; the heart and the blood-vessels; the lungs, the liver, the stomach, and the kidneys; all give direct intimations of their diseases, by the interruptions and derangements of their ordinary functions.

The brain and nerves exhibit direct symptoms of their diseases in every manner and every degree in which sensation or voluntary motion, the senses or the intellect, are capable of being impaired or perverted. The heart and blood-vessels exhibit direct symptoms of theirs, in the strength or weakness of their pulsations, their unusual extent, their unequal succession, and the sounds accompanying them; also in many qualities and varieties of the pulse, and in the unequal course and distribution of the blood itself.

All are acquainted with the direct symptoms which impute diseases to the lungs, when, instead of a respiration which should be easy and uninterrupted, there is panting and wheezing, and stertor, and cough; and when, instead of the humid vapour which in health is separated by the bronchi, and mingles and glides forth with the breath, there is a hard and difficult expectoration of phlegm, of mucous or purulent secretion, or of blood.

But the direct symptoms which impute diseases to the lungs most unequivocally, and which make the most precise discovery of their nature and seat, are not of common or popular apprehension. There is a method by which the entire lungs, and each separate portion of them, can be scrutinized, and by which we can learn where respiration is perfect, and where it labours. It is the method of auscultation. And this method of auscultation does not merely discover a defect or failure of function in the lungs at this part or that, and so leave us to infer, from reasoning or from other circumstances, the exact nature of the disease (this, indeed, would be a great deal, and as much as the direct symptoms which respect the functions of parts are generally able to do); but auscultation often leads to more — discovering not merely the symptom, but the disease itself.

The excess and defect of bile, and various qualities of that secretion different from those of health, are the *direct* symptoms by which the liver shews itself morbidly affected. And the common consequences that immediately flow from impediments to the digestive function are the direct symptoms by which the stomach declares its complaint. Such are distentions, eructations, and rejected food, which has undergone the process of fermentation, and become acid and putrid: for these plainly shew that the substances submitted to the stomach have been left to suffer the chemical changes to which they are naturally obnoxious, the organ having lost its controlling power over them.

So, too, in the various changes which the urine is apt to suffer, in its excess and its defect, in the predominance of an acid or an alkaline quality, in its amorphous sediments and its chrystalline deposits, we have the direct symptoms which lead us to search for disease in the kidneys.

But each of these systems and organs requires from the student an express acquaintance with its natural functions, before he can be prepared to examine and appreciate their errors and defects; and then these errors and defects themselves he must expressly study, before they can yield him all the information which, as direct symptoms of disease, they are calculated to convey.

Now, whatever part be affected, when we bring our own knowledge of its disordered functions, and add it to what the patient tells us of its disordered sensations, we shall generally come somewhat nearer to a right notion of the seat and nature of the complaint; sometimes, indeed, to a *perfect* comprehension of it, so far as it is capable of being ascertained at all; inasmuch as there *are* conditions of disease into which no further inquisition can be made when we have learned the feelings and the functions of the part to which they belong.

Many of the local complaints which we are called upon to witness and to treat, are *not* of a nature to affect the structure of the part. In them it is not clear that there is any thing to be ascertained beyond the symptoms which respect its sensations and functions; whether, if the part were laid bare to us, and we could see and handle it, we should have any better notion of its complaint, or how to treat it. A man has a pain in his stomach, and he cannot digest, and yet he has no organic disease; and this being the case, I really do not comprehend how we are more

likely to learn the cause of the pain and the indigestion, or the proper method of its relief, seeing the stomach, than seeing it not.

But with all the knowledge of disordered functions which our best observation can furnish us, and all the knowledge of disordered sensations which the patient's faithful interpretation of his own feelings can supply; with all the light which sensation and function can, as direct symptoms, throw upon the disease of the part affected; still we often need *other* symptoms and *more* light to inform us what the disease really is, and what its treatment should be.

There is yet another order of direct symptoms besides those which *immediately* respect the sensations and functions of the part—viz. those which *immediately* respect its structure.

When parts are within reach of the sight or the touch, we can often judge whether their forms and structure be different from what they ought to be, and thus obtain direct evidence of their disease: and this evidence may be all that we require. What we see, or what we feel, may convey to us a complete knowledge of the disease, and render all consideration of other symptoms unnecessary. Sometimes these visible or tangible deviations from healthy structure are not only the direct symptoms expressive of the disease, but the disease itself.

In the morbid affections of external parts we can examine at once their state and structure, and learn in what respect they differ from what is natural; and the observation of this *direct symptom* almost supersedes the necessity of inquiry for any other; for herein we witness the very manner and process of the disease itself. We see the increased vascularity and tumescence which constitute inflammation, the lymph which is the material of adhesion, and the fluid which is the essential product of suppuration. We see all those sensible changes in the condition of the skin and cuticle which constitute the many orders and varieties of cutaneous diseases—rashes and vesicles, and pustules, and scales, &c.; in which it is obviously impossible to distinguish the symptom from the disease, or the disease from the symptom.

Again: we learn by the touch that the os uteri is changed in structure—that it is scirrhous, or that it is ulcerated, or that a polypus passes through it or grows from it: and here we have not only the palpable evidence of the diseases, but the diseases themselves.

But our business, as physicians, is chiefly with internal parts and organs; all of which are beyond the reach of our sight, and but a few perceptible to the touch; and these few only under certain conditions of disease.

Some organs of the abdomen, when disease has produced an increase of their natural bulk, become palpable through the integuments, and allow an examination of their shape and dimensions. But, after we have in this manner obtained such information as is possible respecting the structural condition of an internal organ, let us be careful to estimate it properly, and not to value it for either more or less than it is worth.

To ascertain by the touch that certain organs within the cavity of the abdomen have undergone an augmentation of their natural bulk—that the liver, or the spleen, occupy a space far exceeding that which nature has allotted them, beneath the cartilages of the ribs on either side—is, without doubt, to fix disease upon them. But it is not to determine the nature of the morbid processes which either are or have been in action within them. Finding the spleen enlarged, or the liver enlarged, we have the palpable result of some morbid action; but what that morbid action has been, and whether it is still in progress (the only questions which are pathologically or practically important), we must seek to discover by other symptoms. The increased bulk, then, of an organ, is a symptom, a direct symptom, of great value and certainty in fixing the *seat* of disease, but nothing more.

But it is by another sense that we are admitted to a much more intimate scrutiny of an internal part than any which the mere touch can afford, and thus obtain *direct symptoms* of its disease; which are often as infallible as those derived from sight itself.

By auscultation we not only become acquainted with the remote effects or ultimate results of morbid action, but often, when the disease is just doing its first rudiment of injury, the secret of its proceeding is betrayed to the ear.

The ear not only discovers the vomica or cavity which is the last of many changes wrought by tuberculous disease upon the structure of the lungs; not only finds their permeable texture converted into a solid mass by the gradual deposition (it may be) of tuberculous matter, or (it may be) of inflammatory lymph; but it can detect the first effusion into the vesicles of a single lobule which is produced by inflammation: it can detect pneumonia almost, perhaps altogether, simultaneously with its beginning to exist.

ST. BARTHOLOMEW'S HOSPITAL.

Row in the Operating Theatre—Excision of the left half of the lower Jaw.

On Saturday last a scene was enacted at St. Bartholomew's Hospital, which, for the credit of the profession, we hope will not be repeated.

Nearly all the students of the hospital had been attending Mr. Earle's clinical lecture, delivered immediately preceding the hour at which operations are performed, when, on quitting the lecture-room, and proceeding to the operating theatre, they found it occupied and actually besieged by a crowd of pupils not belonging to the hospital.

Mr. Earle addressed them, requesting them to give way and allow the students of the hospital to enter, at the same time promising that all who were medical students should be admitted after their own pupils had been accommodated. He further urged the impropriety and cruelty of their conduct towards the patient who was about to undergo a very severe operation, and was in a state of anxious suspense. It was in vain to remonstrate with them, as they resolutely kept possession, making a most disgraceful noise and disturbance close to the wards where the patients about to be operated on were waiting. His arguments being of no avail, Mr. Earle was obliged to declare that he would not perform the expected operation, and ordered his carriage to the door. After nearly an hour's delay, and by means of the active interference of the treasurer and beadles, the door-way to the theatre was cleared, which was soon occupied to an overflow. Among the visitors we observed M. Jules Cloquet and other distinguished foreigners, who, we fear, must have received a bad impression of the character of our medical students from witnessing such a scene as has been described.

The operation which had excited so much interest was the removal of one half of the lower jaw from a young man named Richard Garden, æt. 25, who was admitted on the 22d of October, with a large irregular tumor occupying nearly the whole of the left side of the lower jaw, extending from near the centre to the ramus, which was so much expanded towards the pterigoid processes of the sphenoid bone, that the exact boundaries of the disease could not be defined; but as the motion of the jaw was perfect, it was supposed not to extend to the articulation. A long elliptical incision was made from the centre of the jaw to about midway between the angle of the jaw and the external meatus auditorius. This incision exposed the edge of the masseter, and divided the facial artery; but this latter was not tied, as Mr. E. said he must divide it again below the jaw, where it emerges from the submaxillary gland. The integuments were carefully dissected downwards with the knife close to the enlarged bone; the submaxillary gland was detached, and the artery secured. A passage having been made in front, the bone was partly sawn through, and then divided with a very powerful pair of forceps. The bone was very hard and compact, and required much force to cut it through; but this was effected without any splintering, and presented a healthy even surface. The masseter muscle and parotid gland were next turned up, and the membrane within the mouth carefully separated. The greatest difficulty presented itself at the upper and inner part, where the ramus was expanded to the thickness of more than an inch. The walls of the tumor were, however, thin at this part, and partly with a strong knife, and partly with the forceps, the tumor was cut through near its upper extremity. The contents of the bony tumor came away entire, but a small portion of the bony cyst was left, with a thick vascular membrane lining it; this membrane was easily detached, and the remaining portion of the bony walls was subsequently removed. In all, four arteries were tied; and as the bleeding had nearly ceased, the divided integuments were approximated with three hare-lip pins and two common sutures. The patient complained of severe pain in his ear, which subsided after two doses of opium.

He has continued to go on most favourably; and when the sutures were removed on the Tuesday, the entire wound was found perfectly united. He has had a very profuse flow of saliva, tinged with blood, and suffers little or no pain, being able to speak and swallow without difficulty.

The tumor, which was nearly three inches in thickness, consisted of the expanded walls of the inferior maxillary bone, lined with a thick vascular cyst, which contained a mixed structure, in part approaching to fungus, but in others having a fibrous character, with some portions of bone running irregularly into its substance. On the upper and outer surface the bony cyst was so thin that it yielded to pressure, and conveyed an impression to the finger like a portion of thick parchment.

Mr. Earle stated, before the operation, that he once met with a case in which this same sensation could be felt, and which proved to be a thin bony cyst containing a

fluid, and a tooth which had taken a wrong direction; in which case it was only necessary to pare off the bony cyst from the surface of the jaw, and the case terminated well; but in the present case the tumor extended so far beneath the tongue, and towards the basis cranii, that he could not expect to meet with a similar state of parts, although the sensation conveyed to his finger was precisely similar.

When the above operation was concluded, Mr. Stanley removed a large pendulous cellular tumor from the labium of a female, similar in structure to some cases related by Mr. Lawrence, in the Medico-Chirurgical Transactions.

THE POOR LAW SYSTEM.

QUERIES ADDRESSED TO PROVINCIAL PRACTITIONERS REGARDING PAROCHIAL MEDICAL RELIEF.

To the Editor of the Medical Gazette.

Sir,

At the last anniversary of the Provincial Medical and Surgical Association held at Oxford, a committee was appointed to consider and report on the best means of affording medical relief to the sick poor, especially with reference to the Poor Law Amendment Act.

As secretary to this committee I am desired to solicit you to further their objects by inserting the present communication, and by supporting it with such arguments as your own just views of the subject may suggest.

The lamentable effect of recent measures both on the sick poor, and on the medical profession, and the determined attempt to *continue* and *justify* them, evinced by the last report of the Poor Law Commissioners, (dated Aug. 8th, 1835) demand the most decided and unanimous conduct on the part of the profession: our opposition will, however, possess but little *moral* force if it be not supported by substantial *reasons*; and these reasons can only be deduced from a correct and ample compilation of *facts*, which are but too frequently to be met with where the new law has been carried into effect.

These considerations will, we trust, induce our professional brethren, residing in those parts of the kingdom already under the operation of the Poor Law Amendment Act, to supply us largely with answers to the subjoined queries: by so doing the labours of the committee will be rendered more efficient, and it is hoped that in consequence a more suitable, more just, and more humane system of parochial medical relief, may be adopted.

[The questions are addressed to individual country practitioners.]

1. What has been the mode of appointing and paying medical officers of parishes in your neighbourhood for the last few years?

2. Has any alteration in the above mode taken place since the introduction of the Poor Law Amendment Act?

3. Are an *equal number* of medical men provided for the poor as formerly?

4. What is the *population* or extent of the district or districts entrusted to one medical officer?

5. What is the *greatest distance* of patients from the medical officer? and is that greater or less than under the old system?

6. How are *orders* for medical relief obtained in ordinary cases? and how in urgent?

7. Have *tenders* been required? and have they been furnished? and to what extent?

8. What are the amounts of the stipends? are they fixed annual sums? or payments *per case?* If the latter, is any gradation according to the numbers or distance of the patients allowed? and is any limitation to the sum total exacted?

9. Are the forms of the contracts in any of their clauses degrading to the respectability of the profession?

10. Have there been any instances of distress and danger to the sick poor? and to what *direct* causes are they attributable?

11. Are there any other particulars bearing on any of the above questions, which occur to you as worthy of remark?

As the prosecution of this inquiry is likely to be attended with considerable expense it is suggested that, on public grounds, communications addressed to the secretary should be post paid.—I am, sir,

Your obedient servant,

H. W. Rumsey.

Chesham, Oct. 26, 1835.

HENDERSON'S EDITION OF CUVIER.

GROSS BREACH OF ENGAGEMENT WITH THE SUBSCRIBERS.

To the Editor of the Medical Gazette.

Sir,

The following is, perhaps, one of the grossest booksellers' jobs that has ever been perpetrated in this country, and should be exposed in the most public manner.

In the course of last year, Mr. Henderson, of No. 2, Old Bailey, issued the first number of a translation of Baron Cuvier's

Animal Kingdom; upon the cover of which appeared the following address:—

"The work will consist of 36 numbers; each will be sold at one shilling. It will appear uninterruptedly the first of every month, and the contents will be letter-press and plates."

The work accordingly proceeded agreeably to the advertisement to the 14th number, but now two numbers (14 and 15) were united, and issued as one, price two shillings; which experiment of the publishers succeeding, they grew more bold, and on the cover of the 22d and 23d numbers appended the following address:—

"The public will immediately see, that according to the ordinary plan of issuing the plates which illustrate 'Cuvier's Animal Kingdom,' the proprietor could never fulfil his engagement and supply the complete quantity. He begs, therefore, to apprise the public that he will take the opportunity, during the ensuing months, of preparing numbers which shall consist wholly of plates."

Accordingly, when they arrive to No. 28, they lay aside the disguise of calling each part two numbers, and boldly issue No. 28, and each succeeding number, at the advanced price of two shillings.

On arriving at the 57th number, the following impudent address appears on the envelope:—

"The proprietors of 'Cuvier's Animal Kingdom,' which is now completed, in so far as the body of the work is concerned, have to intimate to their numerous subscribers that there will be appended to it, besides a general alphabetical Index of its contents, a careful, concise, and comprehensive synopsis of all the matter found in the work, which will form in itself a complete compendium of the 'Animal Kingdom.' There will also be published an explanation of the plates, in a separate form, so as to afford an opportunity of having them bound up with the work, or otherwise, as circumstances may suggest.

"The proprietors have also to state, that in every succeeding number there will be twelve plates, until the whole are published, without any addition being made to the usual price."

Now it might be expected, from the above address, that the work really was completed, and that the succeeding parts were merely designed to render it more complete; which the purchaser might or might not choose to possess: but no such thing; not only is the text not completed, but in the very last numbers which have appeared (74, 75, and 76) are the following plates:—"Over, pl. 2, 5, 6, 11, 14, 19, 20, 21, 22, &c. &c."—besides several of the early plates, relating to classes Pisces and Mammalia.

The manner in which the work has been thus spun out is as follows:—The proprietors have found no difficulty, out of the innumerable varieties of the animal kingdom, to get up a succession of cheap plates, illustrative of the varieties of the species; which, however, are wholly unnecessary for the illustration of the text. Thus there is Pl. 33, Pl. 33, bis; Pl. 33, ter., Pl. 33, quarter, &c. &c.

Now, sir, I appeal to any one, whether it is not a gross imposition to be obliged to pay 6*l.* 4*s.* (and God knows how much more before the work is completed) for what was originally announced for 1*l.* 16*s.*? But I call upon the proprietors, not merely in the tone of indignant complaint, but for the further purpose of bringing them to deserved punishment. It is my fixed determination to call upon them in a court of law to disgorge their ill-gotten gains; and it is for this purpose that I now call on you to expose the job, in order that other subscribers may come forward, and make common cause with myself against imposition.—I am, sir,

Your obedient servant,

P.

November 4, 1835.

DEATH BY VOLUNTARY ABSTINENCE—MEANS OF PREVENTION.

Morey, the supposed accomplice of Fieschi, is said to have determined to die of inanition. Grave questions are raised by some of the French journalists whether he ought or ought not to be *forced* to take food: some of them doubt whether it were even justifiable to introduce alimentary matter into his stomach by the gum elastic tube. In this country, we believe, there would be no hesitation in preventing a man from committing *suicide,* no matter in what way he proposed to effect his purpose: the law here does not allow a British subject to make away with his own life. We wish a simple method were tried on M. Morey, which has been suggested by a French practitioner—namely, to place him on his back, keeping his head steady, and then to pour down his nostrils some nutritive liquid: it would reach the pharynx at once, which would involuntarily contract upon it, and convey it along the gullet. Not a drop would get into the air-passage if the head were only kept steadily fixed.

ISSUE OF THE *DE VENTRE INSPICIENDO* CASE.

Mrs. Ann Fox, in whose case the ancient and indecent writ for ascertaining the existence of pregnancy was sought to be revived (see our last vol. p. 697), was delivered of a son and heir a few weeks since.

DEATH OF SIR DAVID BARRY.

It is with much regret that we have to announce the sudden decease of this gentleman, which took place on the morning of the 5th, at his house in Welbeck-Street.

Sir David was seized on Wednesday, about one o'clock, when walking in the Haymarket, with extreme faintness, and was carried into a shop. Dr. James Johnson was immediately sent for; but being from home, Mr. H. J. Johnson went to his assistance, and had Sir David taken home in a coach, when messengers were dispatched for Dr. Copland and Mr. M'Intyre. All the above-named gentlemen arrived soon after, by which time their patient had rallied considerably. He complained of pain in the belly; and having eaten some fruit on the preceding day, which was supposed to have disagreed with him, a warm cathartic was administered. The bowels were moderately acted upon in the course of the evening, and he was seen by some of his attendants at ten o'clock, apparently better. There was then no tenderness about the abdomen; his pulse 84, and natural; and his own impression that he had been seized with some mortal affection, which had at first existed, had passed away. He stated, however, that he experienced an odd kind of uneasiness in the belly on turning himself in bed, and his breathing was observed to be rather short. He requested to have something to take in case of again becoming faint; which request was accordingly complied with. A few minutes past one in the morning he had occasion to get up, uttered an exclamation that he was dying, and instantly fell back in his chair, dead.

The body was examined on the evening of the 6th, when the cause of death was found to have been an aneurism of the descending portion of the thoracic aorta. It had burst into the mediastinum, and thence into the right thoracic cavity, which contained an immense quantity of blood.

Sir David Barry was in his 56th year. He was for some years in the medical department of the army—originally, we believe, in the Portuguese service. Latterly he had been a good deal employed by government in investigating the subject of epidemic diseases, particularly yellow fever and cholera; in regard to the latter of which he was a strong contagionist.

ROYAL COLLEGE OF SURGEONS.

The announcement in our last number, regarding the recent election, was incorrect. Mr. Green obtained his seat in the Council on the resignation of Mr. Lynn. The vacancy caused by the death of Sir William Blizzard, has been filled up by the election to the Council of Mr. Callaway. The vacancies which simultaneously occurred in the Court of Examiners were filled,—the former by Sir B. Brodie, and the latter by Mr. Samuel Cooper.

We are compelled, by want of space, to omit for this week the List of Gentlemen who received Diplomas at the College of Surgeons last month; also the names of those to whom Certificates have been granted by the Society of Apothecaries.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Nov.* 3, 1835.

Abscess	5	Inflammation	39
Age and Debility	44	Bowels & Stomach	5
Apoplexy	13	Brain	5
Asthma	14	Lungs and Pleura	3
Cancer	7	Insanity	5
Childbirth	5	Jaundice	2
Consumption	101	Liver, diseased	6
Convulsions	50	Measles	13
Croup	3	Mortification	5
Dentition or Teething	15	Paralysis	4
Dropsy	26	Scrofula	2
Dropsy on the Brain	20	Small-Pox	22
Fever	18	Sore Throat and Quinsey	2
Fever, Intermittent, or Ague	1	Spasms	2
Fever, Scarlet	24	Stricture	1
Fever, Typhus	3	Unknown Causes	4
Gout	2		
Heart, diseased	4	Stillborn	38
Hooping Cough	15		

Increase of Burials, as compared with the preceding week . . . 62

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Oct. 1835.	Thermometer.		Barometer.	
Thursday . 22	from 30	to 51	29 62	to 29 50
Friday . . 23	38	48	29·55	29·53
Saturday . 24	43	52	29·59	29 67
Sunday . . 25	37	55	29 68	29 30
Monday . . 26	42	52	29·23	29·39
Tuesday . . 27	36	49	29 62	29·78
Wednesday 28	30	48	29 88	29 95

Prevailing winds, S.W. and S.E.

Except the 27th and 28th, generally cloudy, with frequent showers of rain.

Rain fallen, 1 inch and ·3 of an inch.

Charles Henry Adams.

NOTICES.

We are obliged to Mr. Foote for his information, but we had anticipated him.

"A. B." should have authenticated his letter; at least confidentially.

"Expertus" might have saved himself the trouble of writing his puffing letter about a new publication; it was quite enough to have received one from the author himself. We shall notice the work in due time, should it prove deserving of that attention,—which we very much doubt.

The letters and papers of several correspondents, which have been unavoidably postponed, shall make their appearance next week.

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THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 14, 1835.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE VII.

AT our last meeting I took a brief general survey of the different methods of arranging the articles of the materia medica, and at the same time offered a few examples of each mode of classification: I also stated my intention of following in these lectures the natural-historical method; but as I shall, hereafter, have frequent occasion to refer to the effects of those generally admitted classes, formed by associating medicines operating on the body in an analogous manner, I have thought it necessary to premise a short account of them, and thereby to save much subsequent repetition.

Class I. *Cerebro-spinants.* — The first class to which I must beg your attention is one consisting for the most part of the substances usually called narcotics. There are, however, several which cannot be termed narcotic, since they do not stupefy, but which, notwithstanding, I think, ought to be associated with them;—I allude now to strychnia and brucia. I propose, therefore, to include them all in one class, under the name of *cerebro-spinantia*, or agents acting specifically on the cerebro-spinal system.

The cerebro-spinants are found among mineral, vegetable, and animal substances, but those employed in medicine are almost exclusively vegetable, and are derived from the families, papaveraceæ, solaneæ, apocyneæ, umbelliferæ, ranunculaceæ, scrophularineæ, and amygdaleæ. For example, opium, hyoscyamus, stramonium, belladonna, tobacco, nux vomica, strychnia, conium, aconite, digitalis, and hydrocyanic acid. There are many substances which act on the cerebro-spinal system, though they are not employed in medicine as such, since to produce this effect they require to be given in such large doses that we thereby compromise the safety of our patient from the irritant operation. I may mention as examples, squills, veratrium, colchicum, and arsenic.

The local effects of the cerebro-spinants are far from being uniform: several of them diminish the sensibility of the parts with which they are placed in contact, and thus deaden pain: some even produce numbness, as monkshood, when applied to the lips, or hydrocyanic acid vapour to the fingers. Opium and others lessen the contractile power of muscular parts, and thus relieve spasm. In poisonous doses many of them act as powerful local irritants, and in toxicological classifications they form the class acro-narcotics. I may mention as examples, belladonna and stramonium. We take no advantage of this effect in therapeutics.

The remote effects of cerebro-spinants vary with the particular substance employed: some primarily excite the vascular system, others appear to have an opposite effect, and are, in consequence, termed *sedatives.* The only point in which they all agree is in their primary and specific influence on the nervous system. But the modifications which they producein the functions of this system, vary with the individual agent, so that we are not far from the truth in asserting that each has its own peculiar and specific mode of action on the organism, and, consequently, that no one precisely resembles another in the nature of its effects. It is, indeed, true, that some have a greater similarity in their mode of operation than others, but when

we examine them closely, we find distinct individual peculiarities. Hence, then, in describing their effects, we must proceed in generalities, for, if we attempt to descend into particulars, many remarks could only be applicable to some of these agents. The difficulty of describing the operation of this class is still further increased by our knowledge of the fact, that the same cerebro-spinant operates unequally on different persons, and in different doses. I think, however, it will be useful to group those whose effects are for the most part analogous, and which are employed in medicine for similar purposes; but no classification of them will invariably hold good.

1. The *first group* we may denominate the *opiate*, or *stupefying*; it includes the narcotics properly so called; for example, opium, hyoscyamus, and lactuca. In small doses they produce symptoms of excitement; in larger ones we find diminished sensibility, muscular contractility, and sleep or stupor, are the ordinary effects. In great quantities the phenomena are similar to those of apoplexy, and constitute what is called *narcotism*. These symptoms point out the uses of the group; in small doses to excite, in larger ones to diminish excessive sensibility, and relieve pain, hence they are called *anodynes* (from *α*, *not*, and *οδύνη*, *pain*); or *paregorics* (from *παρηγορεω*, *to soothe* or *alleviate*), to diminish inordinate muscular contractions, as convulsions or spasm (the first affecting the voluntary, the second the involuntary muscles,) and to procure sleep, whence they have been termed *hypnotics* (from *υπνωτὶκος*,) or *soporifics* (from *sopor*, sleep, and *facio*, to make).

2. The *second* we may designate the *strychnos*, or the *tetanic group*. It includes all substances whose active principle is either strychnia or brucia, or both of these alkalies; namely, nux vomica, St. Ignatius's bean, the lignum colubrinum and false angustura bark. The mental faculties are by this group unaffected; and the principal symptom is tetanic convulsions. The sensibility of the body is so far affected that the slightest touch will frequently bring on a violent convulsive paroxysm. Certain facts hereafter to be detailed, lead us to infer, that the action of these substances is confined to the spinal marrow. We employ this group principally in paralytic affections, more especially where these are unaccompanied by any organic changes of the nervous system, as in paralysis by lead. This group is by Cottereau formed into a distinct class, under the name of *tetanics*. If this be admitted, and the term narcotic applied to stupefying agents only, as opium, where ought cocculus indicus and other agents to be placed, which produce both tetanus and stupor? It appears to me the tetanics pass so insensibly into the narcotics commonly so called, that the two ought to form only subdivisions of one class.

3. The *third* or *hydrocyanic group* comprehends all substances whose active principle is hydrocyanic acid. We must, however, extend this definition, since it has been found that several vegetables which yield this acid, and are energetic poisons, do not contain it as their active principle, but that they have some other substance whose effect is analogous, and which is easily converted into it. In this group we include the bitter almond and essential oil obtained from it, the cherry laurel, &c. This group contains the poisons which are the most rapidly fatal of any known. In large doses they produce sudden death preceded by convulsions and insensibility; in smaller ones they act as *sedatives*; that is, they diminish action without causing any previous excitement. They are used in therapeutics to diminish what is called "morbid irritability" of the pulmonary and alimentary organs.

4. The *fourth* or *acro-narcotic group* contains belladonna, stramonium, conium, and aconite. This is by far a less perfect group than the three preceding ones. In poisonous doses they excite more or less local irritation, an effect particularly observable in aconite. Hence, while the three first are called acro-narcotics, the fourth is frequently termed a narcotico-acrid. Their effects on the nervous system are variable; but delirium is a frequent symptom, sometimes stupor, and occasionally convulsions. Their uses in medicine are various: belladonna is principally employed to dilate the pupil and to relieve neuralgic pains; hemlock to diminish sensibility in painful diseases.

5. The *fifth group* agrees in several points with the fourth. Thus, in poisonous doses, we find symptoms of local irritation; delirium, stupor, and convulsions, also occur. But the peculiarity of this group is the depressing effect on the arterial system; marked by faintings, feebleness, and irregularity of pulse. Tobacco and fox glove are the only substances which belong to it. They, therefore, are ranked among *sedatives*. When given in large doses, the first causes great feebleness of the muscular system, the second sometimes salivates. Both (but especially fox-glove) are employed to reduce arterial action, and tobacco on account of its purgative effect.

In addition to these we might make two other groups; one called *inebriants*, the other *nervines*: but I shall notice them under the head of stimulants.

The immediate cause of death when the

cerebro-spinants have been given in large quantities is not always the same: sometimes it would appear that the respiratory muscles do not receive their proper supply of nervous energy, in consequence of which this function is performed with increasing difficulty until ultimately death takes place from asphyxia. This is the kind of death caused by opium, and sometimes by dilute hydrocyanic acid. Before the cessation of life you observe the breathing becomes laborious or even stertorous, as in cases of apoplexy; and if you open the body immediately after death, the heart is found beating oftentimes with considerable force and for some minutes. [A rabit was killed by hydrocyanic acid, and the fact shewn.] These are the cases in which it has been proposed to prolong life by artificial respiration until the cerebral disorder has passed off. The proposition is not supported merely by its ingeniousness and plausibility, but by experience. The following is a case in point related by Mr. Whateley, and quoted by Dr. Christison in his work on Poisons. A middle aged man swallowed half an ounce of crude opium, and soon became lethargic. He was roused from this state by appropriate remedies, and his surgeon left him. But the poison not having been sufficiently discharged, he fell again into a state of stupor; and when the surgeon returned, he found the face pale, cold, and deadly, the lips black, the eyelids motionless, so as to remain in any position in which they were placed, the pulse very small and irregular, and the respiration quite extinct. The chest was immediately inflated by artificial means, and when this had been persevered in for seven minutes, expiration became accompanied with a croak, which was gradually increased in strength till natural breathing was established; emetics were then given, and the patient eventually recovered. I have several times restored animals apparently dead from the use of hydrocyanic acid, merely by keeping up artificial respiration, and Sir Benjamin Brodie has done the same with animals apparently killed by the oil of bitter almonds.

In some cases the immediate cause of death is believed to be paralysis of the heart. The grounds for this belief are, the cessation of the heart's action (in some instances even before the animal had ceased to respire); on examination after death, the heart is found distended, the left side with scarlet, the right side with dark-coloured blood. Sir Benjamin Brodie says the infusion of tobacco kills dogs and cats in this way. I have frequently tried the effect of the infusion on rabbits, but have found death is produced, as I believe, from the stoppage of respiration. [The lecturer threw an infusion of tobacco into the peritoneal sac of a rabbit: in a few minutes the animal was so far paralysed as to be incapable of standing, and its breathing became short and quick. Shortly afterwards convulsions came on, in which the animal died. On opening the chest the heart was found still beating, though much more feebly than in the animal killed by hydrocyanic acid, and all the cavities were remarkably distended with blood.] On the supposition that tobacco kills by paralysing the heart, we have been recommended to stimulate this viscus by slight galvanic shocks. Even acupuncture has been talked of as an admissible remedy, if the patient appeared to be *in articulo mortis.* I may remark, that Bretonneau has fully established the fact that we may acupuncture the brain, the heart, the lungs, &c. of animals without any inconvenience; and Carraro, an Italian physician, has successfully tried this practice on animals that had been asphyxied.

A third mode of death from cerebro-spinants is that arising from suspended respiration produced by convulsions of the respiratory muscles. We have an example of this in strychnia, brucia, and all substances containing them. If I introduce half a grain of strychnia into the peritoneal sac of this rabbit, you will observe that death will take place within seven or eight minutes, and, as far as we can judge from the convulsions which are excited, stopping respiration. [The experiment was performed; the animal became convulsed in six, and died in eight minutes.]

There are many points of considerable interest in reference to the operation of cerebro-spinants, which can only be answered hypothetically or theoretically. One is the pathological condition of the brain and spinal marrow; and whether the peculiarities in the operation of different cerebro-spinants arise from different parts of these organs being affected, or from different modes of affection: in other words, whether the peculiarities depend on variations of *locality* or of *quality* of operation. I think it must be concluded that opium operates principally on the cerebral, strychnia on the spinal, functions. But what may be the particular pathological condition of the brain or of the spinal marrow, induced by these poisons, we have not sufficient evidence to enable us to form any well-grounded opinion. There are, however, several reasons which lead us to suspect that opium produces a congested state of the cerebral vessels. Possibly other cerebro-spinants may give rise to a similar condition, but as each has a peculiarity of effect, so there must be some other conditions accompanying

this, and which is probably the cause of its peculiarity.

Another interesting subject of inquiry is the probability or otherwise of the absorption of cerebro-spinants. The active principles of many of them are not detectable, by physical or chemical means, either in the blood or secretions, so that we are deprived in these cases of positive evidence. Barbier states that the odorous matter of opium has been detected in the urine, in the sweat, and in the liquor exhaled by the pleuritic membrane; and the same author tells us that the milk of a nurse who had taken opium was narcotic. Hydrocyanic acid has also been detected in the blood and other parts of the body. That some cerebro-spinants, therefore, get into the blood is indisputable, but whether others do, we have not at present the means of determining.

But even if it were proved that the active principles of all get into the blood, it cannot be proved that the effects are the consequences of the absorption. They may be the *post hoc*, and not the *propter hoc*. The excessive rapidity of the action of strong hydrocyanic acid appears almost inconsistent with the notion of its previous absorption. Lastly, if we had good reasons for concluding that the effects of cerebro-spinants arise from their absorption, we have yet to learn the relation of the two series of phenomena—the absorption and the consequent effects. The whole subject is involved in such obscurity, and the positive facts are so few, that we can offer no theory of any great value or importance.

Class II. *Stimulants, Excitants, or Calefacients.*—The next class of substances which I propose examining is that called by some *stimulants*, by others *excitants* or *calefacients*, and which includes a considerable number of substances exceedingly difficult to characterize generally. They have all a warm, acrid, and pungent taste; and in most cases are more or less odorous, frequently in consequence of the presence of volatile oil. When swallowed in moderate doses they increase the appetite, give rise to a sensation of warmth in the stomach, expel gaseous matters and assist digestion. In larger quantities they sometimes irritate, excite nausea, and even vomiting, and produce thirst. Several of them appear to confine their operation to the alimentary tube (the carminatives), while others affect the whole vascular system, increasing the force and frequency of the heart's action, and in consequence raising the temperature of the body, and augmenting the number of respirations in a given time.

The local action of this class of substances is not uniform: many of them powerfully affect the organ of smell, as ammonia, æther, musk, the labiate plants (as the mints,) the fruit of umbelliferæ (as caraway, aniseed, &c.) laurineæ (as camphor, cassia, &c.) When applied to the skin in a concentrated state and for a sufficient length of time, some of them produce heat, redness, pain, and swelling; and if the application be persisted in, suppuration, vesication, or gangrene. As the general system is not always affected in proportion to the degree of local irritation produced, so this latter is not regarded as essential to the stimulant operation. The labiate plants have very little power of irritating or inflaming, though they act on the nerves of the tender and delicate parts, as the lips.

The classes stimulants and cerebro-spinants are closely connected. Nutmegs, for example, which are generally classed among stimulants, have a narcotic operation. Musk, valerian, and some other usually admitted stimulants, affect the cerebro-spinal system apparently in a specific manner: these agents have in consequence been called by some *nervines*. On the other hand the cerebro-spinants frequently partake of the stimulant character. Opium indeed is often employed by persons in health on account of its exciting effects. The substances usually denominated *inebriants*, as alcohol and æther, are also examples of cerebro-spinants possessing a marked stimulant operation.

Stimulants, by affecting the general vascular system, excite the secreting and exhaling organs, and thus they are frequently denominated sudorifics, diuretics, emmenagogues, expectorants, &c. As I have grouped the cerebro-spinants, I shall attempt the same with this class.

Group 1. This contains the *Acrid stimuli* used to assist the digestive process, under the name of *condiments;* but which, when given to expel wind and relieve spasmodic pain, are termed *carminatives.* This group includes the spices, as ginger, the peppers, cassia, cinnamon, cloves, pimenta, canella, &c.; the fruit of the umbelliferæ (as fennel, caraway, aniseed, dill, and coriander); the labiate plants (as the mints); and the agents frequently termed antiscorbutics, consisting of certain volatile vegetable stimuli derived from the families cruciferæ and asphodeleæ, (as mustard, horse-radish, leeks, onions, garlic, &c.)

Group 2. *Resinous stimuli,* comprehending the balsamic substances, or those containing benzoic acid, as tolu, peru, benzoin, and styrax; the solid resinous substances, as olibanum, mastic, and guaiacum; and liquid resins, or those composed of volatile oil and resin, as the turpentines.

Group 3. This contains the *diffusible*

and *fœtid* or *antispasmodic stimuli.*—Here we place the fœtid gums, (asafœtida, galbanum, sagapenum, opopanax, ammoniacum, and myrrh); the animal substances, musk and castoreum; empyreumatic oils, ammonia, alcohol, æther, &c.

Class III. *Tonics.*—I proceed now to notice *tonics*, a class of substances having considerable importance in practice, but the operation of which is very imperfectly understood. They have received their name from *τονος, tone* or *vigour*, being supposed to strengthen and invigorate the body. In this sense, however, all substances contributing to the support of the vital powers are tonics; nutrients, nay, even evacuants, may be regarded as indirectly so; since without the former life cannot be supported, and the latter by removing diseases tend to prolong it: but it is well known, in the common acceptation of the term tonic, neither of these classes of substances are intended to be included. Nutrients only strengthen by becoming digested and assimilated, whereas the digestion and assimilation of the active principle of a tonic would render it inert.

Tonics do not invariably strengthen: in some cases no obvious effects are observed by their exhibition, in others they act as irritants and stimulants. We have, therefore, still to learn what is their constant and invariable action. The strengthening or corroborant operation is observed only when the body is preternaturally weak, though not always even then. In the state of health no obvious effect is produced, except occasionally a temporary excitement of the appetite. Even in debilitated states of the system this is sometimes the only result from one or two doses. In order to produce the tonic effect on the system generally, these agents require to be exhibited for some time. We then sometimes find the muscles acquire more power, the solids become firmer, and all the functions are performed with more energy. The pulse is rendered stronger, but without being quickened; and the patient is capable of greater exertion than before.

If there be irritation or inflammation of the alimentary canal previous to the exhibition of the medicine, or if some febrile disorder be present, accompanied with a hot and dry skin and a furred and dry tongue, tonics will only add to the severity of the symptoms: they then act as irritants or excitants. In weakly and irritable subjects I have frequently seen the sulphate of quinia cause headache, and give other evidence of its action on the nervous system. These facts shew that tonics are stimulant powers; indeed it is not easy always to distinguish the classes tonics and stimulants from each other. They mutually approach and gradually pass the one from the other, so that many substances may with equal propriety be arranged under either head.

The true tonics are of vegetable origin; animal and mineral ones are indeed mentioned, but it is doubtful whether any of them deserve this name. The only animal substance which has been employed as a tonic is the extract of ox bile, which still constitutes an article of the materia medica in some of the continental pharmacopœias. A considerable number of mineral substances are termed tonic, though there appears to me but little ground in most instances for such designation. For example, the preparations of arsenic, silver, copper, bismuth, zinc, &c. have all been called tonics, and principally for the following reason: cinchona, the most powerful of the vegetable tonics, and in fact the type of the class, has long been celebrated as a curative agent in ague and other periodical diseases; hence it has been assumed that any substances capable of fulfilling the same indication must be possessed of the same properties, and thus arsenic has been called a tonic. But the mode of reasoning and its conclusion are both erroneous; it is indeed true that cinchona and arsenic have in common the power of curing an ague, but the same effect is frequently produced by many other very dissimilar substances: for example, blood letting, alcohol, and mental affections. If, therefore, arsenic be called a tonic for the above mentioned reason, these agents must be similarly denominated. If we admit this, it follows tonics can no longer be regarded as substances promoting strength, but merely as agents curing particular diseases. Before we have any right to associate arsenic among tonics, we must completely alter our definition of these substances, or show that arsenic improves the appetite and promotes strength to the body. Iron is almost the only metallic substance that can claim with any shadow of propriety the tonic property.

The mineral acids agree with the vegetable tonics in increasing the appetite, but the effect is only temporary, for by continued use, instead of improving they injure the digestive functions, thereby differing considerably from the vegetable tonics.

By many writers on the materia medica the substances called *astringents* are formed into a class separate from that of tonics. They are supposed to be distinguished by the contraction and corrugation they produce in the tissues to which they are applied, and which has been termed astriction. Thus they contract and give greater density to the muscular fibre, and diminish the calibre of the blood-ves-

sels and exhalents of the parts with which they are placed in contact.

When used to check hæmorrhage they are called *styptics*. Applied to the tongue they give rise to a peculiar sensation of roughness and stypticity. All vegetable astringents, however, are tonics: that is, they increase the appetite, promote digestion, and are capable of fulfilling the same therapeutic indications (as curing the paroxysms of an intermittent fever.) But mineral astringents and styptics are not tonics.

The vegetable bitters have the property of retarding the process of fermentation, and hence may be useful in dyspepsia, where there is a disposition to acidity and flatulence. The beneficial effects sometimes obtained in verminous diseases by the use of bitter and ferruginous preparations, has by some been referred to a poisonous operation excited by these agents on intestinal worms, a notion thought to be supported by the fact, that infusion of quassia is destructive to flies, and that the *amer* of Welther (now called carbazotic acid) is asserted by Emmert to be poisonous to fowls and lizards. But according to the experiments of Redi, bitters are not an immediate poison to worms. Baglivi mentions that a saturated infusion of wormseed (semina santonici) killed an *ascaris lumbricoides* in five hours (Redi says between seven and eight hours,) but that a much more bitter decoction of absinthium required thirty hours. Hence it is quite clear that it cannot be by its bitterness that the wormseed operates as an anthelmintic.

The local effects of the astringent tonics have by some writers been attributed to a physical or chemical agency. Dr. Cullen, for example, places astringents under the head of medicines acting on the simple solids, though in another part of his work he admits that they act on the living likewise. The late Dr. Adair Crawford supported the same opinion of the physical agency of tonics, and endeavoured to prove it by experiment. He immersed pieces of intestines, skin, &c. in various bitters and other infusions, and some in water merely; he then observed the comparative weights required to break them. From these data he inferred that bitters increase the strength of the intestines in the following order: Peruvian bark, galls, chamomile flowers, gentian root, columba root, cascarilla, myrrh, and serpentaria; but this mode of reasoning naturally leads to erroneous inferences, since the vital powers of the system are totally overlooked, and in the second place the tonics may have acted in this case merely as antiseptics.

We may establish the following groups of tonics:—

Group 1. *Simple vegetable bitters*, including gentian, quassia, columba, simaruba, willow bark, Iceland moss, &c.

Group 2. *Pure astringents*, as catechu, kino, nutgalls, oak bark, uva ursi, rhatany root, &c.

Group 3. *Astringent bitters*, as cinchona.

Group 4. *Aromatic bitters*, as cascarilla, chamomiles, &c.

Group 5. *Mineral tonics*, as iron.

Class IV. *Emollients.*—The substances called emollients next claim our attention, their action being precisely opposite to that of astringents, as they produce relaxation and weakness. Applied to inflamed parts they diminish heat, tension, and pain; they have a relaxing influence on the muscular fibre in a spasmodic condition. When used to prevent the action of irritating matters on the body, they are called *demulcents*. Mucilaginous, feculent, oleaginous, and saccharine substances, are in their local effects emollients; when digested they are nutrients.

During life, the particles of the body are kept in approximation by two forces, attraction and the vital principle; and as emollients cause relaxation, it has been supposed by some, that they do so by overcoming the cohesion of the molecules independent of the vital principle. Thus we know that warm water and oil relax dead matters (as leather), and in consequence it has been thought that they influenced living parts in the same manner, and therefore, that emollients were physical agents. But we should always be cautious in applying physical explanations to vital phenomena. In the present instance the reasoning does not appear good. It is indeed true that some emollients act as relaxants to inorganized matters, but the reverse is not the case: all relaxants are not emollients. Cold water diminishes the cohesion of dead animal matters, but not of living tissues, and the same remark applies to many other agents.

Class V. *Refrigerants.*—Refrigerants constitute another class of medicines. Various substances are used in medicine to diminish the temperature of the body when preternaturally increased, under the name refrigerants. The only agent that can with propriety bear this appellation is cold used in the form of ice, cold air, cold bath, cold lotions, cold drinks, &c. However, acidulous vegetable matters, the dilute mineral acids, the super-salts of the alkalies, and some of the neutral salts, have been used for a similar purpose.

We can readily understand the agency of cold as a refrigerant, but it is not easy to say how the latter mentioned substances can have the effect here supposed.

Class VI. *Sudorifics or Diaphoretics.*—Dia-

phoretics or diapnotics are those agents supposed to promote the insensible perspiration, while sudorifics are those that produce sensible perspiration or sweat. It is highly probable that the insensible perspiration and sweat differ from each other chiefly in their physical conditions, the first being in the vaporous form, the second in that of the liquid; though it is stated that sweat contains less carbonic acid and more salts than the insensible perspiration, but the difficulty of obtaining the latter for examination, renders any assertions of this kind very doubtful. If the cutaneous exhalation be moderate it is rapidly converted into vapour; but should it take place faster than the atmosphere can carry it off, either from the increased cutaneous transposition, or from an alteration in the hygrometric state of the air, an accumulation on the skin takes place in form of drops, called sweat. There is then no essential distinction between diaphoretics and sudorifics; indeed the same substances may be either the one or the other according to circumstances. Heat and diluents are the most powerful diaphoretic or sudorific agents, but a variety of stimulants are employed as such; for example, ammonia, guaiacum, mezereon, &c. A variety of solid substances have been used as media for the application of heat: hot sand is a very old remedy, being mentioned by Celsus and Dioscorides; bran, ashes, earth, and plaster, have also been employed. The inhabitants of Crimea employ the saline mud found on the sea-shore: hot dung is used in France against rheumatism, and by the Poles against syphilis. The husk of the grape and the refuse of the olive, from which the oil has been drawn, undergo fermentation, and in this state have been successfully employed in Paris against rheumatism. Water in the liquid or vaporous form, and dry air baths, are powerful sudorific agents. Frictions, warm clothing, exercise, and cold affusion, are among other means of promoting diaphoresis.

The opinions of Dr. Edwards regarding the manner in which cutaneous transpiration is effected, seem to require a slight notice here. According to this physiologist, cutaneous transpiration is effected in two ways: 1, by a physical action or evaporation; 2, by an organic action or transudation. The first is the consequence of the porosity of bodies, and takes place equally in the dead and living state. It is influenced by the hygrometric states of the surrounding air, by its motion or stillness, by its pressure, and by its temperature. Thus dryness, agitation, and diminution of the weight of the air, increase this mode of transpiration. Transudation, or the organic action of transpiration being a vital process, depends essentially on causes inherent in the animal economy, although it may be influenced to a certain extent by external agents. Thus elevating the temperature of the surrounding air, preventing its frequent renewal, and covering the patient with warm clothing, are means which promote this organic action, but which check the physical operation of transpiration. When, therefore, we speak of diaphoretics and sudorifics, we mean agents which promote the vital actions of the extreme exhaling vessels of the skin, by increasing the general force of the circulation, or exciting the cutaneous vessels. Ammonia, violent exercise, alcohol, &c. act by increasing the vascular action generally: whereas heat, friction, &c. operate by exciting the cutaneous vessels. The preparations of mercury, sulphur, garlic, onions, &c. have been detected in the cutaneous exhalation of persons who have swallowed these substances, and hence when they give rise to diaphoresis it has been presumed that they operate by entering the blood-vessels, and stimulating the cutaneous vessels by contact; the specific action of these substances on the skin is, however, very doubtful. The older writers explained the occasional beneficial effects of sudorifics by supposing that some peculiar morbific matter was expelled from the system, the retention of which had produced the disease. Hence such remedies were reckoned among the *Alexipharmaca* and *Alexiteria*. This hypothesis is totally devoid of foundation. Cold may suddenly check the vital action of the skin, and some internal organ may in consequence become disordered; but we ought rather to say that the affection of the internal organ is caused by a metastasis of the vital action, than by a metastasis of the sweat; for although cold diminishes the vital action of the skin (transudation,) yet it does not prevent the physical action (evaporation.) The operation of diaphoretics is facilitated by increasing the quantity of fluids in the system, which is best done by the exhibition of tepid water. They are most likely to be successful if given at bed-time, inasmuch as there appears to be a greater disposition to sweating during sleep (especially towards the morning) than in the waking state. The combination of diuretics or of purgatives with diaphoretics, always diminishes the effect of the latter. If warm liquids be taken into the stomach and cold be applied to the skin, the vital action of transpiration (transudation) will be checked, and diuresis produced.

Class VII. *Errhines.*—Errhines are local irritants applied to the mucous membrane of the nose to excite an increased discharge

of mucus, or to provoke sneezing: in the latter case they are termed *ptarmics* or *sternutatories*. The milder errhines such as sugar, act mechanically: others, for example, asarabacca, veratrum, tobacco, &c. are acrids. They are principally employed as local agents.

Class VIII. *Expectorants.*—Expectorants are medicines which promote the secretion and expulsion of mucus and other secreted matters from the bronchia, the trachea, and the larynx. In the healthy state the mucus secreted by the membrane lining the air passages is got rid of by evaporation and by absorption; but under certain circumstances an accumulation takes place, and nature then endeavours to get rid of it by coughing; the act itself being called expectoration. Hence the term expectorant has sometimes been applied to substances exciting cough, commonly to agents promoting the secretion of bronchial mucus, and sometimes to medicines which diminish bronchial exhalation. For example, Dr. Paris terms sulphate of zinc an expectorant, because in humoral asthma it occasionally moderates exhalation, and consequently renders the expectoration of the mucous secretion more easy.

Some expectorants are *local*, as the vapors of resinous and balsamic substances, dilute chlorine gas, &c.; others are supposed to act through the general system; these may be called *remote* or *indirect* expectorants: among them are the fœtid gums, tobacco, squills, the nauseating emetics, &c.

Class IX. *Emetics.*—Emetics are those medicinal agents used to excite vomiting. All of them produce in the first place more or less unea iness and nausea, the intensity and duration of which, however, bears no relation to the violence and duration of the subsequent vomiting. Thus the sulphates of zinc and copper excite speedy vomiting with but little nausea, and are, therefore, preferred as emetics in narcotic poisoning: tartar emetic, on the contrary, is a powerful nauseant, and is consequently employed when we wish to depress the actions of the system. During the stage of nausea, the pulse is small, feeble, and irregular, the face and lips pale, a sensation of relaxation and coldness of the whole system is experienced, the saliva flows copiously from the mouth, the eyes lose their lustre, and the whole countenance appears dejected. These symptoms continue for a variable period, and are then followed by the ejection of the contents of the stomach. As soon as actual vomiting commences, the general phenomena are altered: the pulse becomes frequent and full, the temperature of the body increases, and a sweat breaks out on the face and other parts of the body. During the act of vomiting, in consequence of the pressure made on the abdominal aorta, and the interuption to the circulation through the lungs, from the impeded respiration, the blood returns with difficulty from the head, the face swells, the conjunctiva becomes turgid and red, the jugular veins are gorged, and the tears burst from the eyes. When the vomiting has entirely ceased the patient feels languid, oppressed, and drowsy, and the pulse becomes weak and slow. The exhaustion is sometimes so great as to be attended with fatal consequences. Dr. Paris mentions a case in which an emetic was imprudently given to a patient in the last stage of phthisis, with the intention of dislodging the pus with which the lungs were embarrassed; syncope was produced, from which the patient never recovered.

The immediate causes of vomiting are numerous, various, and even opposite; but are reducible to three heads:—

1. *Mechanical* agents; as suddenly distending the stomach largely with warm water, or diluents of a similar kind; a practice which we resort to when we wish to excite vomiting with the least possible irritation, and to dilute at the same time. No satisfactory explanation has yet been given of the operation of this cause.

2. *Irritants* applied to the stomach; as undigested food, the acrid and caustic poisons, and the substances usually employed to excite vomiting (emetics); some act simply by the local irritation they produce; for example, mustard, gamboge, euphorbium, &c.; others have in addition, apparently a specific emetic quality, since they excite vomiting to whatever part of the body they be applied: tartar emetic and ipecacuanha are examples of these.

3. *Sympathetic means;* as tickling the throat with the finger or a feather; whirling, sailing, or swinging; certain disagreeable odours; pain; injuries of the brain; calculi in the kidneys, &c. The mode of operation of this class of agents is quite unknown.

Besides the evacuation of the contents of the stomach, emetics produce increased secretion and exhalation from the mucous lining of the stomach and duodenum. When the vomiting has continued for some little time bile is usually evacuated; and it is generally thought that emetics increase the secretion of this fluid as well as of the pancreatic juice.

Class X. *Cathartics.*—Cathartics or purgatives are those medicines which produce alvine evacuations: the mildest are termed laxatives; the strongest drastics; while those that evacuate much water are denominated hydragogues. When we reflect that the intestinal surface consists of

about 1400 square inches, on the whole of which secretion and exhalation is going forward, it must be evident that purging offers a very powerful means of diminishing the quantity of the fluids of the body. Most, if not all, are local irritants, and in some instances the effect seems to depend solely on this, as in the case of gamboge, and of euphorbium. But a considerable number of the drastics appear to have an influence over the bowels independent of their local action, inasmuch as we find them purging though they are applied to the skin, to wounds, or are thrown into the veins. This will occur with castor and croton oils, elaterium, colocynth, hellebore, &c. These are specific purgatives. The substances called laxatives have a very slight local irritant action: indeed, Barbier denies that they are irritants. Some purgatives produce evacuations without the discharge of much liquid matters: these are supposed to act principally on the muscular coat of the intestines. Sulphur, manna, rhubarb, and magnesia, have been enumerated in this list.

All the powerfully irritant purgatives probably increase the secretion of bile by irritating the end of the ductus choledochus; but mercurials, aloes, and rhubarb, are supposed to possess a remarkable power of this kind. Some of the drastics are said to act more on one part of the canal than on others: aloes, black hellebore, elaterium, colocynth, and savine, are supposed to exert a greater action over the large than the small intestines: further experiments and observations, however, are required to confirm this opinion, for the statements usually advanced in proof are not sufficient to establish it. Some purgatives are absorbed: the colouring particles of rhubarb and of gamboge, the odour of oil of turpentine and sulphate of potash, have been detected in the blood; while the colouring matters of cassia pulp, of senna, of rhubarb, and of gamboge, have been detected in the urine: senna, rhubarb, or jalap, given to the nurse, will occasionally render the milk purgative.

Class XI. *Emmenagogues.*—Emmenagogues are substances supposed to have the property of exciting the menstrual discharge. But the uncertainty of all the agents that have been said to possess this power, and the uterus not being an organ intended for the excretion of foreign matters, have led writers to doubt the existence of any agents really possessing a specific emmenagogue effect. When we consider the variety of circumstances that may occasion a suppression of the catamenial discharge, we can have little difficulty in believing that no one substance can prove emmenagogue in all cases. Deficient menstruation is rarely, perhaps, to be considered an idiopathic disease, but merely a symptom of other diseases; and therefore emmenagogues must be relative agents. Most of the medicines which have had this denomination are either stimulants (as the fœtid gums, rue, mercury, &c.) or drastic purgatives (especially those which are supposed to act on the large intestines; as hellebore, savine, and aloes.) Electricity, the warm bath, stimulating injections into the vagina, sexual intercourse, dancing, &c. are amongst the most powerful means of exciting the menstrual discharge, by determining blood towards the pelvis; but unless a disposition to the natural evacuation concurs with the remedies employed, the latter alone seldom succeed. When deficient menstruation seems to depend on, or be connected with, a want of power in the system, the employment of tonies, (as iron and cinchona) is often found beneficial. On the contrary, when the stoppage is accompanied with symptoms of general plethora, blood-letting and other evacuants are the most proper remedies.

Class XII. *Parturientia.* — I think a class should be admitted under this name, to include those agents that assist the parturient efforts of the womb. The ergot of rye may be taken as the type. This class will correspond with the *Abortiva* of Eberle.

Class XIII. *Diuretics.*—Diuretics are substances which are supposed to promote the secretion of urine. Now there are two ways of increasing this secretion: one is by augmenting the quantity of fluids taken into the stomach, or by removing any cause which diminishes the secretion; the second is direct, by means which are supposed to act specifically on the kidneys: the latter are the diuretics properly so called; but they are so exceedingly uncertain in their operation that their existence has been doubted. In fact, this secretion is liable to great variation under ordinary circumstances, so that it is not always easy to say whether a medicine has proved diuretic or not. Temperature, season, climate, and time of day, are among the circumstances which influence the quantity of urine discharged, which has been enormous sometimes even without the use of diuretics. Dodonæus mentions a man who discharged forty pints a day. Barati notices another case where 3674 pints were evacuated in ninety-four days, being at the rate of thirty-nine pints a day for the whole term; and Fonseca knew an instance where 200 pints were evacuated daily, but the time is not mentioned.

The following tables, drawn up by Mr. William Alexander, are to a certain extent useful, in shewing the effect of different diuretics:—

A TABLE *of the different quantities of urine always discharged in an equal time, viz. from nine o'clock in the morning till two o'clock in the afternoon; when an equal quantity of the same liquid was drunk, but with different diuretics, in different quantities, dissolved in it.*

	℥	ʒ	℈
By lbj. ʒvijss. simple infusion of Bohea tea, standard	15	4	0
By do. with ʒij. of salt of tartar	22	7	2
By do. .. ʒij. of Nitre	22	0	0
By do. .. 4 drops of Oil of Juniper	30	3	0
By do. .. ʒj. Salt of Wormwood	19	7	1½
By do. .. ʒij. Castile soap	19	1	1
By do. .. a tea-spoonful of Spt. Nitr. dulc.	16	6	1½
By do. .. 15 drops of Tinct. Cantharides	16	4	0
By do. .. ʒij. of Sal. Polychrest	16	3	0
By do. .. ʒss. of Uva Ursi	16	1	0½
By do. .. ʒj. of Magnesia Alba	15	5	0
By do. .. ʒij. of Cream of Tartar	10	2	0½

A TABLE *of the different quantities of urine evacuated in the same space of time, after drinking the same quantity of different liquors.*

	℥	ʒ	℈
By lbj. ℥vijss. of weak Punch, with Acid	21	2	3
By do. .. new Cow Whey	18	6	0
By do. .. decoct. Diuret. Pharm. Edin.	17	5	0
By do. .. London Porter	16	7	0
By do. .. decoct. Bardan. Pharm. Edin.	14	7	0
By do. .. warm Water Gruel	14	6	2
By do. .. Small Beer	13	7	1
By do. .. warm New Milk	11	7	0

You must, however, recollect that diuretics are relative agents, and their effects are not necessarily the same in the healthy and diseased conditions of the body.

In order to promote diuresis, diluents should be given, and the skin kept cool. If the latter caution be not attended to, the substances exhibited as diuretics frequently become sudorifics. Going into the cold air out of a warm room, or applying cold to the extremities, will oftentimes excite the action of the urinary organs, when all other means have failed: and in hot countries, where the heat provokes excessive sweating, the secretion of urine is very considerably diminished.

Class XIV. *Aphrodisiacs* — These are substances supposed to have the power of exciting the sexual feelings. Phosphorus and cantharides are usually regarded as such.

Class XV. *Sialogogues.* — Sialogogues, or those medicines that excite the salivary discharge, next deserve a few words of explanation. Some of them are *local;* that is, they produce this effect by application to the mouth; and when used in a soft or solid state are termed *masticatories:* of course they act equally on the mucous follicles of the mouth, and on the salivary glands. Some of these agents act mechanically, as when wax or mastic is chewed; others are powerful acrids; for example, ginger, horse-radish, the root of pellitory of Spain, &c. Certain substances, however, act as sialogogues, when applied to distant parts of the body; these are the *remote* sialogogues. Among these may be mentioned the preparations of mercury and of gold, fox-glove, hydrocyanic, arsenious, and nitric acids, nauseants, &c. All of them, however, are more or less inconstant, but the preparations of mercury the least so.

When we excite ptyalism by mercury, it is rather with the view of proving that the system is under its influence, than for any advantage supposed to be derived from an increased flow of saliva.

Class XVI. *Caustics.*—There are three classes of substances which are denominated caustics; namely, heat, or the *actual cautery;* secondly, *escharotics* or *corrosives*, or those that have a powerfully chemical action on the tissues to which they are applied, (as the potassa fusa); and thirdly, *catheretics*, or those that influence the living properties of the part, but have little chemical action, (as the sulphate of copper.) Some caustics have only a local action, others are capable of affecting remote parts. The mineral acids, the alkalies, nitrate of silver, muriate of antimony, and burnt alum, are examples of the first, and the peroxide of mercury and white arsenic of the second.

Class XVII. *Inflammantia.*—Substances capable of exciting inflammation in the parts to which they are applied. When employed so as to excite redness of the skin merely, they are called *Rubefacients.* The milder ones, such as frictions and warm pediluvia, excite the skin temporarily, while the stronger ones, (mustard, for example), irritate and inflame. Where a large ex-

tent of the cutaneous surface has been reddened by one of this class, a febrile condition of the body is set up, as is shewn by the acceleration of the pulse, and increased temperature of the body. Some inflammants occasion vesication, or the exhalation of a thin serous fluid, by which the cuticle is detached from the true skin: these are called *vesicants* or *epispastics.* When a large portion of the skin is separated, we call it a bleb, bulla, or ampulla: but when the detached portion is small and orbicular, a vesicle. Sometimes the inflammantia produce a secretion of pus; they are then called *suppurants.*

Class XVIII. *Acids.*—Acids are used in medicine for a variety of purposes, but I shall notice them here principally as chemical agents acidifying the fluids of the body. When from any cause, such as disordered digestion, particular kinds of foods, or other circumstances, white sand (either phosphate of lime, or phosphate of ammonia and magnesia) appears in the urine, the internal use of acids will frequently diminish or remove it. We likewise resort to the use of acids to neutralize the alkalies, when they have been accidentally or purposely swallowed.

Class XIX. *Alkalies.*—These agents are used to alkalize the contents either of the stomach or the urine. When employed to neutralize any acid which may be found in the stomach, they are termed antacids or absorbents. If the urine contains uric acid, as shewn by the deposition of this substance in the form of red sand or gravel, the alkalies are exceedingly useful in counteracting it.

[It may be proper to state, that this lecture is an abstract of five discourses given in the theatre.]

DR. ELLIOTSON ON LIFE AND MIND*.

To the Editor of the Medical Gazette.

Sir,

In perusing, several years ago, the fourth edition of Dr. Elliotson's translation of Blumenbach's Physiology, I amused myself with making a few notes in the margins of the pages, on some of the accomplished translator's opinions on Life and Mind—opinions which appeared to me to be unworthy of their author, and calculated to bewilder and mislead his readers, particularly the juvenile portion. As part the first of a new treatise on Physiology, by Dr. Elliotson, has just appeared, not so much a translation of Blumenbach as an original work, I was induced, for the purpose of comparison, again to peruse both my own notes and the chapter on life and mind, as it stands in this, the fifth edition; not altogether without the expectation, I confess, of finding the work purged of certain speculative views, which, to say the least of them, are but little fitted for an elementary treatise. In this expectation I was disappointed. The same opinions are stated afresh, with, if possible, greater confidence than before, and illustrated from a variety of sources—from the writings of Gall, from those of several eminent divines, not omitting appeals to the sacred scriptures.

I am far from intending to insinuate that there is any novelty in the opinions referred to. Life and mind have always supplied *quæstiones vexatæ.* They have afforded matter of controversy almost ever since men were accustomed to differ, which is equal to saying, ever since they began to speculate. Very probably such disputes might long ago have been set at rest, had philosophers submitted to confine their attention to the phenomena of animal being, discoverable by consciousness and by observation, and reasoned from these with the same caution they have exhibited in reasoning on most other subjects. But prejudices, chiefly connected with theology, have interfered; and wonderful has been the confusion of thought and language displayed in this above all other controversies, as the reader will readily admit who has the patience to peruse, among a long catalogue of treatises that might be named, the singular work of Barclay on Life and Organization; or the far more interesting, though prolix, chapters on the same subject, in the Memoir of Cullen, by Dr. Thomson.

In his preface the author has intimated that the correction of any errors, and the communication of any facts, either publicly or privately, will be esteemed by him a valuable favour. It is in the same fair and candid spirit, I trust, in which this invitation is given, that I accept it; with the intention, nevertheless, of freely canvassing some of his views, which I cannot help regarding as altogether illogical, and well worthy of refutation for the student's sake, who is but too generally an easy convert to whatever wears the semblance of novelty, provided it is presented with a bold assurance, which dissipates at once all difficulties and doubts.

Much that Dr. Elliotson has written on

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* Human Physiology, by Dr. Elliotson. Part First. London, 1835.

This letter, and the one which is to follow, were partly prepared for the press last spring, soon after the work appeared; but circumstances, which it is needless to explain, prevented their being earlier forwarded for publication.

life is judicious and philosophical. "We see matter," he observes, "in a certain state, possessed of a certain power which we term life; and the object of physiology is merely to observe its effects, just as it is the object of chemistry to observe the circumstances of the affinity of different bodies, and of physics to observe other phenomena of matter, without vainly speculating on the essence of matter, or the essence of affinity, to comprehend which our faculties are in their nature incompetent*." After sentiments so sound and so well expressed as these, it excites surprise to be told that "the Almighty has endowed organic matter with the superaddition of life†." And the surprise is not lessened when we further read, that "a power or property of matter [*e. g.* life or attraction] cannot be matter‡." The tenableness of the latter opinion I shall have occasion by and by to canvass; but the former, that life is a superaddition to matter, deserves a brief examination, especially as the same view, with respect to the superaddition of certain other qualities to matter, is expressed in another passage in the following terms:—

"Substances consist of particles endowed with certain properties, without which their existence cannot be conceived—viz. extension and impenetrability, with others which proceed, indeed, from their existence, but are capable of being subdued by opposing energies, namely, mobility, inertness, and with others apparently neither necessary to their existence, nor flowing from it, but merely superadded; for example, various attractions and repulsions, and various powers of affecting animated systems§."

This account of matter is far from being unexceptionable, for several reasons, but I shall refer to one only. It is no doubt allowable to say, with reference to extension and impenetrability, that, constituted as our senses are, these qualities necessarily enter into our conception of matter, as something extended and capable of resisting compression; and with regard to certain other qualities of matter, that they are unnecessary to this abstract conception of it. But instead of this guarded view, the writer expresses himself in such a manner as to imply that there are certain qualities necessary to the existence of matter, and certain other qualities which neither flow from, nor are necessary to, that existence, but merely superadded; which seems to me to be altogether erroneous; and, moreover, as we shall see, is the source of confusion in some of his subsequent reasoning. It has been customary for metaphysicians to divide the qualities of matter into two classes, primary and secondary, in order to distinguish such as matter in every form exhibits, and which consequently are essential to *a definition* of it, from such as it may or may not exhibit, according to specific differences existing between different substances, and according as any given substance undergoes changes in its chemical or its mechanical states. But why should it be thought that the one class of qualities is essential to the existence of the particles of substances, and the other not? Why are extension and impenetrability qualities more essential to the existence of the particles of the substance named gold, for example, than yellowness, which, according to Dr. Elliotson, is a superadded quality? Matter, in every condition it is possible to conceive of it, we are never to forget is only known to us by certain qualities, and that by a quality of matter, whether it be a primary or a secondary quality, we mean *that* which is the cause of a particular sensation in us percipient beings, and mean nothing more. Extension and impenetrability, therefore, are nothing but matter *in certain relations to the mind*, precisely as mobility, attraction, inertness, colour, viscidity, fluidity, odour, sweetness, are nothing but matter in certain other relations to the mind. The feelings of extension, colour, sweetness, &c., it is true, are not matter, but feelings excited by matter, accompanied *necessarily* by the belief, however, that matter when it excites such feelings is extended, coloured, sweet. Hence it is clear that yellowness—that quality of the particles of the substance gold which excites in me a feeling of yellowness—is not something superadded to the gold. On the contrary, it is doubtless as essential to the existence of the gold, in the circumstances in which this quality is perceived by me, as extension and impenetrability are to the existence of matter in general. To assert that yellowness is not necessary to the existence of the gold, in the circumstances in which this quality is perceived by me, is to assert what is utterly unmeaning; for what, I ask, is the yellowness of the gold, but the gold itself in a certain relation to the percipient mind? just as the quality of impenetrability, likewise possessed by the gold, is nothing else than the gold in a certain different relation to the same percipient mind*.

To apply these observations to the phe-

* Page 30. ‡ Page 39.
† Page 41. § Page 2.

* It is scarcely needful to remind the young student that "matter in the abstract," or "matter in general," is nothing in nature, but is merely a logical figment. Matter which is known to us, that is to say terrestrial matter, is gold, silver, sulphur, phosphorus, or one of the other uncompounded bodies; or a compound of two or more of these.

nomena of life:—"The Almighty," says the writer, "has endowed organic matter with the superaddition of life." Here, then, is a further addition to organic matter, besides the qualities previously superadded before it was organized: in other words, living organic matter possesses, first, qualities necessary to its existence as matter; secondly, qualities not necessary, but superadded; and, thirdly, vital qualities superadded to both! This statement, in my opinion, is perfectly erroneous. It is enough if we say that what are called the phenomena of life are qualities of matter in certain conditions, necessarily arising, so far as we know, from such conditions. Life, indeed, is not the name of a quality, properly speaking, but a generic term inclusive of a great number of qualities, which matter, as it exists in animals and vegetables, impresses on the percipient mind; these vital qualities, to vary the mode of expression, being *nothing else* than matter, as it exists in such bodies, perceived by the mind through the instrumentality of the senses. So far from having reason to regard the phenomena of life as qualities superadded to organic matter, we have no reason whatever to infer that matter could exist, in the *identical state* in which it exists as a plant or an animal, without exhibiting the identical vital phenomena it does exhibit, any more than we have to infer that oxygen could exist as oxygen, without possessing its present properties; or matter in general exist without possessing extension. Were I to be shown a mass of iron, or of granite, exhibiting vital phenomena, I should be ready to admit that life was, in this instance, superadded, knowing that iron and granite do not commonly exhibit such qualities; but when I see vital properties always belonging to matter, in forms, as regards their composition, perfectly unique, and which art cannot imitate, I infer that they are the properties of matter in such an inimitably compounded condition; and the conclusion is warranted by the fairest analogy, it being found that changes in the composition and mechanical condition of matter, are attended by corresponding changes of properties. A lump of gypsum, for example, possesses certain properties. We separate its constituents, and a variety of new properties are immediately exhibited. We reunite the substances; the new properties vanish, and the old properties return. Here it is surely reasonable to infer that the changes, in regard to properties, resulted from changes in the conditions of the matter. If it should be said that the conclusion is a precarious one, because there is little or no analogy between chemical and vital properties, I reply that as little analogy is there between the condition of a chemical salt and that of a seed or an ovum. If the two classes of properties are immensely different, no less wonderfully different are the two kinds of bodies as to the nature of their composition. I wave, however, all further remarks on Dr. Elliotson's opinions concerning life, as my own views on that subject are to be found, in a very brief form, in an early volume of the Medical Gazette, in refutation of an hypothesis by Dr. Prout*, and in what remains I shall confine myself to the author's sentiments on mind.

"Mind," says Dr. Elliotson, "is the functional power of the living brain. As I cannot conceive life, any more than the power of attraction, unless possessed by matter, so I cannot conceive mind unless possessed by a brain, or by some nervous organ, whatever name we may choose to give it, endowed with life. I speak of terrestrial, or animal mind; with angelic and divine nature we have nothing to do; and of them we know, in the same respects, nothing†." Again, "A physical inquirer has to do with only what he observes: he finds this power, but attempts not to explain it. He simply says the living brain has this power, medullary matter though it be. Seeing that the brain thinks, and feels, and wills, as *clearly* as that the liver has the power of producing bile, and does produce it, and a salt the power of producing a certain form, and does crystallize, he leaves others at liberty to fancy an hypothesis of its being a subtle, immaterial, immortal substance‡."

These passages sufficiently convey the writer's opinion, that it is the brain which feels, thinks, and wills; or, in other words, that what are called the qualities of the mind, are, properly speaking, qualities of the brain,—the brain and the mind, so far as these phenomena are concerned, being identical. The arguments by which he strives to support this view will be examined in a future letter. Were it, however, a *sine qua non*, dissenting as I do from Dr. E., that I must of necessity maintain by argument that the mind is immortal—the shift, in part, which he assumes an opponent must be forced upon—I should decline the discussion. Whether I hold the mind to be an immortal substance, or the contrary, has nothing to do with the proposition in question, which is limited to this, that *the brain thinks, as clearly as that the liver secretes bile*. It will appear, I trust, in what I shall say, that I am at liberty to steer clear of all hypotheses whatever relative to the mind's destiny, and that I am

* Lond. Med. Gaz. vol. viii. p. 144.
† Part I. page 32.
‡ Ibid. I. page 39.

in no instance obliged to advance beyond what I actually know from simple consciousness, and consciousness aided by the senses.

When a person is led to reflect on the knowledge he possesses,—suppose of mind, of a chemical salt, of the medullary substance brain, of the bile, or of other objects,—and sets himself to discover how he obtains his knowledge, he finds that the inquiry is exceedingly simple. He speedily dicovers that, leaving testimony out of the account, his knowledge of mind is derived from consciousness, and that from no other source does he obtain his knowledge of it. He is conscious that he exists—that he has feeling, reason, and volition — and that these faculties, which, by turning the mind to contemplate its own operations, he is able to subject to rigid examination, belong to *that*, whatever it is, which he is accustomed to denominate *I*. Further, by a law of his nature, he is conscious that his hand, his foot, the other parts of his body, and all objects which impress his senses, are distinct from this same entity. He never confounds this conscious being with any thing else whatever, and intuitively feels that it involves an absurdity to suppose that it could be added to, or subtracted from, or divided. In a crowd, in solitude, at all times, and however occupied, he never doubts of his own identity —that he is in every respect the same being to-day that he was yesterday. The properties of mind he learns by consciousness, and by a law of his nature is compelled to ascribe them to *something* of which they are the properties. The properties of matter he obtains a knowledge of also by consciousness, through the aid of the senses, and intuitively refers them to *something else*, of which they are the properties. Concerning matter equally as concerning mind, all his knowledge depends on consciousness, in the more extended sense of that word. And yet, as has been stated, by a law of his being, he never confounds perceptions of mental properties—volition, reason, memory, fear, hope, joy—with perceptions arising from impressions made on the senses—extension, hardness, fluidity, bitterness; in other words, he never confounds the properties of mind with those of matter, nor ceases to feel, whenever he reflects on the subject, that matter in all its forms is one thing, and mind, in all its infinitely diversified states, is another and entirely different thing.

These are first principles (if I, indeed, have succeeded in stating them correctly), which it is not optional to admit or to reject. If they are rejected, every proposition which it is possible to present for the assent of a rational being may be rejected. On the assumption, then, that they are indubitably true, I proceed to the examination of this notable proposition,—that it is as clear that the brain thinks, as that it is the liver which secretes bile. In regard to the liver, I become acquainted with it by the help of my senses; as also, by the same means, with the fact that the bile is formed in the liver, and issues from its duct. No sane person will differ from me on this point: he will at once assent that it is the liver which produces the bile. In like manner, were I to subject a liquefied salt to the action of heat, he would agree with me, on witnessing the formation of crystals, that the saline particles have the power of taking a certain definite crystalline form. In these instances I should need to assume nothing; what I asserted would be granted without a moment's hesitation. But Dr. E. asserts that it is not more clear that the liver produces the bile, than that it is the brain which feels, thinks, and wills. Will this, I ask, be as readily conceded as true? Manifestly not. *I*, for one, do not acknowledge that there is sufficient resemblance between mind and bile—between secretion and thinking—to warrant the comparison at all; or that the knowledge we possess of the one class of phenomena can be employed for the elucidation, in the way of analogy, of the other class; and that *I* am altogether *singular* in thus dissenting from this opinion of the writer, no one will assert. The bile is obviously matter, the crystal is matter, and both are confessedly products of matter; in short, matter in fresh forms. But thought, volition, anger, joy—the alleged products of the brain—are not *obviously* matter in fresh forms. Has Dr. Elliotson touched a volition? Has he tasted or smelt an emotion? When has he weighed or measured a thought, confined it in a jar, reflected or refracted it, or made it inhere in some body, so as to demonstrate its presence and its nature? Has he examined its qualities by a chemical test? If he has done none of these things, it is evident that mind does not possess the properties of matter, according to any known definition of that substance. And if it cannot be shewn that mind possesses so much as one quality either of ponderable or imponderable matter, how can it be with fairness affirmed that mind is a product of the brain, as clearly as that bile is a product of the liver? It may be suggested, further, whether it at all diminishes the difficulty, to assert, with Dr. E., that mind is *a property* of the living brain, since a property of matter, be it living or brute matter, is nothing different, and *cannot be conceived as any thing different*, from matter.

That Dr. Elliotson has fallen into some confusion of thought, appears from this

avowal—that as he cannot conceive life more than the power of attraction, unless possessed by matter, no more can he conceive mind, unless possessed by a brain; which is tantamount to saying that he sees no more difficulty in conceiving the brain to exhibit the phenomena of mind, than in conceiving matter to exhibit the phenomena of life and attraction. But here he falls into the error of comparing mere terms—life and attraction being nothing else than terms which express certain phenomena of matter—with mind, which it is a gratuitous assumption on his part to represent as a term expressive of phenomena of matter. The comparison, therefore, cannot be allowed.

Another proof of confusion of thought appears in the singular assertion, that he cannot *conceive* mind unless possessed by a brain. For my part, I could conceive mind, were I entirely ignorant of animal anatomy. A brain enters in no respect into my conception of mind. The intimate and wonderful connexion that subsists in animals between a brain and mind, is a fact in physical science which might have remained unknown as late in the progress of discovery as the circulation of the blood, or even to the present hour, without, perhaps, materially retarding the progress of mental science. That the idea of a brain, then, is *necessary*, in order to conceive of mind, is not to be admitted. The idea of a brain does not enter, I venture to assert, into Dr. E.'s conception of mind. His meaning must be, that he has no knowledge of the existence of any terrestrial mind, *his own conscious mind excepted*, except as exhibited in a living animal; in other words, that he knows nothing of *disembodied* mind. And no wonder, unless it were thought reasonable that minds should be visible, and might be subjected to examination, as the naturalist examines his specimens of moths and plants.

It is true we have irresistible evidence for the existence of other minds besides our own. Every person intuitively assents to the irresistible nature of the evidence for this fact. What I consciously experience in myself, however, with respect to the nature and power of my own mental faculties, is to me the *sole basis* of this evidence. I reason, and I perform a variety of operations, using means to an end. In the mechanism and actions of my own body, and in all nature around me, I behold proofs of agency and design, although the agent I discover not: nevertheless, when once I have detected, not contrivance alone, but wonderful skill, employed in effecting an infinite variety of designs, I intuitively infer the existence of an intelligent mind, the author or cause of all such effects. This is an intuitive, irresistible inference. I have no conscious perception of the supreme mind, nor of inferior minds, distinct from my own. I infer the existence of the Deity from his works, in the same manner precisely as I infer the existence of a fellow mind; it being in the nature of things impossible, without miraculous aid, that I should form even the faintest conception of the existence and attributes of other minds, except by the evidence of such operations on matter as I am conscious that my own mind is, in some degree, capable of imitating or producing: which is merely a different mode of affirming that the existence of no mind can become known to me unless it is possessed of faculties in some sense resembling those of my own. Doubtless the evidence for the existence of a fellow-mind is more clearly and immediately perceived than that for the existence of the Deity, because the evidence for the former is of easier observation, and lies within an infinitely smaller compass. I behold a form and organs, like my own, moving and acting with intelligence, as I myself move and act; and to this form I intuitively ascribe the possession of a mind with like faculties and affections. In this *inferential* recognition of a fellow-mind, what need is there I should recollect, or even know, that there is a brain and nerves, by which it in some manner operates, more than that there is, in the same body, the pancreas and the pineal gland?

A question sometimes started by those who regard the brain and the mind as identical, is this: if it be not the brain's office to think, of what use is so large an organ in the animal economy? The brain, it may be said in reply, probably, indeed certainly, serves a *variety* of uses in the animal economy *. But the question is a mere trap, and there is no need whatever to be entangled in it; for they who assert that the brain is the mind, are bound to prove what they affirm, and not to throw the burden of proving what are the uses of the brain on their opponents. In reflecting on what this question imports, the student is especially bound to recollect and consider well what mind is—that it is an existence having no quality in common with anything in the material world; that it is by mind alone he investigates the structure and the functions of the brain—the nature of the mind itself, the nature of whatever besides he attends to; and that without mind he could know nothing—could not even so much as doubt whether or not he possesses a mind! Should he disregard this preliminary train of reflection, he will in all probability feel the

* See the Gulstonian Lectures and the other Physiological Works of Dr. A. P. W. Philip.

question to be very perplexing, since he will be ready to regard the mind in the same light as the materialist—as being not that most wonderful thing that thinks, but something that is analogous to the parts and products of the body,—a mistake which will leave him deservedly in the power of his assailant. Unhappily, the ability to turn the mind inwards, to study its own qualities, is apt to be in a great measure lost, or, perhaps, never acquired, for want of cultivating, in a moderate degree, the habit of abstraction; and thus the power of accurate discriminative thought is greatly impaired, or altogether wanting. If the student possess and have diligently exercised the power of reflecting on his own mental operations, he will perceive that *primâ facie* he ought not to admit that the brain is the mind, without the clearest and most direct evidence; for this weighty reason—that he knew a great deal about the mind, had been intimately acquainted with its powers and properties, when as yet he knew little or nothing respecting the brain. While, therefore, he recognizes the *uses* of the brain and nerves, as a legitimate and interesting object of physiological inquiry, he clearly perceives that, merely for the purpose of extricating himself from the dilemma his opponent is attempting to force him into, it is one he is no ways particularly concerned to pursue.

Were it my present object to discuss what are the uses of the brain in the animal body, I should have no objection, with Dr. Elliotson, to allowing that the brain, besides other purposes which it subserves, is the organ of the mind; provided we agreed to attach to the term *organ*, in this relation, a modified meaning. This proviso would be necessary; for if we say that the brain is the organ of mind, and the lachrymal gland is the organ of tears, attaching the same meaning to the term organ in these different connexions, we fall into palpable error; and why?—because while it is matter of fact—a thing not admitting of dispute—that the use or function of the lachrymal gland is to separate from the blood the tears, we are not warranted in affirming of the brain that its indisputable use or function is to separate from the blood, mind—to produce, in any way, mind from matter, or even at all to *produce* or *originate* mind: consequently the brain is the organ of the mind in a manner, and *in a sense*, totally different from that in which the lachrymal gland is the organ of the tears.

Let us briefly inquire (ought I not rather to say conjecture?) in what respects the brain and nerves are to be regarded as instrumental in the operations of the mind. The nerves appear to be instrumental in sensation, in the widest sense of that term; visible objects the mind perceives by the instrumentality of the optic nerves, odours by the olfactory nerves, sounds and sapid bodies by the nerves appropriated to the senses of hearing and taste; and touch, heat, cold, muscular conditions, a great variety of sensations in the stomach and alimentary canal, and in the other systems of organs in sickness and in health, by means of nerves and of certain prolongations of the spinal marrow, extending within the cranium. Unquestionably the phenomena of sensation in the animal kingdom, with perhaps a few exceptions, is co-extensive with the endowment of nerves; and defect in the sentient power is found to accompany a morbid state of the nervous structure. It is likewise no less true that volition, in producing the infinite variety of muscular motion, acts by certain portions of the spinal marrow and certain nerves which exist for this special office. The brain, indeed, we are taught to believe, according to the most recent discoveries (if, indeed, the deductions of vivisectors are, in the present state of our knowledge, really entitled to the name of discoveries), is necessary "not so much that the mind may feel impressions, as that they may be remembered and availed of for useful purposes; not so much that volitions may act as stimuli on muscles, as that acts of volition may be so excited as to produce regular and voluntary actions under the guidance of desires, and of judgment and experience, as distinguished from blind instinct *."

Thus the brain, it should seem, is the centre where, by the instrumentality of nerves of sense and of portions of the spinal cord, the mind receives all manner of impressions of bodily conditions, and of objects without; as likewise the centre whence, by the instrumentality of a different set of nervous prolongations, it moves the muscles in using the hands, in walking, standing, running, leaping; in the motions of the eyes, in eating, in speaking, and in thousands of minor acts which are continually being performed, and are too numerous even to be recounted.

Seeing, then, the mind obtains its knowledge of facts relative to the external world, by the instrumentality of the nervous substance, is there not reason to infer that in *conception, memory, recollection*—*i. e.* in retaining present perceptions and in recalling such as are past—*the mind employs the same kind of nervous instrumentality as that by which it in the first instance obtains them?* A large number of facts seem to warrant an affirmative answer; for it is well known that confused perception, and

* See Dr. Allison's View of the Progress of Medical Science in the present Century.

partial as well as complete losses of memory, are frequent consequences of diseases affecting the brain; as likewise, in some degree, of that induration of the brain which occurs in extreme old age. I remember conversing with a gentleman, who, having had a severe attack of fever five or six years before, still remained in entire oblivion of all the events of his life previous to his illness. He was a widower at the period of the seizure, but he had no recollection of his wife. In one instance only did he imagine that he obtained a glance of the past. On paying a visit to the house where he resided before and at the time the fever took him, he entered one of the apartments, and instantly felt a persuasion that he had been there before. Whether or not perfect recollection has in this instance returned, I am unable to say. Some persons, again, after an illness or an accident affecting the brain, have forgotten, for a time, the language they were in the habit of speaking, and commenced speaking a language they formerly learned, but had apparently ceased to remember. Others have forgotten the names of certain objects, or the names of certain classes of objects. The variety of affections of this nature is, indeed, very great. The student will find an account of them, written with much discrimination, in Abercrombie's Inquiries concerning the Intellectual Powers; a work chiefly designed for students of medicine, and worthy of their most attentive study. In old age, when the brain loses that softness which is probably the necessary condition of fitness for the perfect exercise of memory; perception and memory, and, as a consequence, attention, are the faculties which become defective; the mind acquiring with great difficulty fresh knowledge, and, with no less difficulty, recalling the old. Hence ensues inactivity of mind, which being accompanied by a feeling of diminished muscular vigour, of general infirmity, of diminished power to impart or to receive pleasure, ends but too often in the melancholy spectacle of habitual discontent; the judgment and certain classes of the emotions still remaining *unimpaired*. Were this line of inquiry to be pursued as far as it might, perhaps, fairly be, it would, I think, be made to appear that delirium and coma, and a large proportion of mental diseases, admit of being traced to modifications of disordered perception and memory; and, as a consequence, to some species of morbid affection of the nervous substance instrumental in the production of these mental states.

Admitting that this view of the instrumentality of the nervous substance is worthy of regard (as I apprehend it is), if we reflect how incessantly the mind is receiving an infinity of impressions from the world around, by means of the senses alone, and new ideas by reflection, aided by the senses—ideas also from testimony in reading and in conversing, and ideas in reverie and dreaming—the number of orders it is ceaselessly sending through the nerves to the motive organs; and the almost as ceaseless demands it is making on the brain for the reproduction of perceptions, in musing, in deliberate reflection, in public speaking, in conversation, in the different kinds of literary composition, in the transaction of business, and in practising the useful and elegant arts—perceptions which are numberless as the sands on the sea shore—we discover that the brain is exceedingly well employed, even were it a larger organ than it is; in fact, that if the brain perform merely the work which has now been assigned to it, it must be allowed to be, beyond comparison, the busiest and best employed organ in the body, without its needing to have imposed upon it the additional exercises of volition, judgment, imagination, emotion; none of which, as mental states, are, so far as is known, manifested by the instrumentality of organic processes *.

Should it, however, ultimately appear to be established on the basis of physiological experiment, that every manifestation of mind, without exception, is accompanied by an organic process, this would be insufficient to secure general assent to the author's proposition, that it is the brain which thinks. An intuitive truth does not lie at the mercy of an experiment. At the present time it is a fact universally admitted that the mental state we call *sensation*, is, in every instance, accompanied by an organic process; and yet few, I presume, bring themselves to believe that it is the nervous organ that feels. Who has ever brought himself really to believe, because the optic nerve is the instrument in seeing, that it is the optic nerve which sees? The impossibility of believing (to say nothing of *proving*) that the mind, which feels and recollects by the instrumentality of the nervous substance, is itself the same identical nervous substance, admits of easy and convincing illustration. I will mention one example. At night, after long-continued and extreme fatigue, I find it to be my duty to examine and rectify an entangled account which has relation to a great number of intricate, extensive transactions, of different dates. On making the attempt, I find myself un-

* It affords me much pleasure to refer the reader to Prichard on the Vital Principle—a work containing many views, however, which I cannot assent to—for much lucid discussion on the instrumentality of the nervous system in the operations of the mind.

equal to the task: the power of attention is wanting. I endeavour to recal a variety of perceptions which are necessary in the commencement of my labour, but in vain; my thoughts do not flow in the usual trains, and every effort at recollection recals only confused masses of perceptions, which disperse, and are succeeded by others equally confused. The duty is not imperative; I might, if I chose, postpone it till another time, but I say to myself, "No; I am determined the account shall be rectified before I sleep; I will not give way to lassitude." I hereupon rise, walk briskly about, and *resolve* that I will not be foiled. What happens? I conquer. The mind regains the mastery. It compels the wearied brain and other organs to obey, and once more exercises its powers in conscious triumph. Would it be rational to say, in this instance, that the mind, which triumphs over the lassitude of the nervous organ enforcing obedience, is merely that organ triumphing over itself? —the living brain compelling the obedience of the same living brain?

In conclusion, then, to assert that it is *obviously* the brain which thinks, is unphilosophical; because it is to trifle with the nature of evidence, the assertion being incapable of proof by any known method of observation and reasoning. Perhaps it may be said, that if it cannot be actually proved that it is the brain which thinks, it may be shewn to be probable. To this notion it is sufficient to reply, that it will require a wonderful amount of proof, and that the proofs be marshalled in some perfectly novel manner, to render it even in the lowest degree probable; because the fact assumed is, as we have seen, altogether repugnant to the natural principles of belief. Before I can admit, therefore, that the living brain is the mind, I must be prepared to believe that what I, and every one else, intuitively reject, may notwithstanding be true: in a word, that the fundamental laws of human belief are of less authority than a disputed inference in the unsettled and, as yet, unsatisfactory science of animal physiology.

But a thousand facts demonstrate, as I have fully admitted, the intimate connexion which subsists between the mind and the brain; the dependence of the mind, in order to the exercise of its faculties, on the integrity, in general, of that organ; and that certain conditions of the nervous system, springing from age, race, education, and the like, are attended by corresponding modifications of mind. These, and other arguments of Dr. Elliotson, in favour of what I presume to call his hypothesis of the mind's materiality, remain for examination in a future letter: not that I admit that any such hypothesis is wanted, or that it is other than gratuitous. It can serve no scientific end; it maintains an unavailing struggle with the unalterable, because intuitive, laws of human belief. Nevertheless, as the hypothesis in question admits of being stated and recommended with some show of plausibility, it is on that account entitled to examination.

Yours respectfully,
JOHN ROBERTON.

Manchester, Nov. 7, 1835.

PRACTICAL SUGGESTIONS

INVITED FOR THE

IMPROVEMENT OF THE NEXT CENSUS.

To the Editor of the Medical Gazette.

SIR,

As you have deemed some of the results of our English population inquiries to be proper for admission into your pages, you may not, perhaps, see any objection to the insertion of observations defensive, at least explanatory, of the process hitherto pursued, and thereby leading the attention of the public towards the suggestion of improvements in the year 1841, or rather in the Act of Parliament in the preceding year, under which the census will be repeated. For although the public, and parish authorities especially, have shewn themselves favourable beyond expectation to these inquiries, and the thinking portion of mankind have not been dissatisfied with the manner in which the results have been exhibited for ready use, they cannot but labour under some disadvantages in speculating on further improvements, because they cannot combine the knowledge of negative as well as of positive arguments; or, in one word, they have not the benefit of experience as a basis for projected improvements; and if I am able to supply that defect in any reasonable degree, I shall not have written this letter in vain.

The first element of the inquiry, the enumeration of the people, has not obtained so much credit as justly belongs to it,—and this, because the mode of pursuing the inquiry appears too simple and vulgar for insuring precision,—because public functionaries are not formally appointed for enforcing attention to the task,—because a law, with little appearance of stringency, is therefore

supposed to be ineffectual. But as far as I have been a spectator of legislation, I should argue conversely, that the more apparatus (necessary or unnecessary) appears in any law, the less likely is it to arrive at a satisfactory result.

I do not suppose that any defect is suspected in the enumeration of the higher—the more conspicuous, classes of society; and those objectors who are not familiar with the operation of the English Poor Laws (which do not extend to Scotland or Ireland), cannot duly estimate the value of our "overseers of the poor," for the purpose of enumerating the inhabitants of every parish or subordinate district which maintains its own poor. In this, as in the case of members of parliament and other popular functionaries, the law of England usually assigns two persons of joint authority for executing the same duty, so that public business shall not be deranged by the illness or occasional absence of any single person. These overseers of the poor have their attention so often called to the condition, and especially to the number, of every poor man's family in their several districts, that in a small parish the overseer is almost able to enumerate the inhabitants without stirring from his own fireside, and in large or populous parishes, is not without assistants replete with the same kind of local knowledge, and who, for the moderate reward offered by the Population Act, are quite ready to perform the duty required by it. Under these circumstances, I request those who doubt of the accuracy of the English enumeration, to compare the probability of successful inquiry, by means of persons who are habitually compelled, by applications for relief, to be conversant in the affairs of every poor family,—with the probability of success by means of any other persons, however well rewarded, however zealous, but who must first sedulously inquire for every cottage and tenement in a rural district, every cellar and garret in a town, before they can visit every family, and enable themselves to answer the simple questions—how many houses, families, and persons, exist in a limited district?—the very limit, the boundary of which, is unknown to those whose office or employment does not compel them to be familiar with it.

I do not herein seek to derogate from the accuracy of the parochial schoolmasters in Scotland, who, for some reasons not dissimilar from the above, for other reasons different, but advantageous, arising from education, have produced a satisfactory return from the northern part of Great Britain.

I mean to confine myself to the supposed weak part of the population inquiry, and think I have shewn, that unless from intentional error or default, the overseers of the poor in England have always made a full and a true return, to the best of their knowledge and belief, as certified by them upon oath before the local magistracy. The chance of their doing otherwise must be estimated according to the strength of temptation operating upon their minds; and the absence of any such temptation ought to satisfy a candid inquirer that he may safely rely on the enumerations of the years 1801, 1811, 1821, and 1831. A shade of doubt, or rather a want of entire confidence, regarding the first of these enumerations, arose from its being an untried experiment, and from the effect of an Act of Parliament which a few years before had compelled parishes to supply men for the naval service. These doubts were not unreasonably urged, nor entirely withdrawn, until three more enumerations had been completed, when a comparative account of the four results in every parish was published in the beginning of the year 1832. This left little room to impugn the earliest enumeration, and the general consistency of the whole afforded satisfactory evidence to every experienced eye, that good faith had pervaded the entire returns. As to the possibility of official concealment or cookery in digesting the returns, it is negatived by the usual publication of them in the several county newspapers at the time; so that the official compiler could not hope to escape detection if he swerved from the truth in the return of any place, or in the totals of any county.

In the Population Act of 1801, the question as to occupation or employment applied to all persons. No such question (as far as I know), had been asked in any other nation; and the mistake of supposing every individual (females, children, and servants, not excepted), to be distinguishable according to occupation, rendered this question of no avail in 1801, as having been variously understood, or remaining un-

answered, from its obscurity. In the three subsequent enumerations, this question was made to apply to families instead of individuals; and in 1831, all males upwards of twenty years of age (amounting to one-fourth of the entire population), were questioned as to their occupations; which inquiry produced a more complete result than was anticipated.

In the year 1821, an important question was hazarded, as to the age of every individual; classing the ages by five years under the age of twenty, by ten years from thence to 100, and finally a class of centenarians. On this occasion it was to be considered whether peremptory legislation was advisable, or whether the object in view was not likely to be more accurately, as well as more easily attained, by adopting the voluntary principle in this personal investigation. For supposing nine-tenths, or a much less proportion (indifferently taken among the population) to make true answer to this inquiry, its object was answered, because by applying the numbers of the several classes to a radix of 10,000, especially if worked on every county, an answer (not the less satisfactory from being inferential) was given to the various questions of mortality, or of increase of population, which cannot be elucidated otherwise than by a classification of the ages of existing individuals. The question was therefore put as a concluding question, in the following words:—"If you are of opinion that in making the preceding inquiries (or at any time before returning this schedule), the ages of the several individuals can be obtained, in a manner satisfactory to yourself and not inconvenient to them, be pleased to state the number of those who are under five years of age, of those between five and ten years of age, &c., distinguishing males from females?" Long afterwards I met with this question copied into the work of a learned foreigner (M. D'Ivernois), who was much struck by this language of respectful request in a national inquiry, making inferences favourable to the civilization and good order of a nation which could be so gently governed by its legislature. On this occasion, the ages of eight-ninths of the population was obtained; the defect of one-ninth arising from entire parishes where the overseers declined the task.

We Englishmen owe thanks to M. D'Ivernois for his good opinion, which is not altogether unmerited; indeed, I may personally add, that in the many thousand applications which it has been my duty to make, soliciting returns, or the correction of returns, where from negligence or accident any defect has occurred, I have never received an uncivil answer; and the success of the parish register inquiry, as well as the enumeration, proves that finally due compliance was obtained. But, in truth, extorted, or even unwilling obedience, is in some cases worse than disobedience; and answers inserted at random by an overseer who might dislike the task of inquiring ages, would be manifestly of this character, as destroying the accuracy of any proportional radix worked on an otherwise accurate return.

I confess that, on more extensive reasoning and general argument, I much dislike such peremptory legislation as affects every individual in a community, and which, if not completely successful, perpetrates evil instead of conferring benefit; without which no alteration in the conventional arrangements of society is justifiable. To alter by force of law the language of a nation, or any part of a nation, has never yet been accomplished. Experiments of a milder character, such as the alteration of the calendar, or of weights and measures, were tried in the course of the French Revolution, with the advantage of absolute power on the side of the innovators. These laws had been preceded by that of the *maximum* (a regulation of the price of food), and many victims perished under the guillotine for disobedience to this dreadful law, which yet failed of effect. The same legislators attempted alteration in the calendar, whereby a seventh day of rest or enjoyment, was to be superseded by a tenth day; but this law only produced confusion until it was repealed, after ten years' trial.

The uniformity of weights and measures throughout France was highly desirable, and underwent a legislative experiment, whereby the multiples and aliquot parts of a *gramme*, or a *metre*, respectively, were to become universal in the daily transactions of every individual. But philosophy was dangerously mixed with this attempt to produce national convenience. The *gramme* and the *metre* were not recognized by the French public, who were not conciliated

by an attempt to introduce the Greek language into the common transactions of mercantile life*. Had the most usual pound weight, and the well-known French foot and toise, been generalized by law, a strong party would have favoured the innovation, and the new law might have become effectual and highly beneficial. An English law of the same kind, in the year 1826, created a new *bushel* under the old name, but incommensurable with the established corn bushel of the south of England and the metropolis, commonly called the *Winchester* bushel, which last, with such aid, might perhaps have been established as the standard bushel for all purposes. At present we only know that great expense has been incurred, and that all experience and knowledge of the corn trade (no unimportant branch of statistics), whether held in memory, or published in books, has been thrown into confusion, or superseded; and this law, after a vain effort to succeed by persuasion, seems destined to continue the experiment by force of penalties and forfeiture†.

Thus much in notice of laws which war against the habits and previous knowledge of a civilized community. What little of penalty or coercion appears in the Population Acts has not been found necessary in practice; and I must be permitted to estimate the accuracy of information thus obtained as highly as that produced by similar inquiries, which are supposed to be strictly enforced elsewhere. I know that an answer, true or untrue, may be exacted by a severe law; but I estimate the value of such answers inversely according to the threats employed and the penalties enforced.

Having now endeavoured to shield our English enumeration from random imputations of inaccuracy, which even in printed books are sometimes assumed as matter of course, undisputed and indisputable, I proceed to notice the only specific charge which has come to my knowledge impugning the enumeration of 1831, or rather that part of it which endeavoured to distinguish the various employments of the trading and handicraft portion of the community. In the Edinburgh Review of April last, objections are stated in a somewhat ludicrous manner against this attempted discrimination. It is impossible to be displeased at this, especially in a publication which heretofore has bestowed very useful attention on the subject of population and the law of mortality; and I only answer that precise accuracy in such a classification is known to be impracticable; but that a knowledge of the number of many of the classes cannot fail to be serviceable in statistical considerations, and finally, that most of the actions of mankind rely upon data much less accurate; nay, that if all imperfect knowledge had been withheld, or were now banished from the earth, we should find ourselves the most benighted race of animals on its surface; as scarcely any thing but arithmetic and the other branches of the mathematics would be left in our minds, and the experience of five or six thousand years in the common affairs of life must be slighted under such severe examination. Better it were that the reviewer, and others of his acknowledged talents, should bestow their attention on the improvement of the next census, as indeed he recommends, but does not exemplify by any practical detail.

He praises, in the census of 1831, and I venture to think justly, the three distinctions of those who cultivate land; adding, however, "and had those employed in taking the census been placed under efficient check or control, to make sure they did their duty, the answers might be depended on." But the census must be taken, at least commenced, on the same day; when about 30,000 persons (averaged at two in every parish or township) are simultaneously employed in it, making inquiry from house to house. Would he appoint another 30,000 persons, specially to superintend the proceedings of that day?—or would he appoint a less number, to pursue the same process during the next month, assigning several parishes to each of these controllers? The latter expedient is obviously preferable: but who is to appreciate and appoint 1000 persons fit for this task? Not the Secretary of State, because he does not possess universal local knowledge; and the appointment must emanate from the

* For common purposes the *metre* may be deemed to exceed the English yard by one-tenth part.

† The imperial bushel (so it is called) exceeds the ancient Winchester corn bushel by one thirty third more nearly than one thirty-second part; but the last is more convenient for memory, being two gallons, or one peck. The exact difference of the old and new corn bushel runs into a line of decimals, not retainable in memory.

magistracy of every district. Solicitation would immediately be in full activity, as these selected persons must be well paid; and after the whole nation had been thus disturbed, the fittest persons would not always be appointed. But to find every cottage in a rural parish, every separate family* in a town, requires local knowledge so exact and particular that the controller must be accompanied by an inhabitant in his censorship of the intended return of the censor first employed, and 15,000 other persons must be set in motion successively in the several parishes. Seldom would the second census exactly agree with the first; recourse must be had to some appeal for final decision, and a third census would usually ensue. But under this process the delay would be highly inconvenient, and the expense, which now perhaps averages at twenty shillings for each place, must amount to five times as much; for two persons must be employed in the second census, probably three in the third, besides that the controller must be a superior person, and paid accordingly; and 60,000*l.* or 70,000*l.* must be added to the 15,000*l.* now expended.

But supposing such increase of expense to be nothing compared with the value of precision, other objections of a weightier kind exist against such a judicial process. The overseers and churchwardens, who now perform their task with good will and alacrity, would be alarmed or disgusted at the foreknowledge that they were to incur formal responsibility—that their population inquiry was to be subjected to criticism and future question. "Is it not enough," they would say, or feel, "that we do our best, and, in submitting our returns to the magistracy, swear we have so done?—are we to be moreover liable to blame for accidental omission or mistake in a new duty, imposed on us for a purpose in which personally we feel little or no interest? We will not work on such conditions!" Thus the mainspring of the machinery, hitherto so successful, would be broken, and the population day, which now creates a sensation not beyond that of treading the parish boundary, would assume a severe character, and become unpopular among those who only can effectually execute the main intention of the population act. In reflecting on such difficulties, the reviewer would perhaps change his opinion, and grant that, in human affairs, the nearest practical approximation to truth is the best rule of investigation. He will even, I think, extend his charity to other parts of the population volumes of 1831; all which I think I could successfully justify in detail, though no more than one or two objections can be noticed in the limited space of a letter.

He says, for instance, that the area of an entire county is not made to correspond with that of its component parts. So say the Population volumes at the end of each county, and enable him to state the difference at 746 square miles in 57,812, or one part in seventy-seven; but as the public are not yet in possession of the entire ordinance survey (as he seems to suppose), this cannot be ascertained. For my part, I think I did good service in approximating (as far as materials were available) to the area of each parish; for it cannot be denied that the character of every place is indicated, in some degree, by the density of its population; and if the Poor Law Commissioners were asked the value of such approximation, added to the occupation columns, I venture to believe they would answer that their proceedings were much facilitated by reference to these particulars; and not the less because absolute precision cannot be ascribed to any part of the Population Inquiry. How much voluntary labour I bestowed in obtaining the best accessible knowledge of the area of every parish, the Population Preface may show (p. xxii.): the expense to the public was not less than 800*l.* additional, for materials and assistants in this particular branch of the inquiry.

I have ventured to say thus much as to the operation of enumerating the people, and the estimate of parochial areas, merely to shew that practical approximation ought not to be checked, much less superseded, until a detailed method of improvement is condescended upon (to use the Scottish phrase) by those who aspire at perfection; and I do assure all persons who may vouchsafe to write to me in that manner, that their proposals shall obtain respectful consideration, in so far as my influence may extend in framing a Population Act in 1840. They must

* How much does our language stand in need of the French word *ménage*, for population purposes!

bear in mind, however, that a national inquiry cannot be loaded with subordinate questions for private gratification.

The Edinburgh reviewer will, I am sure, forgive my criticisms, as being introductory to the above announcement; and I can sincerely avow my high opinion of the article herein mentioned, as containing an excellent epitome of British Statistical information, in which he goes far towards refuting his own opinion of its non-existence. Indeed, I cannot but think him too liberal, rather over-candid, in his depreciation of British statistics, as compared with similar knowledge on the continent of Europe. Permit me to imagine a French philosopher in the same liberal humour as the Edinburgh reviewer, and writing upon the same subject. "In France," he might say, "we have been miserably defective in the first element of statistics, in ascertaining our national population. In the time of Necker, we assumed a fictitious number, produced by an arbitrary multiple applied to the then imperfect register of burials. With the Revolution commenced a new æra, and the semblance of increasing population was founded on a higher multiple similarly applied. In fact, our population was never enumerated until Lucien Bonaparte became Minister of the Interior, under the First Consul. Before the termination of the war, another actual enumeration took place, but the result was not made public; and since the war, we have undergone two enumerations, the last of them nearly contemporaneous with the last British enumeration. But alas, so far are we in France behind all other nations in statistics, that the *sexes* have never yet been distinguished, and the relative number of males and females is entirely unknown. It is easy to understand why Bonaparte during the war did not exhibit the effect of his conscription law by such a disclosure; but why is this useful knowledge now withheld? In fact, we must confess that the genius of our government at all times tends to concealment, as much as that of our rivals in England tends to publicity."

Sweden has been honourably distinguished as the first nation attentive to inquiries as to the number of its actual population, and rate of mortality. The other northern nations, after some pause, have imitated Sweden, and at present, Prussia stands pre-eminent in such investigations. About fifty years since, Spain instituted an inquiry, which affords particulars of the then existing population far beyond example, even to the present time. Males, females, the married and unmarried, widowers and widows, the various grades of ecclesiastics and other classes, were carefully distinguished; but the misfortunes of Spain have thrown all this knowledge into oblivion, and very few copies of that Census are extant: but the splendid volume which contains the *Censo de la Poblacion de Espana* of 1797, institutes a comparison with the former Census; and therein preserves its most important results, as well as those of an earlier Census in 1796.

Some of the statistical inquirers on the continent, alluded to by the reviewer, are unquestionably meritorious and original; but they seem to be intermingled with as large a proportion of book-manufacturers as in England, whose confident statements must not be doubted, for certainly they cannot easily be confuted, because their authorities are not produced, when they venture upon any thing beyond avowed compilation from one or two original works.

In my next letter I propose to examine the details, and the probable amount of deficiency in English parish registers (which will become much more valuable if the ages of the enumerated population shall be ascertained a second time in the year 1841); and in adverting to parish registers, I shall again have to refer to the article in the Edinburgh Review, herein often mentioned; and much more to the article in that Review, published in March 1830; the best concentration of science and knowledge in population and the law of mortality which ever was presented to the public.

I remain, sir,
Your obedient servant,
JOHN RICKMAN.

November 10, 1835.

PARALYSIS IN CHILDHOOD.

FOUR REMARKABLE CASES OF SUDDENLY INDUCED PARALYSIS IN THE EXTREMITIES,

Occurring in Children, without any apparent Cerebral or Cerebro-Spinal Lesion.

By JOHN BADHAM, M.D. Worksop.

CASE I.—Ann Hare, aged 2 years, was brought to me by her mother, on the 14th of August, with paralysis of the

right leg. Her account of this seizure was as follows:—

The child had enjoyed uninterrupted health to the evening of her attack, with the exception (if, indeed, it can be so called) of slightly augmented thirst and some drowsiness, now remembered by the mother to have preceded the seizure by two days. On the evening of the 13th the child was put to bed, having run about and amused herself as usual during the day. On the following morning her mother's attention was first attracted, in dressing her, to an unusual appearance of the eyes, which, as she said, appeared to be turned inwards. A new cause of apprehension presented itself in putting the child on her feet, when it was found she could not stand. Medical advice being immediately sought for, it was thought sufficient, I understand, to keep the bowels open and to employ a warm-bath. No advantage, however, being procured under this treatment, she was brought to me on the 14th; a week after her attack. Her *appearance* at this time did not denote *any* disease—she was playing in her mother's lap; but on accurately examining the two limbs, it was found that *motion* in the right was completely destroyed, and in the left somewhat diminished; while *sensation*, perfect in the left limb, was impaired, without being suspended, in the other. At this period there was neither wasting of the limb nor diminution of temperature. Strabismus very decided; the pupil drawn to the inner canthus, and the eyeball fixed there, as I found in attempting to direct the vision outwards, and the pupil on the same side as the palsied limb was dilated. No disturbance of constitution could be traced from teething, or from torpor of the bowels; nor, on examining the spine attentively, could I detect any tenderness on pressure. I directed calomel in repeated doses, cold applications to the head, blisters to the spine, and cataplasms to the affected limb. Under this treatment the drowsiness was removed in five days. On the fourth, indeed, from its adoption, the ball of the right eye became suddenly liberated from its constrained position; the other eye recovered more slowly, a few days afterwards. The limb now shows (on the 15th of October) some return of sensibility, but the temperature is considerably below that of the other, and loss of substance has proceeded to a considerable extent. The exercise of the will over the affected extremity, though entirely abolished at first, has now partially returned, inasmuch as she no longer drags the limb after her, as she at first did, but projects or flings it forward with a jerk, the direction and force of which she seems not to have the slightest power to moderate or control.

Case II.—My second case singularly occurred a few days afterwards; its subject a little girl, also 2 years old, who had been seized, a week previously to my seeing her, with an equally sudden loss of the same extremity; which took place also during sleep, without any prior intimation. Thirst, remarked also in the former case, was a more prominent symptom in this; and the mother thinks there may have been somewhat more drowsiness about the child than common. The pupil of the left eye was dilated; the eyeballs free in their sockets; and, indeed, this unimpaired mobility constituted the only marked difference between the two cases. This case was chiefly treated with strychnine externally applied, but without any benefit; nor did stimulating embrocations and mercurials meet with better success. The perfect contractility of pupil was soon recovered. but the limb remains perfectly palsied and useless, though sensation has been somewhat restored.

Case III.—My attention has been a third time arrested by a case analogous in all its material points to the two just cited, except that the paralysis here is of the *upper* extremity. The patient, a girl, aged 2 years and some months, without any previous fit or other appreciable cause, was found one morning to have lost the use, and a great portion of the sensibility, of the left arm, which was swollen throughout its extent, the right arm being also implicated, though to a very inconsiderable degree. The right pupil is much dilated, the left is contracted; the evacuations from the bowels have been dark and offensive. No tender spot can be perceived on the spine; all the functions are healthy.

This case was also treated with strychnia, preceded by smart purgatives of calomel, &c., and with no better success than the last. The limb is now, after the lapse of nearly two months, hopelessly paralysed, and swings like a suspended object attached to the body.

Case IV.—John Hollis, a little boy

of 2 years and a half, was lately seized with exactly similar symptoms to those above described. The action of the left lower extremity entirely lost, sensation much diminished; the temperature of the affected limb is diminished, and it is somewhat reduced in size, the muscles being more flabby than those of the other limb.

1. The extraordinary youth of the patients is to be noticed. It will be observed that the age in all the above cases corresponded within a few months.

2. Although each case was either preceded by or ushered in by some apparent cerebral symptoms—viz. in two by drowsiness, in the others by an abnormal state of the pupil—yet

3. It is remarkable that in no one instance has the health been in any degree impaired.

4. If the case in which the remarkable strabismus occurred should lead us to suspect a cerebral complication, rather than a spinal one, there is other suspicion of congested, oppressed, or irritated brain.

The above cases (of which the first was seen some time ago by Dr. Outram, and three of the four subsequently by my father, Professor Badham), may probably suggest analogous ones in the experience of some few of your readers, who may also be able to help me to a treatment from experience; for we know nothing of the intimate character of any species of palsy, so as to manage it philosophically, and to adopt a medication without a defined object. A view which admits of being stated in intelligible language, is scarcely medicine. Some would doubtless in these cases recommend issues; some would counsel electricity; others would put me in mind of Arnica, or other *soi-disant* remedies for palsy; but I am unwilling to disturb the digestive and nutritive functions in such young subjects, and would rather count on these important processes as my auxiliaries, than venture on equivocal remedies which may, or rather must, depress them. I shall be thankful, however, for any suggestion; for it is lamentable indeed to witness one of the most humiliating infirmities of age inflicted on infancy! Perhaps one may reasonably expect some of the little patients to outgrow their disastrous condition, unless it shall be concluded that palsy in such instances is only an unusual and early evidence of deeply latent cerebral mischief.

Case of Diffusive Cellular Inflammation of the lower Limb, successfully combated by Calomel, Turpentine, and Leeches.

Ann Hudson, aged 4, was brought to me late one evening, when the following notes were made:—The child had been attacked three days before I saw it. The right limb much swollen, inflamed, burning hot, and pitted on pressure; the integuments shining; their colour a dusky red. The mother had traced the erysipelatous tumor, for such it is, *from a small spot or pimple under the knee*, which came, she said, spontaneously, and without injury. Febrile symptoms are now urgent; voice shrill; head seems much affected, and the hands are frequently raised to it; there is constant moaning, and no sleep since the seizure. The inflammation extends half way up the thigh, but the most intense redness is below the knee. The bowels have been opened, but no relief has resulted. The tongue is moist, but with a dark fur. Leeches were applied, and to encourage the bleeding the limb was enveloped in a poultice made with spirit of camphire, &c.

Eight grains of calomel were directed to be taken immediately, and two grains every two hours afterwards till seen again; and a terebinthinate purge and enema administered.

Aug. 24th.—The tumefaction is now checked and stationary, but the redness and general pyrexia not diminished. Bowels not yet freely opened; the moaning continues. An almost mathematical line separates the inflamed and healthy cutis. Lest it should be transgressed, ordered this line to be pencilled with lunar caustic, and the calomel to be repeated every four hours, with a repetition of the terebinthinate draught and enema. Towards evening eight more leeches were applied on the most inflamed parts.

25th.—A more quiet night resulted; less moaning and fever, and tongue cleaning; the limb, or that portion circumscribed by the caustic, has still a red and dusky colour, and is very hot, and the integuments tense. Fever becoming higher in the evening, I ordered two free incisions through the integuments and cellular tissue, and spirituous fomentations.

26th.—This day every urgent symptom subsiding; tongue cleaning; bowels freely purged; skin cool, &c. Had refreshing sleep during the night, and all subsequently went on well.

I had a strong reason for the activity of treatment here adopted. This child's brother, a year older, had been seized in a precisely similar manner, and like it, from some constitutional peculiarity, without any obvious or external exciting cause. I had been called to see it when both legs were much distended, and large effusion had already taken place throughout the cellular membrane. Cerebral symptoms were clearly denoted by a stupor and a moaning when disturbed (for otherwise it would lie and complain not) increased the alarm. To all this excitement a state of collapse suddenly supervened. The child died. At its very commencement I had predicted this fatal issue, from having treated three similar cases in black children in the West Indies, none of which had excited my apprehension at their commencement. I verily believe the child whose case I have given to have been unequivocally saved by the treatment; and I believe these cases of diffusive cellular inflammation end, when fatal, by effusion in the brain.

Worksop, Notts,
Oct. 19, 1835.

ACCOUNT OF A CANADIAN MEDICAL SAINT,

AND HIS METHOD OF PRACTICE IN CHOLERA.

To the Editor of the Medical Gazette.

Sir,

In the Lancet of the 10th instant is contained a communication which has the following statement:—" In the summer of 1832, when the cholera was devastating the Canadas, *a tattered old man* appeared in Montreal, whom the Catholic populace were induced speedily to canonize for his *successful treatment* of this disease, which was at that time almost as mysterious to the population as the stranger himself. To this modern saint vast numbers of the infected were brought, who were *restored to health from the most violent attacks of the inflammatory stage, as well as the worst possible collapse*, even after the duly authorised practitioner had exhausted and relaxed his efforts. The remedy consisted of a due admixture of maple-sugar, charcoal, and lard, which melange was given to his patients in large quantities after every ejection. The results were astonishing, and could be indicated in the speedy alteration of the countenances of the sufferers, and the early subsidence of vomiting and purging."

Now this " tattered old man," as he is here called, I happen to have personally known. His name was Ayres; he came out of the States, and was said to be a graduate of the University of New Jersey; but I never heard himself say that he possessed the latter distinction. He, however, appeared unexpectedly and oddly, giving out, or causing it to be given out, that he was St. Roche, the principal patron saint of the Canadians, and that he had descended from heaven to cure his suffering people of the cholera. Nor did he fail of doing good by this means; for the bigoted people think every thing of their saints and priests, and nothing of any body else; therefore he in a great measure cured them of fright, which was five-sixths of what he had to do. As he took no money for his pains, he of course had plenty of patients, and a great fame; and the doctors (some of whom afterwards fell martyrs to their exertions during this period) were not a little pleased to find him easing them of the most unpleasant part of their burden.

The writer does not give the whole of Dr. Ayres' plan; for in addition to the maple-sugar, charcoal, and lard, he had the patient rubbed with lye-water, and gave him plenty of soup (broth rather) to drink; also water in which hot charcoal had been immersed. But now, as to his cases and success.

The great mortality from cholera at Montreal was in the first three weeks after its appearance; during which time, as all was consternation, and Dr. Ayres's fame fresh, of course every recovery where he had been called in was trumpeted as a case of recovery from spasmodic cholera; and I fear that the writer of the communication alluded to rests somewhat on popular report for his information, when he says, "vast numbers of the infected were brought, who were restored to health from the most violent attacks of the inflammatory

stage, as well as the worst possible collapse, even after the duly authorised medical practitioner had exhausted and relaxed his efforts." But if he was on the spot at the time, and witnessed what he relates, I can only attribute it to the confusion which prevailed, that I was not able to obtain similar evidence. Now Dr. Ayres's patients were nearly all Roman Catholics, and he was constantly going among them as fast as the best Canadian horses and gigs could carry him; so that there could have been comparatively few of them whom he did not see: yet the Roman Catholic burials in that year amounted to 2732, whereas they were only 989 in the year previous; and of the whole population (of which the Protestants make comparatively a small part), the number of deaths in three months and a half during the progress of the cholera, was very nearly equal to the annual average in the nine preceding years.

But as to the nature of the disease which Dr. Ayres generally treated; I contend that his successful cases were not spasmodic cholera at all. Ever since that period until November of last year, I lived on the spot; knew the Saint; had frequent opportunities of conversing with him, and ever made the most diligent inquiry concerning his practice of every one who was in the least measure able to afford information; and yet I never could be shewn any man, woman, or child, whom he had cured of spasmodic cholera. There was scarcely any case of his which recovered that differed at all from the ordinary summer diarrhœa of the country. He commonly said, when called to a real case, "It is too late,—you did not have me soon enough." And although he for some time held up my friend, Mr. Forsyth, of Montreal (who in his anxiety obtained his advice) as a subject of recovery, it clearly appears that Mr. Forsyth's case, although it would have been looked upon suspiciously by any medical man at first, yet had not in it one infallible symptom of spasmodic cholera. The trick, however, became so well understood, and the Doctor's disinterestedness so generally suspected, that upon his presenting a petition for remuneration to the House of Assembly in the following January, it was wholly rejected, notwithstanding that the members are almost entirely native Canadians. But I can yet speak more to the purpose on this point:—The first cases of cholera which came under my notice, were among the raftsmen, on the Ottawa. I had never seen the disease before, and being in an uninhabited part of the country, without any other medical person to consult with, felt anxious to try every thing which was spoken of as at all successful. I accordingly obtained Dr. Ayres's prescription from himself, and, as the ingredients were at hand, had a large quantity of this ointment-medicine prepared, and gave it out to every body who asked for it. However, both this and every other means which I tried completely failed there, and I did not save a single case. I was shortly ordered to Quebec, to assist the medical officers in that garrison; in a week after which, a Serjeant Evans (of the Royal Staff corps), and a servant of my own, both healthy young men, with three children of the former, and several women belonging to a small detachment left behind, became martyrs to the charcoal system.

But Dr. Ayres had another chance of displaying his powers; for, in 1834, Montreal was just as badly off from the cholera as before; and the disease, at this latter period, carried off more people of high rank, who had every facility of employing him, if his fame had stood the test of public opinion (for he remained in the neighbourhood, and, notwithstanding his person and circumstances, had succeeded in marrying one of the fairest Canadian damsels resident there, and who was also one of the first-fruits of his miraculous powers), yet nobody thought of calling him in; and his obscurity was only to be contrasted with his former renown. How shocking, that, in one month and a half, thirteen hundred and twenty-five people should have died, and he in the midst o. them!

I do not profess to know what share his medicines might have had in curing the cases of diarrhœa, which he treated as cholera, or how much the confidence he inspired prevented the former from running into the latter disease; but every one knows, that, from the days of Celsus to the present time, diluents have proved highly instrumental in curing diarrhœa, and hence the value of his soup;—added to which, the practice of producing vomiting, by whatever

means, greatly assists in curing the disease in that particular country, which was effected by his huge balls of lard.

I am, sir,
Your obedient servant,
J. FARR.

Woolwich, Oct. 29, 1835.

MISCELLANEOUS CASES IN PRACTICAL SURGERY.

To the Editor of the Medical Gazette.

SIR,

THE following cases have occurred, within the last six weeks, in my practice; and as they present some points of peculiarity, you will probably deem them deserving of a brief record.

I am, sir,
Your obedient servant,
T. F. PALMER.

38, Golden Square,
Nov. 5, 1835.

CASE I.—*Irritable Stump, arising from the inclusion of the Fibular Nerve in the Cicatrix, in consequence of which its extremity became bulbous.*

W—— C——, æt. 52, had his leg amputated on November 23, 1832, at the usual distance below the knee, on account of an accident which had crushed the inferior parts of the limb. About nine months after the operation, the patient began to experience twitches and cramps of the muscles of the stump, which gradually became more severe until August 1834, when the surgeon who had amputated the limb removed a portion of the fibula, conceiving that this might be the cause of irritation, as it was more prominent than it ought to be. No relief, however, was experienced from the operation, but, on the contrary, the symptoms continued to increase so as to render the patient's life truly miserable, partly from the pain which he suffered, and partly from the deprivation which it occasioned of his natural rest. At the time that he applied to me, the muscles of the stump and thigh were incessantly agitated by the most violent muscular contractions, attended with the usual pain which accompanies cramp. Having examined the cicatrix with great care, I found a minute papillary projection at the lower part, which, from its excessive sensibility, I had no doubt was the extremity of a divided nerve; so that I had no hesitation in immediately recommending its removal, as the only effectual mode of getting rid of the painful annoyance. The operation by which I accomplished this, I performed with the assistance of my friend, Mr. Ford Copeland, of Argyll-street. I insulated the nerve for about half an inch, and removed it. The trunk of the nerve, I found, was about twice its natural size, terminated by a bulbous extremity, about twice as large as the nerve itself.

The relief which has been afforded by the operation is not absolutely complete, for the stump is still occasionally agitated by slight spasms, and the nerve, for above two inches above the cicatrix (as may be perceived by feeling through the external integuments) is still enlarged, and very sensitive. Unquestionably the relief which has been afforded is immense; but I now regret that I did not, in the first place, remove a greater length of the nerve, so as completely to obviate the possibility of its being again involved in the new cicatrix.

CASE II.—*Hæmaturia, in which five pints of bloody fluid were evacuated from the Bladder at one time.*

On the 5th of October, 4 P.M., I was requested to visit D—— T——, æt. 45, who was represented as labouring under retention of urine. When I entered the apartment, I was forcibly impressed with the look of anxiety and distress depicted in my patient's countenance, which appeared to me to indicate that serious mischief had already taken place. I found that an irritation of the urethra, attended with strangury, had come on on the preceding morning, without any apparent cause, which, in the course of a few hours, terminated in complete retention of urine, characterized with the usual symptoms of ineffectual straining. He lay in this state for upwards of twenty-six hours, at the end of which time the bladder (which was much distended) projected considerably beyond the umbilicus, attended with a plain sense of fluctuation. I immediately introduced a large silver catheter into the bladder, and drew off five pints of bloody, grumous, highly offensive matter, which had nearly the same consistence as the fluid which is evacuated

from ovarian dropsy, and was neither acid nor alkaline to tests. I afterwards repeatedly injected the bladder with tepid water, until it ceased to be discoloured; ordered a large opiate draught, and directed my patient to return to his bed; and in the course of two days he was perfectly recovered. No coagula were observed in the fluid.

On the 25th of October, that is, just three weeks after the preceding occurrence, he was seized with acute pain in the small of his back, stretching down to the groins, attended with numbness of the left thigh, retraction of the left testicle, and sickness. These symptoms lasted for four hours, and then completely went off. I conceive, therefore, there can be no obscurity respecting the source of the hæmorrhage in the first attack. It can scarcely be doubted that a renal calculus became dislodged from some cause from its nidus in the kidney, and abrading the pelvicular mucous lining, occasioned an effusion of blood into the bladder. A further change in its situation on the 25th, probably led to its descent through the ureter, and final expulsion from the bladder.

The small quantity of urine which was mixed with the effused blood, may be accounted for by the hæmorrhage, which, when considerable, always occasions a more or less complete arrest of the action of the kidneys. I am at a loss, however, to understand the reason why the blood did not coagulate in the bladder, or, not being coagulated, why it was not promptly evacuated.

Case III.—*Epilepsy from Venereal Enlargement of the Cranium, cured by the operation of Trepan.*

J—— B——, æt. 35, requested my advice on the 24th of October, 1834. She complained of intense recurrent headaches, situated over the right temple, accompanied with a few lichenous eruptions over the arms. The cranium, at the part to which the pain was principally referred, was exceedingly tender to the touch, but there was no swelling or apparent inflammation. From the history which I was able to extract from her, there could be little doubt that her husband had inadvertently communicated to her the disease, and that the symptoms which she complained of had a venereal origin.

I shall not, however, occupy the time of my reader with detailing all the circumstances of the case, or the various modes of treatment which were had recourse to without effect. Suffice it to say that they none of them afforded more than partial or temporary relief. The eruptions, it is true, on the skin disappeared after a few weeks, but the pain and tenderness of the head became daily more aggravated, until, towards the beginning of the present year, she was seized first with fits of temporary forgetfulness, then with attacks of fatuity, and finally with epileptic paroxysms, which, as time wore on, became more frequent, and lasted for a longer time.

In March I divided the integuments of the forehead down to the bone, which afforded complete relief from the fits, and partial relief from the headaches, for six weeks; but the slight caries which resulted from the operation soon healing over, and the fits recurring with redoubled severity, I was now compelled to remove a circular portion of the scalp, with a view of encouraging a plentiful suppuration from the bone, and giving a free egress to the matter. This also arrested the fits for a few weeks, but after a while they recurred again, and were a third time relieved by a seton in the neck, which procured for her a more complete respite than she had yet enjoyed. This, however, did not last.

About the middle of September, as she was walking in the street, she was seized with complete hemiplegia of the left side, without, however, losing her consciousness, or the occurrence of any convulsions. The hemiplegia was relieved by venesection and cupping; but the fits now recurred more frequently than ever, so that on the 3d and 4th of October, she scarcely recovered from one fit before she fell into another.

Finding that no relief could be given by other means, and being apprehensive, from the degree of cerebral excitement, that effusion on the brain would speedily ensue unless something was done, I determined on trepanning the frontal bone, which operation I performed on the 5th of October, with the assistance of my friend, Mr. F. Copeland. I found the cranium more than twice as thick as it is naturally, and firmly adherent to the dura mater. She had one severe fit within two hours after the operation, and another on the following morning,

since which time she has been perfectly free. The dura mater, as I expected it would do, sloughed opposite to the opening made into the cranium, but without any unpleasant consequences. The parts are now firmly incarned over, and the danger of a hernia cerebri entirely past. The patient declares that she has not felt so well for the last two years.

I thought it probable, when I undertook the operation, that matter might have formed between the dura mater and bone; and it was only upon the chance of finding this state of things that I recommended the operation. In this, however, I was mistaken; the dura mater was firmly adherent, and perfectly healthy; so that I can only account for the relief which has been afforded by supposing, first, that an inconsiderable degree of pressure, arising from the thickened cranium, has been removed; and secondly, that the free discharge proceeding from the diseased part, may act as an excellent counter-irritant, and at the same time tend to reduce the thickened bone. I do not, however, contemplate that the relief will be effectual or lasting.

ON ANIMAL HEAT AND VITALITY.

IN REPLY TO DR. WILSON PHILIP'S LETTER.

To the Editor of the Medical Gazette.

SIR,

ONE object of my last letter was to disclaim any attempt to decide on the still disputed points concerning the nature of the nervous influence, and its relations to secretion and muscular action. Dr. Wilson Philip, in his last communication, regrets that I decline entering upon these questions; and I can assure him that my indisposition to do so proceeds neither from "indolence of mind which shrinks from difficulties," nor from want of interest in the subject; but from the conviction that, until we have more decisive facts than those which we at present possess, discussion will be as fruitless and as unconvincing to future as it has been to former arguers. Many of your readers must be aware how much these subjects have been debated, from the time of Whytt and Haller to the present day; and although no one denies that the experimental researches of Dr. W. Philip, and other recent inquirers, have greatly increased the precision and extent of our knowledge in these matters, yet we find standard physiological writers not only differing from Dr. Philip in his conclusions, but stating that the province of the nervous system in producing the phenomena of life, were more accurately understood by Haller*.

So far from conclusive, so little convincing, have been the numerous experiments of modern physiologists, that it would be difficult to find any two authors who entirely agree in their deductions from them; and the proverb, "quot homines, tot sententiæ," may, with peculiar aptitude, be applied to the investigators of the functions of the nervous system. To give a brief example.

The researches of Dr. Wilson Philip are, I believe (though he appears not to think so), well known to the reading part of the profession: yet Drs. Bostock, Alison, Henry †, and others, while they assent to his conclusion that muscular irritability is independent of nervous energy, do not admit his views of the nature of the nervous influence, or of its relation to the processes of secretion and nutrition. Mr. J. W. Earle, on the other hand (the latest writer on the subject), rejects Dr. Philip's conclusion, which the authors just named admit, and adopts those to which they refuse assent; except with regard to the nature of the nervous influence, which he also holds has *not* been identified with electricity. Thus, with the addition of a tribute to the improved state of physical science, we find revived the old discussions of Whytt and Haller, in which the student of the history of medicine may learn how inconclusive are discussions, and even experiments, in physiology, when founded on a preconceived and favourite system.

After these declarations, the majority of your readers will excuse me if I decline entering on the much trampled arena of *nervous* discussion; and in the following comments on certain points of Dr. Philip's letter, as well as in my former communications, my wish is to preserve a neutrality with regard to opinions concerning the influence of the nervous system on the processes of

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* See Preface to 2d edition of Professor Alison's Outlines of Physiology, and Dr. Bostock's System of Physiology, vol. ii.

† Third Report of the British Association, &c.

secretion, nutrition, and muscular action.

On this ground, in my last letter, I did not deny the power of the spinal marrow to produce heat; but I stated that its share in this process is hypothetical. It is on ground disputed in the general questions of nervous agency, from which I purposely keep aloof. I will merely observe, that the same "direct facts" which Dr. Philip considers to prove that the spinal marrow contributes to maintain animal heat, have been associated, in Dr. Marshall Hall's observations, with failure of the capillary circulation*. I am quite aware that certain injuries of the spinal marrow, or of any other part of the nervous system, will cause a depression of animal temperature; but whether this effect is direct, or intermediate through functions which such injuries to the nervous system paralyze (as a crushed limb will stop the heart's action), is an unsettled question. Thus, also, Chossat (whose excellent memoir is less known than it deserves to be) found that bruising the semilunar ganglion of the great sympathetic nerve, or tying the aorta below the diaphragm, was followed by as rapid a diminution of animal heat, equally in the œsophagus and rectum, as could be caused by removing the brain or spinal marrow †.

The following paragraphs contain a position which may be commented on without treading on the questionable ground:—

"The confusion which prevails on this subject cannot, I think, be better illustrated than by the circumstance of a person so well acquainted with it as Dr. Williams supposing that animal temperature *can in any degree be maintained* by the functions of circulation and respiration after the influence of the nervous system is wholly withdrawn." "When the nervous influence is wholly lost, the mechanical parts alone of circulation remain. It is impossible, under such circumstances, that *any degree* of animal temperature can be maintained, the chemical part of the vital functions wholly depending on that influence."

Now it would be in vain for me to seek support for my *supposition* in any experiment on the body of an animal. Although I might destroy the whole brain and spinal marrow, and then find that artificial respiration still retards the cooling of the animal, yet this process, and the circulation that it maintains, would get no credit for the heat generated; for under these circumstances Dr. Philip says, "whether the brain or spinal marrow exist or not, there is already in the *nerves* more nervous power than the blood can use*." But if I remind Dr. Philip that some heat is evolved from *blood out of the body*, both during and after the absorption of oxygen, *from changes merely chemical*†, which, as far as we know them, resemble those which take place in the body, and that the temperature of the blood in the body, and its perfect exposure to the air by respiration and circulation, must greatly promote these changes, will he not admit that I have some ground for "ascribing to the functions of respiration and circulation the power of generating heat, although to small extent, unless supported by other functions‡?"

The latter part of Dr. W. Philip's letter contains remarks with many of which I entirely agree; but as far as regards myself, they are unnecessary, since they arise from a misinterpretation of a sentence in my former letter. "Whilst we admit the possibility of electricity having properties yet unknown which may hereafter explain the various processes of organic life, we must for the present content ourselves with referring them to a cause of which we confess our ignorance by calling it vitality." The real meaning of "our ignorance of the cause" may be explained by placing the phrase in opposition to a sentence from the commencement of Dr. Philips's letter: "Till the functions of the sensorial and nervous powers are clearly distinguished, and *the nature of the latter ascertained*, all discussions respecting them must more or less be, as they have always hitherto been, involved in error and consequent confusion."

In conclusion, I again disclaim for my views respecting the changes of the blood and animal heat, a necessary connexion with any of the various opinions

* On the Circulation of the Blood.
† Mémoire sur l'Influence du Système Nerveux sur la Chaleur Animale. 4to. Paris, 1820.

* Experimental Inquiry, 3d edition, p. 157.
† My own experiments, formerly noticed, and those of Dr. Scudamore: Essay on the Blood, &c.
‡ Medical Gazette, p. 50, *ante*.

respecting the nervous influence and its province in the phenomena of life. I have, indeed, referred to facts connected with this subject, but the interpretations of these facts have been excluded as doubtful; and if in two or three passages the words nervous and vital have been too indiscriminately used, a correction of these terms will not injure the tenor of the arguments. My views of the cause of animal heat are not inconsistent with those of Dr. Wilson Philip, since he no longer terms animal heat a *secretion*. The object of our inquiries are distinct: his refer to the vital functions which determine the evolution of animal heat; mine to the chemical changes which are its immediate cause.—I am, sir,

Your obedient servant,

CHARLES J. B. WILLIAMS.

Half-moon Street,
Nov. 10, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Friderici Arnoldi Icones Nervorum Capitis Heidelbergæ. 1834. Folio. Baillière.

THESE are at once the most beautiful, accurate, and economical plates of the nerves, which we have seen. Their number is nine, each accompanied by an outline, with numerals; and the price is but thirty-two shillings. They are, at the same time, strict copies from nature, and interesting specimens of the lithographic art. The author says, in his preface, "Summa religione affirmare possum, nulla filamenta in iconibus pro nervis recepta esse, quorum natura dubitationis ansam præbuerit." He then enumerates the points in the anatomy of these nerves of which he is the discoverer, viz. the otic ganglion, the auricular branch of the vagus, the recurrent of the first branch of the fifth, the nervus petrosus superficialis minor, the gangliform enlargement of the facial, and the conjunction of the sympathetic with the auditory. The author adds, lastly, "Omnes compilatores oro atque rogo, ne tangant has tabulas;" and states how injurious incorrect copies of such plates become to the cause of science.

We shall proceed to detail the principal of the author's views of the nervous system. They will be found, we are persuaded, to be full of interest.

Nerves of different kinds are distributed to the head, on account of its numerous and important organs. The cerebral nerves, the four superior cervical, and the highest portion of the nerves of the vegetative system, belong to this region of the body.

The head is the seat of the sensorium. The organs of the senses are situated in the head; those nerves prevail, therefore, in the head, which belong to animal life. The reverse of this is observed in the abdomen; for the vegetative system prevails in the region of the viscera.

The nerves of the ganglia are principally distributed to the arteries; the cerebral and spinal nerves, to the voluntary muscles, and to the organs of touch. The cephalic portion of the sympathetic nerves gives many filaments to the carotids, external and internal, and to the vertebral arteries, and forms several conjunctions with the cerebral and spinal nerves. The four superior cervical nerves are distributed upon the occiput and neck; the cerebral nerves, upon the cranium and face, and the organs attached to each of them, upon the larynx and lungs, the œsophagus and stomach.

Twelve pairs of cerebral nerves have been enumerated from the time of Sömmering. Their denominations are as follow:—1, the olfactory; 2, the optic; 3, the oculo-motory; 4, the trochlearis; 5, the trigeminus; 6, the abducens; 7, the facial; 8, the acoustic; 9, the glosso-pharyngeal; 10, the pneumo-gastric; 11, the accessory of Willis; 12, the hypoglossal.

All the cerebral nerves may be aptly divided into two classes: the first comprises the nerves of sense, properly so called; the second, the *intervertebral* nerves of the cranium. The first (the olfactory), second (the optic), and eighth (the acoustic), belong to the first class; the rest, which, like the spinal nerves, have a double, anterior, and posterior root, belong to the second, take their origin from the brain, and pass between the vertebræ of the cranium.

The intervertebral cranial nerves may be subdivided into two *pairs* : of these, the third (oculo-motory), together with all which follow to the seventh (the facial), forms the *anterior pair;* the ninth (the glosso-pharyngeal) to the twelfth (the hypoglossal) inclusive, constitute the *posterior pair.* The *posterior root* of the anterior intervertebral pair is the large portion of the fifth pair (trigeminus); the *anterior root* is constituted by the third (the oculo-motory), the fourth (trochlearis), the sixth (abducens), and the minor portion of the fifth, or the masticatory. The *anterior root* of the posterior pair of intervertebral nerves comprises the eleventh (the accessory) and twelfth (the hypoglossal): the *posterior root*, the tenth (the pneumo-gastric). The cerebral nerves may, therefore, be arranged thus:—

Class I.—1st, 2d, and 8th pairs.

Class II. ....

Roots.	The Intervertebral Nerves, viz.:— The Anterior.	The Posterior.
Anterior.	3d; 4th; 6th; minor portion of the 5th; 7th pairs.	11th; 12th pairs.
Posterior.	5th pair.	9th; 10th pairs.

The cerebral nerves may be divided in another manner:—The nerves of the anterior and posterior intervertebral pairs are *mixed*, *i. e.* are *motor* and *sentient*, or *simple*, *i. e.* are *motor* only. To the *former* are to be referred the fifth (trigeminus) and seventh (the facial), the tenth (the pneumo-gastric), with the eleventh (the accessory), and the ninth (the glosso-pharyngeal); to the *latter*, the third (the oculo-motory), the fourth (the trochlearis), the sixth (the abducens), and the twelfth (the hypoglossal).

In this point of view the cerebral nerves may be distributed thus:—

Class I.—1st, 2d, and 8th pairs.

Class II. ....

The Intervertebral Nerves, viz.:—	Mixed Nerves.	Simple Nerves.
The Anterior.	5th, 7th pairs.	3d, 4th, and 6th pairs.
The Posterior.	10th, with 11th, 9th pairs.	12th pair.

Of the cerebral nerves, the third, fourth, and sixth, move the eyes; the twelfth, the tongue; the fifth gives sensation to the face, the nostrils, the mouth; the tenth, to the œsophagus, the larynx, and the lungs. To the fifth is added the minor portion, as the masticatory nerve; to the tenth is added the eleventh, as a vocal and respiratory nerve. The larger portion of the seventh is the motor nerve of the face; its minor portion is a nerve of sensation. The ninth pair, by its greater part, gives sensation to the root of the tongue and to the pharynx, and by its minor portion moves the pharynx.

The nerves of the first class differ from the other cerebral nerves in their structure, formation, and vital property; they give off no branches in their course; they resemble the cerebral substance in structure more than the intervertebral nerves; they proceed, in the fœtus, from the cerebral cells; they perceive some irritants alone, as odours, light, and sound.

The intervertebral nerves resemble the spinal nerves; both their anterior

and posterior roots arise from the cerebrum: the posterior root is furnished with a ganglion, of which the anterior is destitute; the posterior consists of many fine filaments which extend to the ganglion; the anterior, although it arises in several filaments, immediately forms a round trunk. The intervertebral nerves give off filaments in their course, and in the organs into which they penetrate. They are not formed by the cerebrum or spinal marrow, but *per se* at the point of origin; they perceive various irritations, so that they excite either muscular motions or sensations; the anterior and posterior roots convey their impressions in different directions. Since, therefore, one series of the nerves conveys the conditions of the brain and spinal marrow to the organs, so as to excite motions, whilst the other conveys external impressions and the affections of organs to the central parts, the intervertebral nerves may be divided into motory and sentient. The anterior root of the spinal nerves belongs to motion, the posterior to sensation. How much certain cerebral nerves belong to the former, and certain others to the latter of these, I have learnt (says the author) by the most attentive anatomical investigation.

By the cerebral nerves, a connexion is established between the sensorium commune and the external parts of the head, the lungs, and the stomach; the mind is, as it were, conveyed to these organs, and the sympathies and mutual influences of the internal parts are effected. The cerebral nerves convey impressions from objects perceived to the mind and constitute sense, or the acts of the mind to distant parts, and excite motions. The different functions of these nerves depend upon their nature, that of the particular part of the brain, and that of the external organ. Thus the trunks and filaments of nerves have, in the first place, different functions from their very structure: the true nerves of sense differ in structure, both from each other, and from the fifth and tenth; the motory nerves differ from the sentient. Thus each part of the brain, from its own structure, is allied to sense or motion, and communicates this or that property to the nerves which arise from it. Lastly, the organs themselves are allied, in their nature, to this or that property of the cerebrum.

The more complicated are the relations of an organ with the brain, the more numerous are its nerves, and the more various their origin and nature. The head comprises many important parts, and its nerves are proportionately numerous. The cerebrum and cranium, which exhibit, in their origin, the normal structure of the spinal marrow and the vertebræ, have undergone such a change in man, that the parts of each nerve are so separated in their origin and course, that many nervous trunks are found, of which the primitive and normal character will only be learnt by the laborious anatomist.

It must be well known to our scientific readers, that the cranium has been traced as being formed of bones compared to *three vertebræ.* These, of course, leave *two intervertebral spaces.* Through these the intervertebral cerebral nerves are supposed to pursue their course, maintaining a general analogy with the intervertebral spinal nerves, being divided only from the varied *forms* of these cranial vertebræ and their intervertebral spaces. There are still some difficult points in the theory; and there is especially great difficulty in imagining that all nerves are either nerves of sense, or of sensation and motion. The nervus septi narium and the pharyngeus, branches of the fifth, and some parts of the pneumo-gastric, present this difficulty. But we must not enter into this abtruse question at the present moment.

A Treatise on Poisons, in relation to Medical Jurisprudence, Physiology, and the Practice of Physic. By Robert Christison, M.D., Professor of Materia Medica in the University of Edinburgh, &c. Third Edition. 1835.

We are glad to find that this excellent work has now reached a *third* edition. It betokens a fully awakened and growing ardour, on the part of the profession, for the eminently interesting subject of which the volume treats.

Nor have the four years which have elapsed since the previous edition, been unproductive of material for the improvement of toxicology; they have enabled the author to add many new illustrations of the principles which he already advocated, and to introduce a few subjects altogether new.

It is by no means our intention to give a lengthened notice of a work so well known: enough for us to announce that it has once more re-appeared, in an improved state. But we must add, that we can give Dr. Christison large, if not unlimited, credit for his vigilance and industry. We have traced him closely over several parts of his new ground, and no where have we detected him loitering or negligent; no where have we found any thing omitted which had the least show of practical utility to recommend it. Of course a notice of the chief points connected with the late Bristol medico-legal case is given; and we perceive that the author entertains much the same opinion that we do regarding the vaunted hydrated tritoxide of iron as an antidote for arsenic—namely, that it simply acts mechanically; he thinks, however, that the tritoxide may be occasionally of use, by enveloping the coarser powder of the arsenic with its fine, impalpable, and adhesive particles.

MEDICAL GAZETTE.

Saturday, November 14, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri: potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

GERMAN UNIVERSITIES—IMPORTATION OF DEGREES.

"It is not every man's lot," said the old adage, "to go to Corinth;" so that it must have been the fate of many in the olden time to be content with merely hearing of Corinth. At the present day, with all our boasted and acknowledged conveniencies by steam-boats and other flying machinery, it happens to the great mass of our profession to have little personal acquaintance with what goes on in other countries; but as we have abundance of travellers who contrive by

"Using limbs instead of head,
To have seen what we have read,"

we may be at once amused, and even reconciled to our privations.

Medical travellers, however, are rather scarce; or when they do make their appearance, we generally find them otherwise occupied than in drawing up notes for their more quiescent and home-keeping brethren. We must be, acordingly, grateful, we suppose, when any of them deign to think of us, and to write a book for our entertainment.

Mr. Edwin Lee has recently produced such a book*, and we are much deceived if it fail to interest most of those who look through it. The author visited almost all the principal seats of learning and medical science in France, Italy, and Germany; and the account which he gives of them is so sketchy and animated, that the chief fault to be found with it, perhaps, is that it does not present us with details more copious; it cuts us short too often, and leaves us to desire more ample information. Respecting Germany and its institutions, in particular, we are sure most readers would be glad to have a more ample statement; for that country has of late, for various reasons (to some of which we shall presently more explicitly allude), become a centre of attraction to no inconsiderable portion of the medical profession in Great Britain.

The hospitals of Germany resemble those of France and Italy, but are inferior in point of size and internal organization: some of them are supported by government; others, as in this country, by voluntary contribution. "A fixed salary," Mr. Lee says, "is allotted to the medical men,—who are not elected by public competition, but are mostly appointed by Government." "The bodies of those who die in the German hospi-

* Observations on the Principal Medical Institutions of France, Italy, and Germany; with Notices of the Universities. By Edwin Lee, M.R.C.S. &c.

tals are generally examined, though morbid anatomy is not very zealously cultivated."

Leave we, however, for the present, the German hospitals; the universities are more to our purpose. Those of Berlin, Halle, Leipsic, Bonn, Göttingen, and Prague, are rapidly touched off by the author: we only wish he had visited more of them, that he had gone even among the minor and worse appointed establishments.

The universities of Prussia are, we believe, very well conducted—as, indeed, the whole system of national education in that country would deserve to be held up to the imitation of other continental States, especially wherever there is a government as absolute as the Prussian monarchy is. But the multiplicity of those schools called universities, both in the Prussian and other States, cannot but have a tendency to detract from their value. There are above twenty of them throughout Germany, all in a great measure independent of each other. All the other Prussian provincial establishments, however, are subordinate to the supreme school at Berlin. "The title of Doctor," says Mr. Lee, "may be obtained at any of the Prussian universities; but all candidates are obliged to repair to Berlin for the public examination, which," he adds, "is practical, rigorous, and searching."

In the Universities of Saxony, of Wirtemberg, of Austria, &c. a similar system with regard to degrees seems to prevail. But in those of Bavaria and Baden, nothing can be more lax or reprehensible.

We wish Mr. Lee had given us a full account of the state of things at Erlangen and Heidelberg. We know enough of those places, however, as *Universities*, to warrant us in reprobating them as in one respect at least disgraceful to the present state of medical respectability in Europe. Their poverty, we must presume, has much to do with influencing their will: but it can hardly excuse the rapacity with which they grasp at the bait of money—the avidity with which they carry on the traffic for what we may venture to call their miserable and worthless degrees. They sell them, it appears, on all sides: they require no personal acquaintance with their graduates; they take no count of documents of recommendation—for how can they know that they are not forged? Only send them the money in due form, and the degree comes by return of post.

That such a system as this prevails at the present day, is, we imagine, scarcely suspected by the generality of people here;—that it has been countenanced in this country, perhaps less so. There are those, however, amongst us, who know it well,—who have dabbled in it,—who have bought their honours, and wear them, too, as if they came from more respectable quarters; others, also, who have done this, but have not the courage to put on the livery for which they have so handsomely paid.

It would be amusing (were there not something of fraud and dishonesty mixed up with the transaction) to notice the masquerading of some of these Erlangen and Heidelberg Doctors among us: we could point out half a score of them at least, the names of some of whom would fill many with surprise.

What is not less remarkable, is, that some of them, too, are very conspicuous among the *reformers* of the profession: advocates for "one faculty," and for a Metropolitan Board for conferring medical degrees. That such persons should preach one doctrine and practise another, is only what is commonly observed among people of their sort: but is it fair or honest in

them to cry out so loudly for a privilege-conferring board here, when they could so easily inform their brethren how they might be accommodated quite as cheaply, and with less personal trouble, elsewhere, and while they have themselves actually been *sub rosa* enjoying the fruits of the system?

Are these worthy persons aware that they are *smugglers*—evaders of the excise—that they import goods for which, if British (and they pass them off as such) they would have a certain duty to pay? Is the government aware that there actually exists in London an *office* for carrying on this contraband trade? We should not even allude to this system of trickery, did not those who deal in it parade before the public with their foreign tinsel upon them, and wear their pinchbeck as if it was sterling gold. The fraud—the fraudulent traffic—is what deserves exposure—and exposed, so far as the *system* goes, we promise it shall be. As to the *individuals*, we let them pass, at least for the present: nor need we, perhaps, further particularize them, as their own bearing will probably give them sufficient notoriety.

What need, we ask, is there of a board in London for conferring degrees, when there is an office in the City where any body, without stirring a step from town, may, with some thirty guineas and three signatures—(who can find a difficulty in that?)—become a *bonâ fide* M.D. with the speed of an interchange of letters? If there be any doubt of this, let an epistle be sent to Dr. Fleischmann, &c. &c. of the University of Erlangen, Bavaria, to know what is to be done for a medical degree, and a prompt reply will be returned, telling the writer where *the money* is to be lodged.

Is it not "too bad" that such pranks should be played before high heaven, and sanctioned by the once reputable name of a German University? How long will the other German establishments permit such things to be done? The force of opinion put down a similar abuse which was, up to a late period, practised in this country, by some of the Scottish Universities; and shall a foreign humbug, ten times more gross, be tolerated? Here there was a possibility of at least detecting fictitious documents, should they be forwarded to Scotland; at Erlangen and Heidelberg there can be no such check. The authorities at these latter places must know—must, at least, more than suspect—that they admit to the *highest honours* of their respective Universities a number of improper characters; and we cannot excuse them when they act deliberately under such an impression.

When it was proposed by the proprietors and shareholders of the Gower-street School, that that establishment should be endowed with a privilege of conferring medical degrees, we thought it not an unfair objection on the part of the established Universities of this country, that, should such privilege be granted, the denominations should not be the same; for there were certain rights connected with the regular graduates to which surely those of the joint-stock establishment could have no claim. On the same ground, now, we totally object to those newly-created Erlangen and Heidelberg doctors mixing among us, without some badge to distinguish them from those who have graduated in an open and unexceptionable manner. We, accordingly, most strongly recommend the legislature—if they do not go the length of suppressing at once the actual traffic in sham titles, imported from Bavaria and Baden—at least that they will fix such an appellation on the new graduates of London as may prevent them from being confounded with the foreign nondescripts. If the system be pursued here much longer—and there is every likelihood of its continuance if not put a

stop to by some decided public censure —it will become imperatively necessary on all those who have legitimate degrees, to qualify them in such a manner, in their signature and announcement, as at once to secure their own claim to respectability, and to put to the blush those personages in borrowed plumes. We shall then see how the M.D. Heidel. and the M.D. Erlang. bear the light beside the M.D. Edin., M.D. Dubl., M.D. Cantab. &c. Unless some such precaution be taken, we know not how the unpleasant confusion can be avoided: only let each person be explicit as to his proper title, and then there cannot be much mistake. In conclusion, should any of our readers think us harsh, or unduly severe in the preceding remarks, we beg them to consider the nature of the evil we are called upon to expose. To those who feel *personally* in the matter, we can only say,

> "Let the galled jade wince: our withers are unwrung."

Ere we close this brief article, we cannot avoid noticing a cheat of another kind, but equally disgraceful, which has recently come to our cognizance. In a certain Royal College of the United Kingdom, which has a school of medicine attached to it, the Professor of the Practice of Physic possesses no medical degree, but stoops to the unworthy expedient of announcing himself, on all occasions, as Dr. *****,—on the strength of an LL.D. degree, which he conveniently obtained for the purpose of *appearing* duly qualified for the appointment. Those who bestowed the place knew well of the trick, but countenanced it, as we fear such shuffling and trickery are but too frequently practised in the same quarter. We are not aware that the deception has been pointed out before; and our only motive for thus briefly calling attention to it at present, is that we consider it deserving of censure of the same kind as the gross abuse above complained of.

PROFESSORSHIP OF MATERIA MEDICA IN KING'S COLLEGE.

We understand that since our former notice on this subject, Dr. Paris has been appointed Professor of Materia Medica in King's College; and we think the institution has been fortunate in securing the services of a gentleman so thoroughly qualified for the chair he is to fill. Dr. Webster, who delivered the summer course, is at present lecturing in the room of his friend Dr. Bisset Hawkins. Dr. Webster declined becoming a candidate in opposition to Dr. Paris.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, November 10, 1835.

HENRY EARLE, ESQ., F.R.S., PRESIDENT, IN THE CHAIR.

THIS was the first evening meeting of the season, and a large number of the Fellows and their friends attended. The appearance of the library was imposing; it is a very spacious apartment, and looks splendid when lighted up. A paper from the pen of Dr. Yelloly was read, entitled, *Observations on Vascular Appearances of Mucous and Serous Membranes, as indicative of Inflammation.* We shall give an abstract next week.

A short discussion arose respecting the views of inflammation advocated by the author of the paper. Dr. Weatherhead, Mr. Mayo, Mr. Arnott, Dr. Clendinning, and the President, briefly expressed their sentiments.

After this, the President begged leave to introduce to the notice of the meeting a short paper of his own, which was accordingly read. It related to an operation which he lately performed at St. Bartholomew's Hospital, on a patient who laboured for fourteen months under a formidable tumor over the scapula. Opinions had been strongly expressed against the propriety of meddling with it, on the supposition that it was malignant; but the result was highly successful, and proved the correctness of Mr. Earle's views.

Before the meeting broke up, the President, in a short address, urged the Fellows to bring forward their papers as early as they could, and not to leave them over, as they generally do, till the latter part of the session. He congratulated the Society on the accession they were about to receive to their numbers, several candidates for admission to the Fellowship having been proposed in the course of the evening.

CLINICAL LECTURE

ON

DISEASES OF THE SPINE,

Delivered, Nov. 3, at the Middlesex Hospital School,

By Sir Charles Bell.

Remarks on the present System of Medical Education—Diseases of the Spine—Lateral Curvature—Curvature with Caries—Scrofulous Inflammation.

Gentlemen,—We have reason to regret that you are so driven from one lecture to another, and without interval or repose, from daylight till night. In these circumstances it is hardly possible for a youth, though possessed of the most ingenuous and happy disposition, to become a diligent student.

We owe the regulations under which we are bound to the Company of Apothecaries, and the criminal neglect of the two Colleges. The evil is twofold: the mind of the student is dissipated, and the office of the lecturer is degraded. It has always appeared to me, and is, I believe, acquiesced in by all educated people, that the student should hold his teachers in respect, that their place should be considered honourable, and that that, too, should be gained by the successful pursuit of the very studies they are inculcating to their pupils. The present system overturns all this. The course of my duties, as well as your course of study, is directed at the Apothecaries' Hall of London.

To whom, then, am I to bid you look as the heads of your profession, as showing you examples of devotion to science, and to whom, indeed, you ought to be directed by the respect and gratitude of mankind? I pretend not to say who is to blame. These changes commenced in Parliament, through the philanthropy of the moment, to prevent the lieges being poisoned. If I should be so unfortunate as to stand in opinion against Colleges and Parliament, I must from time to time call upon you to remember for what purpose you are here in London. Your long list of certificates you must have; but I conjure you to act as if anatomy, and such uses of anatomy as you see in hospital practice, were the business of your life in London, and not to be satisfied with learning to answer such questions as may be put to you at any board. Look upon the opportunities offered to you, as leading hereafter to the decision of matters of life and death.

What has been said of books is more applicable to lectures. A great English philosopher said of his adversaries—"If I read as much as others, I should be as ignorant as they were. The meditation on a few good works is more profitable to the understanding than an abundant draught of indiscriminate learning."

But let me turn to our present duties. I have requested you to come into the theatre rather than your room in the hospital, that I might show you these preparations in connexion with the cases you have seen. And now observe the advantage of giving clinical remarks to those to whom I know the right elements have been taught, instead of addressing gentlemen from three or four neighbouring schools, who have in all likelihood no ideas in common with me.

I will furnish you with an example how easy it is to give such lectures to those who have been initiated in the principles. If you go into Percy ward you will there find a man lying with a wandering and bewildered eye, with a very very pale face, and spasmodic twitching of the eye ball. There is a deep-coloured extravasation of blood around the eyes, with grumous blood, or brown serum, passing from his ears. This man has fallen upon the vertex of his head; and were I to enter upon the consideration of the case with those who had not gone through the demonstration of the bones of the cranium with us, you know full well that I should be obliged to enter upon the whole structure of the skull, and the mechanical principles on which it is built. But now, with one word, one half sentence, I can say to you there is the example of which I have been speaking. Here a blow has been inflicted upon the upper part of the parietal bone, and you see the effect upon the temporal bone, and upon the ear. Here is a man lying with a fissure in the base of the skull from a blow on the superior convexity. It is not requisite that I should go into the whole proof, and repeat the demonstrations; I have merely to say, that this is an illustration of the principles I formerly established*. Or, supposing that I have to point out to your notice another case, where the humerus is fractured near the head, I have hardly occasion to speak to you of the necessity of particular appliances to counteract that condition of the parts when, by the division of the bone, the adjustment between the pectoral

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* This man has since died. A rent was discovered passing across the base of the skull through both temporal bones, and the orbital plates were broken. There was ecchymosis of the scalp, and the brain had blood effused in its texture, near the vertex; but there was no fracture of the bone at this part. Purulent matter was found in the lateral sinuses.

muscle, the latissimus dorsi, and deltoid, is lost.

After this preface, I beg your attention to *Diseases of the Spine*. First, we have a case of what is termed lateral curvature. Secondly, there is a case of curvature of the spine connected with caries of the bodies of the vertebræ. Thirdly, there is a case of inflammation and scrofulous disease going on in the top of the spine, near the occiput. Fourthly, there is a case of irritation and spasmodic affection of the sterno-cleido-mastoideus, affecting the vertebra in a secondary way. There is next, fifthly, a case of permanent rigidity of the sterno-cleido-mastoideus, and distortion of the head and shoulder. These cases are at once under your eye in Seymour ward; and there cannot be a subject better suited for your especial notice than this.

Would that our philanthropic meddlers, who set you to study botany within the sound of Bow bell, knew the hundredth part of the miseries inflicted on this class of patients through ignorance, and the tricks of cold-hearted and selfish quacks, who will boldly undertake to reduce bones, such as you see here distorted by formidable internal disease, accusing you regular surgeons of incapacity and timidity.

The least of these errors, and one of daily occurrence—now I speak to your hearts—is the ordering a young lady, of delicate frame, with nervous and sympathetic pain in the back, to lie during eight and sixteen months in the horizontal position; one who is nervous, low, and spiritless, whom to keep in health requires all your means, and most of all stands in need of bodily exertion and change of scene.

LATERAL CURVATURE OF THE SPINE.

I shall take advantage of the dresser's book to read the cases written down. The first case is that of Eliza Milras, admitted so far back as July 23. There is here no case written out; but I have repeatedly pointed out what are the circumstances which characterise the condition of the spine in this young woman. Here is a young girl, and girls are chiefly subject to this lateral distortion of the spine, in whom you discover these marks: first, the parents of the patient notice an awkward gait as she walks across the room, especially if she be on ceremony, and they then observe that the right shoulder is *out*, and that the dress falls from the left shoulder. When you loosen the clothes, and expose the back; you find that the right shoulder is prominent, that the scapula is elevated; that the cause of the elevation is a large protuberance of the ribs, and not any affection of the scapula; that the shoulder is not in fault, but that the ribs, by spreading wide, and by their gibbosity, have thrown up the shoulder-blade. On the left side, and upper part of the thorax, the ribs are huddled together, and consequently depressed, and the scapula also. You find that the expansion of the ribs on the right side is owing to a curvature of the spine, the convexity of which is to the right; that there is a second curvature below, giving the spine the shape of the Italic *S*. You may trace the same characters in the reverse direction, as they progressively present themselves; that is, you find that the distortion occurs in an indolent girl, weak, with her constitution disturbed by the first occurrence of uterine irritation. She has pain in the back, great weakness, languor; and with this debility she *hangs*, as they express it in the north, upon one foot—upon the right foot. The consequence of that is the relief of the left extremity, and the inclination of the lowest part of the spine to the right side, with a slight spiral twist. When the spine is thus twisted to the right side by a habit of standing, the whole body would be thrown to that side, were it not steadied and poised by a second curve, such as you see here [shewing the specimen.] It is the second curve which throws out the ribs to the right. You see [still regarding the skeleton] the spine first inclined to the left side, and then inclining to the opposite side; and now you perceive that the great convexity being towards the right side, and the concavity towards the left, the left ribs are drawn together, and consequently the whole extent of the thorax is here diminished.

We shall not speak of the distortion of the fore part of the thorax, or of the appearance produced on the mamma. The ribs on the right are expanded, and consequently the convexity is increased; hence the scapula, lodging upon them, stands higher than the other; and thus we see the reason why the right shoulder is thrust out, and generally higher. You also observe the reason why there is an inequality in the pelvis, and why the mother or the governess first notices the thrusting out of the ala ilii. I mention this kind of distortion by way of contrast to what is to follow. It is a mere defect of a poor constitution,—of the constitution disordered and irritated by the first arrival of the period of constitutional change in the female; and most of all, is induced or promoted by indolence—by a want of that activity which I need not state to you is the great source of perfection of structure. That spine [presenting a preparation] is thus distorted; and you have an opportu-

nity of comparing it with all those other specimens before me where the deformity is a consequence of disease. The spine consists not only of bones, but of cartilages, ligaments, and numerous minute muscles, going from point to point; and you will recollect that I took the opportunity of stating that the bones did not grow by themselves, nor did the cartilages, ligaments, or muscles, but that they all belonged to one constitution, and failed or became perfect together; therefore it is quite superfluous for any person to talk of the affection being in the muscles, ligaments, or bones, each separately. The whole texture of the spine is weakened, and it is principally owing to that want of freedom of action which is its stimulus to perfection. The principle of cure, therefore, must be activity, which activity must be at first gentle, and may be directed so as to counteract the curves. But you must recollect that you do not bend the spine as you would straighten a crooked stick; you must direct its growth aright. And to be effectual and permanent in your cure, your means must be long pursued.

What these are, you have in part seen in the treatment of this patient.

SCROFULOUS CARIES OF THE VERTEBRÆ.

Let us now take up another case. "Ann Hatchies, 20 years of age, was admitted on the 9th of September. She states that about a year and a half ago she was subject to pain and weakness in the loins, and a general weakness of the whole body; that about two months after this she lost the use of her lower extremities; soon afterwards she was admitted into the Salisbury Infirmary. She continued there six weeks, during which time various remedies were administered, and issues were applied on each side of the affected part of the back, but without affording any relief. She says that she knows not how to account for her illness, unless it be that she had been accustomed to lift heavy weights. At present there is very little pain any where; she feels comparatively easy when in the recumbent posture, but when made to sit up she experiences a sense of weight and weakness in the back. Her lower extremities have lost all voluntary motion, but sensation is not entirely gone. At one time she had great difficulty in passing her urine, but now that symptom is much relieved. On examining the back, there is a projection backwards of some of the last dorsal vertebræ. The catamenia appeared when she was first affected, and have not been seen since. Her general appearance is that of a person in health. When the extremities are touched, they move, though without her knowledge. There is no relaxation of the abdominal muscles. She feels when the foot is cold, or when cold is applied to the extremities."

Here you have a case which presents before you, in a few sentences, the character of scrofulous caries of the vertebræ, or the *acute curve* of the vertebræ; and the first thing that you mark in this narrative is the formidable contrast it presents to the last. Again, I refer to what I said of the bones, that the bodies of the vertebræ themselves are spongy and light; and that in proportion to the spongy character of bones, are they subject to scrofulous inflammation. There is some little debate among pathologists as to whether the disease begins in the ligaments, the intervertebral substance, or the body of the vertebra. If we refer to dissections, we find that all are affected; and it is an idle speculation to endeavour to determine which is primarily attacked. We know that the bones, the ligaments, and the cartilages, are parts of the body most liable to scrofulous action; and in the spine they are most intricately combined. In the case, it is said that there was a wrench, or at least that the patient was obliged to raise heavy weights. Now it is very possible that the spine may have received on some such occasion a slight injury. I do not quite agree with my friend, that the girl appears to be healthy; for I should say, on the contrary, that she has a most distinct scrofulous diathesis marked in her appearance.

Now, gentlemen, you perceive the advantage of a school. Here is a case nothing at all like the last; and I put into your hands [referring to a specimen] a spine with an acute projection directly backwards, that you may contrast it with the former specimen. The projection in the back is formed, as you see, by the wasting, ulceration, and absorption of certain of the bodies of the vertebræ.

When you have these facts before you, I need hardly point out to you what will be the great questions of practice.

But first as to what is said of sensibility remaining, whilst motion is quite gone. There are some points in the structure of the spinal marrow, and the situation of the disease, that may relieve our perplexity on this head. You remember the character of the spinal marrow, and that the roots of the nerves of motion lie along it on the surface, next to the bodies of the vertebræ, while the roots of the nerves of sensation are on the posterior surface. Again, if you remember the manner in which the ligamentum denticulatum runs down on each side of the spinal marrow to connect it with its sheath, you will at

once perceive that there is a sort of septum—imperfect certainly—but a sort of partition, between the anterior and posterior surfaces of the spinal marrow. It certainly is not correct to state that it is the curvature of the spine which, causing the bones to press upon the marrow, produces paralysis. In fact, the paralysis precedes the sinking of the spine, and often disappears when the curvature is at the utmost. It is the inflammation of the bones which disturbs the function of the spinal marrow. If inflammation, therefore, takes place in the bodies of the vertebræ, it must affect the motor nerves earlier, and to a greater extent, than the nerves of sensation. We observe the fact in this case, and this is the explanation that I put upon it.

You are aware that there are people in town who pretend to restore the shape in this curvature or "dislocation" of the spine, and who ridicule the regular surgeon for not attempting and effecting the cure of this scrofulous caries of the vertebræ. They recommend the raising of the body, or the stretching of the spine by instruments. It comes, therefore, to be a very important question to you, what you should do in these cases. You may deceive yourselves, or you may deceive the friends, by extending the patient some inches, and you may support your patient in this dangerous position by an iron case; but what are you actually doing: you are raising one diseased surface of the vertebræ from the other. I repeat to you that inflammation loosens the textures of the ligaments, and when the ligamentous connexions are thus destroyed, and there is actual caries consuming one or two of the vertebræ, if you raise the spine, you rend the soft union of the inflamed bones, and make a gap between their surfaces, by the union of which it is that we hope for improvement and cure. It is to anchylosis that you must look for the most favourable issue; and unless the parts be kept perfectly at rest, and unless they be kept together, how in the world can we expect anchylosis to take place? Besides, there is great danger in thus drawing the diseased bones separate; for you will observe, that although there is a gap in the bones, there is still a soft ligamentous union, and this will be torn. There is another thing to fear, and that is the sudden falling down of the spine when it is thus raised. I look with horror upon the effect of a sudden fall when the bones are thus held apart by mechanical means. Such an accident would cause a sudden curve or check of the spinal marrow itself. What does this quackery mean? It is doing fatal injury if the disease has not proceeded to anchylosis. But if anchylosis has taken place, may good be obtained by this stretching, or twisting, and moulding? Be assured that all that can be performed is by causing the healthy part of the spine above and below the anchylosis to accommodate to the permanent curve, and be made somewhat straighter: a real advantage, you will say; true, but at great hazard to the part originally diseased. The quack accomplishes this at the risk of breaking up the bony connexion, or tearing asunder those connexions which are not yet bone, but only ligament, and thus exciting inflammation. You see, then, what must be the condition of cure—that the patient must be kept in perfect rest.

Look round the hospital, and you will see hundreds of instances—I do not, I am sure, exceed the number—hundreds of instances where motion is keeping up disease. It is so with all the joints, it is so with all the ulcers which give you so much trouble, and so it is here. As long as a person moves about, and twists and turns,—for you are aware that there can be no change of the extremities without a corresponding change of the centre—so long there is an incessant source of irritation. It is your business, then, to keep the patient in the horizontal posture, to have the mattress moulded so that the projection of the spine can sink into it without injury, and the sides so guarded that if the patient moves up at all there may be no motion in the inflamed parts.

Then comes the proposal of counter-irritation. Some surgeons say that this does no good; yet I have been loath to allow this. You may adopt counter-irritation by issues; but issues and setons are to be omitted when a fever of irritation is kept up in the patient's constitution, for as the remote origin of the whole is in the constitution, you must endeavour to rally its powers.

I would make just one remark more, because I see it stated in a book which, I believe, is a good deal read by the class of students, that there is no disease which presents itself in so great a variety of forms as this. This seems to be very extraordinary; but I think I know how this opinion arises; it is by mistaking the disease. When there is caries of the vertebræ—when there is an actual falling forward of the spine—the symptoms may vary, inasmuch as the inflammation may reach to the spinal marrow or it may not; it may be attended with more or less pain, with more or less paralysis; but I hold that the case is ever obvious in its symptoms. Nevertheless, it must be admitted

that there is nothing more difficult to distinguish than the different affections of the back and loins. The practitioner is continually falling into mistakes. A languid girl, of fifteen, complains of pain in the loins, and inability to walk or sit long. On the spine being examined, she starts away when a particular process is pressed. Here is the source of error; girls with no actual disease shrink and complain when certain spines are touched, and if you implicitly trust to this you may be misled. Now you say, "Do not deceive yourself; are you sure there is pain?"—and you try again. Still there is distinct pain; but in this we must not put confidence; and, by a little further examination, we find there is another painful part, remote from the first, and which, on being pressed, gives the same sensation. These are hysterical and nervous pains. Another source of mistake is in the pains symptomatic of internal disorder; as of the colon, kidney, or uterus or ovarium.

If a weak hysterical girl be, in these circumstances, laid on the horizontal plane,—confined necessarily to her room—disappointed of all variety, all amusement, and the muscles unexercised for a great length of time—what effect, according to the principle I formerly laid down, is to be expected, and what hope of a rally of the constitutional powers?

If I have raised one, I have raised twenty young women out of this condition alone; some of them having been from eight to sixteen months in the horizontal posture without any disease of the spine at all, but merely suffering from a sympathetic pain which arises from an internal disorder.

DISEASE OF THE UPPER VERTEBRÆ OF THE NECK.

I will now take the case of "Mary Blainer, 16 years of age, admitted on the 15th of October, on account of a distortion of the head upon the neck, or displacement of the upper vertebræ of the neck, which throws her head to the left shoulder. There is a swelling extending from the occiput, five fingers' breadth, down the neck. The third and fourth vertebræ project, but they are obscurely felt, as an inflammatory hardness surrounds the bones, and occupies all the upper and back part of the neck. She cannot turn her head without pain, and the chief motion is about the fifth vertebra of the neck;" that is to say, if she attempts to move the head, the spine yields only about the fifth vertebra of the neck, and all the upper part is an inflexible mass. Here is a preparation shewing what may take place in these upper cervical vertebræ, and renders it probable that such a soldering is in our patient now progressive. As a proof of what extraordinary defects may arise in the upper vertebræ, I shew you another preparation. There are two spinal canals, as it appears, and what could have come of the spinal marrow at the time the atlas and dentata were so strangely displaced? [The lecturer here held in his hand a remarkable specimen of fracture of the atlas and of the dentata, with displacement of the fragments: the anterior half of the atlas was united, by its two broken extremities, to the fore part of the dentata, and thus presented the appearance of two foramina for the spinal marrow in one bone]. In this patient, however, the chief motion is about the fifth vertebra of the neck. The case goes on to state, that "in the summer she had rheumatic fever, which continued for four months. On getting better of the fever, this swelling began. It is observed that she has no weakness of the arms or fingers, and never had. She has, however, been subject to a difficulty of swallowing sometimes:" but she says she has had no experience of this for some time. She herself attributes this difficulty of swallowing to a tumor which took place in the anterior part of the throat. There are two caustic issues in the neck, and she has taken iodine and liq. potassæ. Her head is more upright than when she came into the house."

The orders of the surgeon to support the neck by a collar, which shall reach from the shoulders to the ears, have not yet been executed.

The first reflection which I make upon this case is prompted by the long continued rheumatic fever; for in entering into these cases you find, for the most part, that the disease is preceded by long-continued fevers or suffering, measles, small-pox, scarlatina, &c. Confinement and general languor will produce a scrofulous diathesis, and a course of mercury, for whatever object pursued, will have the same effect; indeed, whatever reduces the constitution will induce a disposition which subjects the person to scrofulous inflammation from a slight sprain of these ligamentous parts.

I dare say this case must appear more formidable to you than the former. Inflammation in the spinal marrow is bad enough, but inflammation or pressure on the medulla oblongata must, you know well, produce instant death. We have had in the hospital some such cases of sudden death by fracture of the spine and occipital bone, and also by the giving way of the ligaments of the upper vertebræ. It is terrifying to think

of the perilous brink on which a girl in this condition stands unconsciously. You have been but lately informed how bone and ligament participate in scrofulous inflammation; and that when the ligament is inflamed, it becomes loose and weak in texture. When, therefore, we see a mass of inflammation around the atlas and dentata, must we not be apprehensive for the state of the ligaments which support the head in connexion with the vertebræ? A slight wrench might tear these ligaments in their weakened condition; and on the displacement of the vertebræ the medulla oblongata would be nipped, and in an instant, death would follow; for if there be pressure at that part, it being the origin of all the nerves of respiration, there is not a word spoken or a breath drawn, but death is instantaneous, as when an animal has been pithed.

Seeing that instant death would be the consequence of an injury to this part of the spinal marrow, we cannot avoid being alarmed at seeing such a mass of disease around the upper part of the spine. It is pretty obvious, at the same time, that this case resembles what is called the poll-evil in the horse. There are ligaments connecting the horse's head to the neck, and bursæ, which are under the great suspending elastic ligament; these ligaments and bursæ suffer when the horse is reined up suddenly, the reins and bit operating through the length of the head, as by a long lever, on the joint of the occiput. We cannot be surprised that the sudden pulling up of a horse should produce sprain, and injury, and inflammation, suppuration, and sinuses here. I apprehend that there is something of the same kind in this case, set up by a wrench, and that the disease may thus be happily external to the bones and theca. You remember how thick the theca is here, and the ligamentum infundibuliforme, that powerful ligament which runs down into the spinal canal from the dura mater, on the inside of the occiput, like a funnel. That, I verily believe, may prove to be the protection to the medulla in the present case, and which allows the girl to go about fearlessly at the moment that I am full of apprehension.

As to the cure, it must be effected by attention to the constant fixing of the part, by supporting the head, and preventing all lateral motion, and inculcating the necessity of avoiding all violence; and then time will solder these parts together, as it were, or, in a more accurate way of speaking, produce anchylosis; and the girl will be safe as to life, but, of course, she must lose all the easy motion of the head on the neck, which can only be imperfectly substituted by the additional motion of the lower part of the vertebræ of the neck.

All this is but preparatory to a case which I believe will interest you still more; it is that of a poor woman who has got an affection of the sterno cleido-mastoideus; it is a curious disease, of which I have seen a great deal. This, again, will lead to another case, in which there is a permanent rigidity of that muscle. We shall take these up on a future day, and then go into another ward, where I see some interesting cases of disease of the nerves of the face, which will follow very naturally afterwards.

ST. GEORGE'S HOSPITAL.

CASES, WITH CLINICAL REMARKS.

Articular Rheumatism—Chorea—Conversions of Disease—Balsams in Chronic Bronchitis.

The following case is a very good example of the effect of colchicum in simple inflammation of the joints. It was this power which caused colchicum to be called "theriaca articulorum."

Jane Reeves, æt. 28, was admitted into St. George's Hospital, under the care of Dr. Seymour, September 16, 1835, for rheumatism; which was confined to the joints and bursæ. Her complaints were of three months' duration, and the elbow and wrist-joint of the left hand, and the joint of the metacarpal bone, with the index finger, were painful, swollen, red, and there was distinct fluctuation present. The pulse was quick and feeble; the catamenia regular; bowels open. She was ordered

> Vini Radicis Colchic. ℥ss; Magnes. Ustæ, ℈ss.; Mist. Camph. ℥x. M. haustus ter die sumend.

> ℞ Extract. Aceti Colchici, gr. iij.; Pulv. Ipecac. C. gr. v. M. Ft. Pilulæ, ij. æquales mane et vespere sumend.

On the 20th, four days after admission, all pain, swelling, redness, and tension of the affected parts, had disappeared, and the patient laboured under the constitutional affection produced in some individuals by colchicum. She had frequent vomitings, some sensation of syncope, and general distress. The vinum colchici was intermitted, but the pills were continued, in order to prevent a return of the complaint, and she was ordered the following diffusible stimulant:—

℞ Sp. Ammon. Succin. ♏xv.; Mist. Camphoræ, ℥iss. M. haustus bis in die sumend.

On the 30th, the swelling, effusion, and pain, again appeared in the index finger-joint, and also (in a less degree) in the wrist, and was attended with much pain.

The use of the wine of the colchicum was resumed as before.

In four days the whole of the symptoms had again disappeared, but she was retained in the hospital upwards of a fortnight, that no doubt might be entertained of her recovery, when she was dismissed quite well.

Dr. Seymour remarked, in his clinical lecture, that although the vinum colchici had appeared to be the most effectual preparation in the present case, it was by no means certain that this would be the case in other examples; that he had seen cases in which it had failed, and the acetous extracts been very effectual; and again, cases in which both had failed, and the inspissated juice (succus spissatus colchici) had cured severe forms of the disease. Thus, not only was the remedy itself almost proverbially uncertain, but the preparations were also liable to great variation; the vinous tincture agreeing best with some, the extracts best with others; and that therefore we ought not to suppose colchicum to have failed after having employed fruitlessly one of the preparations.

The following case also, in its courses, shows at least the auxiliary advantage of colchicum over effusion into the joints, accompanied by acute pain. The case is also remarkable on other accounts, presenting the rapid transition of one disease to another, which has been so well described in the late Dr. Ferriar's paper on the Conversion of Diseases.

Maria Lum, æt. 14, was admitted into St. George's Hospital, under the care of Dr. Seymour, August 19, 1835. The patient was much reduced by indigence and illness. She was thin, weak, pale, and affected by a constant irregular action of the voluntary muscles, so as to constitute an aggravated case of chorea sancti viti. She articulated with difficulty, owing to the spasmodic protrusion of the tongue; and her pale, unhealthy, emaciated appearance, was very striking. She had suffered, it was reported, from rheumatism. Her bowels were confined; catamenia *nondum apparuére*; pulse feeble and small; no heat of skin; tongue clean. The patient was purged, and then put on a course of medicine which experience has shewn to be most effectual in similar cases—the preparations of steel, with purgatives. She was ordered

T. Ferri Ammon. ʒj.; Ammon. Subcarb. gr. iij.; Mist. Camph. ℥ij. st. haust. b. d. sumend.

℞ Pil. Aloes c. Myrrh, gr. viij.; Ft. Pilulæ, ij. alternis noctibus sumend.

She was ordered milk diet, and subsequently ordinary diet (four ounces of roast meat daily).

On the 26th, little amelioration having occurred, she was ordered, instead of her draught,

Mist. Ferri C. ℥j. b. d. and the use of the shower-bath.

Some advantage appeared to be derived from this plan; still she became occasionally worse; and, on the 17th of September, the preparation of steel was changed to the subcarbon ferri; ℈j. of which was taken three times daily.

On the 23d Sept. a very remarkable change had taken place; the patient was comatose, the pulse extremely quick and feeble, the pupils dilated, and the head rolled back, as in cases of organic disease of the brain. She was insensible to external stimuli; the urine scanty, with occasional rigid spasms of the lower extremities. Dr. Seymour entertained no doubt of effusion existing in the brain; he ordered as follows:—

Abrad. Capellities. Applic. Emplast. Cantharid. capiti raso, et exhibeat Ung. Hydrarg. parti vesicatæ.

25th.—The spasms of the extremities much less frequent. Pulse quick and small, irregular; skin hot; still senseless; urine scanty.

℞ Tinct. Cantharidis, ♏xxx.; Infus. Digitalis, ʒiij.; Aq. Menth. Pip. ℥x. M. ft. haustus ter die sumend.

29th.—A considerable discharge of urine has occurred; answers questions indistinctly. Pulse 110, very weak, but regular.—P.

On the 3d of October she was sensible, answered questions rationally, and the involuntary spasms of the muscles, so severe at the commencement of her illness, had nearly ceased. The urine continued to be abundant; she complained of some pain on the right side of the head.

Applic. Hirudines vj. pone aurem dextram.

On the 12th the leeches were repeated, from a return of pain.

She continued the diuretic mixture until the 12th October, when it was omitted. The spasms of the voluntary muscles had entirely ceased.

On the 21st a new set of symptoms presented themselves. The right hip and knee were much swollen, tense, full of fluid, but without redness; and the pain was so intense that she could not move the limb without screaming, nor bear it to be touched without suffering as severe pain as in disease of the hip-joint. Pulse 120, but without greatly increased heat of skin; bowels open. She was ordered—

Vini Colchici, ♏xx.; Magnes. Ustæ, gr. x.; Mist. Camph. ℥x. M. ft. haust. ter in die sumend.

℞ Subm. Hydr. gr. iij.; Opii, gr. ½. Ft. pilula alternis noctibus sumend.

22d.—Moves the leg more easily, and with less pain. Pulse 110; skin warm. —P.

24th.—Pain in hip and knee very greatly relieved; some effusion into the wrist-joint and bursæ of the left hand. No pain of head.

27th.—There were no remains of effusion, or swelling, or pain, in any of the joints. Some sickness from the colchicum. Bowels open.

Inter. omnia.

Since this time the patient has gained strength; no symptom remains of any of her complaints.

It has often been observed, that in the rare instances of metastasis of rheumatism to the brain, it has been when the disease has affected the joints and bursæ. In this instance there appeared to be a connexion between the effusion into the brain and effusion in the joints.

The following is an example of an aggravated case of chronic bronchitis, cured by the employment of the balsams, once so much used in the treatment of pulmonary consumption, and which fell into oblivion after the pamphlet of Dr. Fothergill, but which are so much recommended by analogy with the fluxes of other mucous membranes:—

William Lloyd, æt. 18, was admitted with the following symptoms:—Cough, with frequent puriform expectoration of a greenish colour, and very adhesive. Pulse 90; tongue furred; dyspnœa; impossibility of lying down in bed. Bowels costive; urine scanty and high-coloured; no dropsy. Has been addicted to drinking; is much emaciated; attributes his complaints to cold.

℞ Balsami Peruviani, ʒj.; Mucilag. G. Acaciæ, ʒij.; Syrupi Papav. Alb. ʒj.; Mist. Amygdalæ, ℥x. M. ft. haustus ter die sumend.

12th.—He was ordered to be purged more briskly with calomel, followed by H. sennæ, and then to resume the use of the Peruvian balsam.

19th.—Expectoration was reported to be diminished. Pulse 100. Sleepless nights from coughing.

Acet. Morphiæ, gr. ½; Oxymell. Simplicis, ʒij.; Aq. Fontanæ, ℥x. Ft. haustus h. s. s. Fish diet.

23d.—Expectoration diminished; lies down in bed, and on either side, without difficulty.

29th.—A pint of porter was added to the ordinary diet (℥iv. of meat), it being remembered that he had been in the habit many years of drinking hard.

The expectoration went on now gradually diminishing. When admitted he expectorated more than *a pint* daily, of green, viscid, puriform matter, without any apparent admixture of saliva, such as is seen when pneumonia is ceasing under appropriate remedies, or, as in this case, in old and very extensive chronic inflammation of the mucous membrane of the bronchi.

Dr. Seymour stated in his clinical lecture, that he had seen most striking cases of recovery, even where hectic fever and great emaciation were present, from the use of this medicine, which appears to act much in the same way as balsam copaibæ, or the turpentines, act in inflammation of the mucous membrane of the urethra.

CONDUCT OF THE NEW POOR LAW FUNCTIONARIES TOWARDS THE MEDICAL PROFESSION.

TREATMENT OF THE SICK POOR—SUGGESTIONS TOWARDS A REMEDY.

To the Editor of the Medical Gazette.

SIR,

THE favourable notice which you have bestowed on my letter, published in the *Times*, induces me to trouble you with some further remarks on the subject. Indeed, at a time when not only the sick poor and the country practitioners are driven to *feel*, but the gentry and clergy of the rural districts are compelled to *see*, the urgent necessity for some alteration in the provisions for parochial medical relief, there cannot, I imagine, be too ample an investigation, or too free a discussion, either of the evils alleged to exist, or of the remedies needed.

It is really a matter hedged round with difficulties. *Pauperism* is always an unwelcome topic; and never more so than when it wears the garb of *sickness and disease.* It therefore requires an effort, and a *strong* one, to induce the community to pay that attention to it which humanity and right reason dictate.

That it has hitherto been grossly neglected by *all* parties, is a sad truism; and particularly that the framers and functionaries of the Poor Law Amendment Act have blinked and slighted it, is but too plain.

If, however, the skilful medical treatment of *more than* 1,000,000 (! !) cases of sickness, occurring annually in the pauper population of this country, be worth consideration, it is surely not too much to expect that the legislature should interfere to provide in future proper attention to the wants of these numerous sufferers.

The Poor Law Commissioners, indeed, seem to be peculiarly unfitted for giving accurate and liberal consideration to the subject. Holding their offices on the "*expressed or implied condition* *" of saving to the greatest possible extent any expenditure on the wants of the poor, it is not surprising that they should act wisely only in those sections of their labours where the connexion between *extravagance of outlay* and *aggravation of pauperism* is proved manifestly to exist. It would be expecting that they should possess the eyes of Argus, or the two faces of Janus, to suppose that, while watching with jealous intentness every penny bestowed on the wretched paupers, they should discover some circumstances where an increase of expenditure might not only be *just* to the sufferers, but *ultimately beneficial* to the community.

And even to lay aside an *à priori* supposition, it is quite sufficient to look at the arguments of these gentlemen, on medical relief, contained in their last Report, and in the itinerant declamations of Sir F. Head, Mr. Gilbert, Mr. Mott, and (*proh pudor!*) *Dr*. Kay, &c. &c., to find that they are utterly incapable of taking an enlarged and philanthropic view of the subject.

The "Parish Doctor," exposed to the summer's sun and winter's blast, toiling by night and day, on a pittance that a foot-boy would spurn, appears to be as fair an object of their animadversion (and, I regret to say, unwarrantable vituperation), as the overseer who was accustomed to pay his own bills from the rates, or the able-bodied impostor, who bullied the frightened farmer into a handsome weekly allowance for doing nothing.

I am not, therefore, unreasonable in asserting, that, to organize satisfactorily pauper medical attendance, it is absolutely necessary that *parliament should interfere*; and, to suggest the leading particulars for legislation in this matter, is my present object in writing to you.

The evils mentioned in my former letter will readily direct the minds of the readers to some alterations, but the *modus* by which the improvement is to be effected, with a *due regard* to the working of the present measures for diminishing pauperism, requires more careful consideration.

It appears to me, firstly, that a *fixed* remuneration for the medical officers of parishes is necessary: without this, the evils of "tender" must continue, and the unworthy avarice of one party, with the infatuated speculation of the other, must remain unchecked.

Secondly, *the remuneration so fixed* must be calculated for *each parish separately*; otherwise the shameful monopoly of numerous parishes by one medical man will still flourish.

Thirdly, the sum paid by each parish must be made to vary with the variation of pauperism; and if (according to the hopes of the country) the burden of poor-rates should gradually become a trifling one, so must the medical salary adapt itself to the reduction.

Fourthly, this salary must be in proportion to the labour of the medical attendant in each parish; and this will, of course, depend, 1st, on the *distance* of the parish from his residence, and 2dly, on the *number* or *density* of the population of the parish.

The item of *distance* in the calculation, is, indeed, a most important one; without it, there would be no limit to the appointment of surgeons residing at a great distance from their poor patients, and no pecuniary check to depriving the sick of that speedy and easily-obtained assistance so necessary for their well-doing.

Fifthly, the remuneration should be such as will *defray the cost of a proper supply of medicines*, &c. &c. and will also afford a moderate surplus proportioned to the exertions of the attendant.

Such are the principles on which a scale for the salaries of parochial surgeons should be calculated, nor do I think the calculation would be a difficult one: there are sufficient data to act upon, and I hope that the Committee to which you allude, in your leading article, will propose one satisfactory both to the profession and the public. *Otherwise* government might appoint a commission, composed of an equal number of *laymen* and *professional* men, to compile a fair and equitable calculation for these payments.

That the above plan would remove many of the evils connected with the pre-

* Vide Report of the Commissioners on Medical Relief, August 1835.

sent system, will hardly be denied; but there are *other evils*, for which *other remedies* must be provided. As I have already occupied, I fear, an undue space in your journal, I will, with your permission, defer the consideration of *these* till the next week. In the meantime,

I remain, sir,
Your obedient servant,
RURICOLA.

November 9, 1835.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN OCTOBER, 1835.

Arthur Philbott, Hookey, Wells.
Samuel Radcliffe, Leeds.
Edman Scott, Wallington, Oxon.
Adolphus Barnett, Commercial Road.
George Newman, Glastonbury.
Samuel Lees, Ashton-under-Line.
George J. M. Wilson, Greenhithe.
John Paterson, Aberdeen.
Charles Upton, Chapel-street, Grosvenor Place.
Edward S. Cuming, London.
Harold Giles, Margate.
Douglas Dutton, Upper York-street.
George W. Bore, Isle of Wight.
Campbell G. De Morgan, Upper Gower-street.
Wm. P. H. Eales, Kingsbridge, Devon.
Benjamin K. Brydges, London.
Arthur Brebner, Jamaica.
George Cooper, London.
William Rose, Wicklam, Bucks.
John Tallon, Dundalk.
Charles Carruthers, Holbeach.
Zechariah Lindo, Bishopsgate-street.
James H. Shirreff, Deptford.
Thomas Wilson, Congleton.
Charles Draper, Kenilworth.
David Prothero, Llandiloes.
Jas. Gilbert, Norfolk-street, Middlesex Hospital.
Simeon W. Hardy, Cork.
Peter Scott, Dundalk.
Noble Seward, Limerick.
C. W. F. Hunter, Kinsale, Cork.
Samuel Marshall, Marnham, Notts.
John Chippendale, London.
W. B. Carpenter, Bristol.
Walter C. Poole, Yanghall, Cork.
John Swan, Castle Durrow, Kilkenny.
Arthur Scott, Canada.
Andrew Durham, E. I.
John C. Tice.
John Friend, Rainham, Kent.
F. P. Pascoe, Penzance.
John R. Philipps, Aberystwith.
John A. Field, Milford.
Henry C. Walton, Dundrap, Dublin.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

October 29, November 5 and 12, 1835.

Edwin Bennett, Manchester.
James Carter, Witham, Essex.
James Gibson, Hull.
Joseph Frederick Handley, Chipping Norton.
Fredereck Le Mesurier, Guernsey.
John Nigel Heathcote, Newcastle.
William Tarleton, Henley in Arden.
F. W. Willsford, Chelsea.
Joseph Simons, Ullesthorpe.
William Bisdon, Dolton, Devon.
Theophilus Goate Gurdon, Hadleigh, Suffolk.
William Roley Lomas, Belper.
William Lord, Farringdon, Berks.
John Daniel Alty, Ormskirk.
Robert Bacon, North Walsham.
Henry Merford, Bath.
William Berwick Clarke, Beekermoat.
Peter T. Kempson, Birmingham.
William Sheldon Sweeting, Bridport.
John James Mason, Cheltenham.
Robert Archibald, Hereford.
Giles Richard Burt, Bridport.
Alfred Atkins, Babbycombe.
John Moorhouse, Sheffield.
William Dunning, Hull.
Thomas Cooper Temple, Cliburn, Westmoreland.
Arthur Newnham, Alton, Hants.
Robert Harland Whiteman, Lewes, Sussex.
John Bowes, Richmond, Yorkshire.
Eleazar Meldola, London.
John Angus Graham, Leeds.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Nov.* 10, 1835.

Abscess	1	Inflammation	34
Age and Debility	45	Bowels & Stomach	1
Apoplexy	9	Brain	2
Asthma	20	Lungs and Pleura	7
Cancer	2	Insanity	6
Childbirth	12	Liver, diseased	13
Consumption	53	Measles	17
Convulsions	38	Mortification	3
Croup	3	Paralysis	1
Dentition or Teething	10	Scrofula	3
Dropsy	11	Small-Pox	20
Dropsy on the Brain	18	Sore Throat and Quinsey	1
Dropsy on the Chest	1	Spasms	1
Epilepsy	1	Thrush	1
Fever	12	Tumor	2
Fever, Scarlet	3	Worms	1
Fever, Typhus	2	Unknown Causes	2
Fistula	1		
Gout	2		
Hernia	2	Stillborn	14
Hooping Cough	8		

Decrease of Burials, as compared with the preceding week . . . 143

METEOROLOGICAL JOURNAL.

Oct. 1835.	THERMOMETER.		BAROMETER.	
Thursday . 29	from 39 to 55		29·85 to 29·98	
Friday . . 30	28	47	30·11	29·95
Saturday . 31	44	57	29·85	29·88
Nov. 1835.				
Sunday . . 1	36	46	30·07	30·14
Monday . . 2	28	47	30·15	30·09
Tuesday . . 3	42	48	29·99	29·91
Wednesday 4	38	43	30·00	29·97

Prevailing winds, N.W. and S.E.
Generally cloudy, with frequent showers of rain.
Rain fallen, 1 inch and ·225 of an inch.

Thursday . 5	from 34 to 42		29·93 to 29·87	
Friday . . . 6	26	43	29·84	29·89
Saturday . 7	26	47	29·97	29·91
Sunday . . 8	37	48	29·85	29·94
Monday . . 9	36	41	30·00	30·14
Tuesday . 10	32	40	30·22	30·30
Wednesday 11	30	44	30·31	30·24

Prevailing winds, N.E. and N.W.
Except the 8th, generally cloudy; rain on the evening of the 7th, and morning of the 9th; also a few drops on the morning of the 11th.
Rain fallen, ·325 of an inch.

CHARLES HENRY ADAMS.

ERRATUM.

In Mr. Pereira's lecture in last No., p. 166, *for* "D.—Medicines that act specifically on the *uterine* system," *read* "medicines that act specifically on the *nervous* system."

WILSON & SON, Printers, 57, Skinner-St. London

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 21, 1835.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE VIII.

CHARACTERISTICS OF THE TWO GREAT KINGDOMS OF NATURE.

GENTLEMEN,—Having already announced my intention of arranging the articles of the materia medica in natural historical order, I proceed in this lecture to give you an outline of the leading divisions which it will be necessary to make.

The terms nature and universe are frequently used as synonymous: and hence, under the head of natural bodies would be included the planets, fixed stars, &c. It is usual, however, to restrict the terms, so as to comprehend the terrestrial bodies only; which, then, are the objects of natural history and of natural historical classifications. Formerly they were divided into three groups, called kingdoms—the mineral, the vegetable, and the animal; but this division has, of late years, been for the most part given up, on account of the impossibility of so characterizing the two latter as to distinguish them from each other. To obviate this difficulty, the two have been formed into one, under the name of the *organized* or *living* kingdom, while the mineral is now called, in contradistinction, the *inorganized* kingdom.

But it has been asserted by some, that no more distinction exists between the organized and the inorganized kingdoms, than between vegetables and animals. There is a group of the latter called, by some of the German zoologists, the *lithozoa* or *stone animals*; on account of their supposed resemblance to a mass of mineral matter. In these the skeletons are external, or cutaneous, and consist of carbonate of lime (sometimes with a little phosphate) agglutinated by gelatinous matter. Here are specimens of these calcareous masses [shewing some madrepores and millepores], which have been supposed to connect the animal with the carbonate of lime of the mineral kingdom. But you will observe that the calcareous masses of the lithozoa are porous, and in the recent state contain fleshy tubes (called polypes) constituting the soft parts of the animal: a structure, nothing analogous to which is found in the mineral carbonate of lime. In the nullipora the pores are not evident, and hence these masses have been supposed to form the closest relation to minerals.

Vegetables also have been stated to be closely related to animals. Here is a drawing of the *Diatoma vulgaris* (fig. 24),

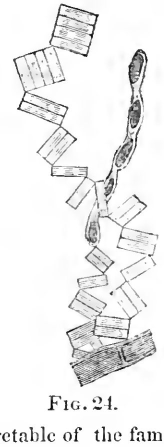


FIG. 24.

a little vegetable of the family *Algæ*; it varies in its form, and in the mode of connexion of its parts. At one period of its existence it is cylindrical, at another it is composed of quadrangular segments, sometimes connected by their sides, at others by their alternate angles. You will observe that these segments have somewhat the forms and appearance of crystals; and Agardh has, in consequence, fancied they form a distinct passage from the vegetable to the mineral kingdom; but their

active properties, the changes they undergo at the different periods of their existence, sufficiently distinguish them.

That vegetables and animals approximate and gradually pass into each other, I have already stated. The German zoologists have a class called *Phytozoa*, including all those animals which resemble plants. In some cases, animals resemble flowers in their appearance; as the different species of *actinia*, or sea anemones (fig. 25).

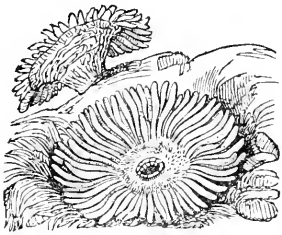


FIG. 25.

On the other hand, vegetables have oftentimes a close relation, in their external characters, to animals. Observe these *algæ* (pointing to some drawings): here are the oscillarias; vegetables having a cylindrical form analogous to the common earthworm, and, like it, also presenting a jointed appearance, from the junction of the segments composing its interior tube. Now these beings are endowed with the power of motion, as if voluntary, like animals. We have depicted *tendaridea pollux* (fig. 26) as an example of the tribe of plants called *conjugatæ*, on account of their having a kind of copulation. Two of these tubes approach each other, and become connected: we then observe that the green-colouring matter (which in each has a star-like form) passes from the joint of one into the joint of the other, and forms there a roundish gemmiform body, which subsequently becomes a new being. In the *zoocarpeæ* we have beings in which the vegetable and animal states

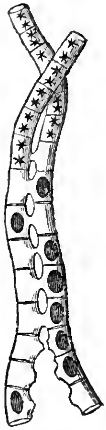


FIG. 26.

appear to succeed each other. Here is the *anthophysis dichotoma* (fig. 27), the tubular

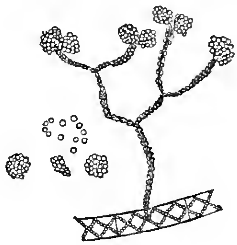
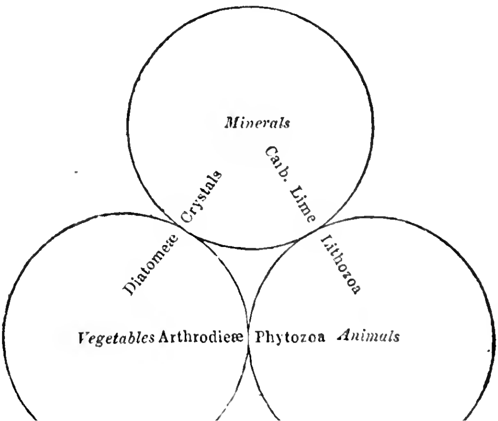


FIG. 27.

filaments of which produce, at their extremities, little globules, which, when detached from their support, possess a locomotive power, and (according to Bory St. Vincent) become a multitude of zoocarpeæ, or animated monads.

We see, thus, in what way it has been supposed the mineral, vegetable, and animal groups approach each other: and may adopt the mode of illustration as represented by the following diagram:—

FIG. 28.



The chief supporters of this opinion are Robinet and Schweigger. I must refer you to their works for further information, as also to Tiedemann's Physiology.

I propose, in the next place, to point out those characters which distinguish the organized from the inorganized kingdom, under three heads:—

1. Differences in chemical composition.
2. Differences in external and internal forms and arrangement of parts.
3. Differences in the actions, or motions.

I propose also to arrange them in a tabular form, so that the contrast between the two kingdoms may be instantly perceived.

1. Differences in Chemical Composition.

Inorganized Bodies.	*Organized Bodies.*
1. The undecomposed or elementary substances are fifty-four in number — namely, oxygen, chlorine, bromine, iodine, fluorine, hydrogen, carbon, nitrogen, phosphorus, sulphur, selenium, boron, silicon, potassium, sodium, lithium, magnesium, calcium, barium, strontium, aluminum, glucinum, zirconium, yttrium, cerium, thorium, tellurium, arsenicum, antimony, chromium, uranium, molybdenum, tungsten, columbium, titanium, gold, silver, platinum, palladium, rhodium, iridium, osmium, mercury, bismuth, copper, tin, lead, cadmium, zinc, cobalt, nickel, manganese, iron, vanadium.	1. The undecomposed or elementary substances are about twenty — namely, oxygen, chlorine, bromine, iodine, fluorine, hydrogen, carbon, nitrogen, phosphorus, sulphur, silicon, potassium, sodium, calcium, magnesium, aluminum, iron, manganese, gold, and copper. Of late it has been asserted that titanium is present in the renal capsules. Of the above-mentioned twenty elements in organized beings, only three or four are met with in considerable quantity — namely, oxygen, hydrogen, carbon, and nitrogen.
2. Are sometimes simple, or elementary; and, when compound, sometimes contain only two elements.	2. Are never simple. Every living part contains three or four elements at least, and frequently more.
3. The compound atoms are capable of decomposition and of recomposition.	3. Organized or living parts may be decomposed, but cannot be recomposed.
4. Are incapable of undergoing fermentation and putrefaction.	4. Are capable of undergoing fermentation and putrefaction.

These I conceive to be the leading chemical peculiarities. There are, however, several points which perhaps I should have added to these; but I have not done so, since they involve some hypothetical details. Thus Berzelius says that the inorganic *compound atoms of the first order* are always *binary;* whereas those of organic bodies are *ternary*, or *quaternary*. Water and sulphuric acid, for example, are each binary compounds; the first containing hydrogen and oxygen, the second oxygen and sulphur. It is, however, assuming too much to say that the compound atoms of organized beings, which are called *organic principles*, are always ternary, or quaternary. It is a fact that sugar consists of

Carbon, 6 parts by weight, or equal to	1	atom.
Hydrogen, 1 part do. do.	1	do.
Oxygen, 8 parts do. do.	1	do.

But it does not follow that it is a ternary compound: it may be a double binary compound of water and carbon. Thus an atom of sugar (an organic product) may be considered theoretically either way:—

$$\left.\begin{array}{lr} 1\text{ water} \ldots\ldots & 9 \\ 1\text{ carbon} \ldots & 6 \\ \hline & 15 \end{array}\right\} \text{ or, } \begin{array}{lr} 1\text{ hydrogen} \ldots & 1 \\ 1\text{ oxygen} \ldots\ldots & 8 \\ 1\text{ carbon} \ldots\ldots & 6 \\ \hline & 15 \end{array}$$

Several bodies which are composed only of two elements—namely, of carbon and hydrogen—are organic products: and although it is true, as mentioned before, that organized matters have three or four elements, yet all organic substances have not; for example, the stearoptene of the oil of roses. Berzelius has offered another distinction equally theoretical. He asserts the electro-chemical states of the compound atoms of inorganized bodies depend on the nature of their elements; while those of organized bodies do not. The following table will illustrate his views:—

	Hydrogen.	Carbon.	Oxygen.
100 parts of Sugar consist of	6·6	39·9	53·3
100 parts of Acetic Acid consist of	4·	48·	48·
100 parts of Oxalic Acid consist of	0	33·3	66·
100 parts of Carbonic Acid consist of	0	27·27	72·72

Now you will observe that sugar, which consists of oxygen united to hydrogen and carbon, possesses neither acid nor basic properties; or, at least, it possesses them in so feeble a degree, that they are only developed under particular circumstances. But acetic acid, which is composed of the same elements, is a powerful acid; and, therefore, we should expect, *à priori*, to find its oxygen (the most energetic electro-negative of its elements) in larger quantity than in sugar, whereas the reverse is the case. Again, carbonic is a very feeble acid, although it contains more oxygen than the oxalic acid, while the latter is more powerful. At one time I felt disposed to adopt Berzelius's opinion, that the peculiarities in these substances were referrible to their organic source; but as we meet with peculiarities equally great in bodies which are not organic, this doctrine seems now untenable. Two or three bodies (both inorganic) may be identical in composition, though different in properties. Thus there are three acids, all composed of—

$$\left.\begin{array}{lr}2\ \text{Phosphorus}\ldots\ldots & 32\\ 5\ \text{Oxygen}\ \ldots\ldots & 40\end{array}\right\}70$$

Yet each possesses peculiar properties, and hence they have been separately named: they are phosphoric, pyro-phosphoric, and meta-phosphoric acids. Such substances are said to be *isomeric* (from *ισος*, equal, and *μερος*, part.)

Let us now proceed to examine the differences in the external and internal form and arrangement of organized and inorganized bodies.

Differences in External and Internal Form and Arrangement.

Inorganized Bodies.	*Organized Bodies.*
1. The volume is variable and indefinite.	1. The volume is variable, but definite. Thus though men vary in their size, there is a limit to the variation.
2. The exterior form may be regular or irregular. When regular, these bodies are bounded by straight lines, and have angles; in other words, the *regular form* of inorganised bodies is *crystalline*.	2. The exterior form is regular, and generally rounded. To this, however, we have exceptions in some of the *Diatomeæ*, which, at one period of their existence, are angular.
3. The internal arrangement of the parts, or the *structure*, may be regular or irregular. If regular, it is crystalline.	3. The internal arrangement of the parts, or structure, is regular. This regular arrangement is vesicular or tubular.
4. Are generally composed of solid, liquid, or gaseous matter, exclusively: some exceptions, however, to this statement exist. Dr. Brewster has discovered colourless and transparent liquids in some topazes, chrysoberyl, &c.	4. Always consist of both solids and fluids. *Musci* and *Polygastrica* may, indeed, be dried without destroying their vitality; and hence they may be said to form exceptions to this statement. But they are probably never so dried as to be wholly deprived of moisture; and, in the next place, in this dried state they give no sign of life; and it is only by moistening them that the vital phenomena become apparent.
5. The separate parts are generally homogeneous, though sometimes heterogeneous.	5. The separate parts are essentially heterogeneous; that is, they possess different properties.
6. The separate parts are independent of each other.	6. The separate parts are always related, and in some cases are dependent on each other. This subserviency of one part to the other constitutes what is called *organization*, the parts being called *organs*.

Such are the principal points which I think it necessary to mention under this second head of the differences between organized and inorganized beings. I now proceed to the third head—namely, differences in actions or motions.

Differences in Actions or Motions.

Inorganized Bodies.	*Organized.*
1. Are produced by the agency of cohesion and affinity, at the expense of other previously existing bodies.	1. Are produced by other similarly constituted bodies, which we call the parents. There are, indeed, many cases in which we cannot demonstrate the existence of a parent; but several reasons lead us to conclude there was one. Hence we adopt as a canon, the statement of Harvey, "*Omni virum ex ovo.*" Cohesion and affinity are insufficient to account for the production of organized beings, and hence we call to our aid another power, which we denominate "*the vital force.*"
2. The preservation or existence is effected solely by cohesion and affinity.	2. The preservation or existence is frequently in opposition to the usual operation of cohesion and affinity: hence we refer it to "*the vital force.*"
3. Are in a state of rest.	3. Are in a continual state of activity, taking in from the external world certain solid, liquid, and gaseous substances, assimilating them to their own proper fluids, and converting them into solids, or preparing peculiar liquids from them. These phenomena constitute the process called *nutrition.*
4. Undergo no alterations in their nature, except such as result from the operation of attraction and repulsion.	4. Are incessantly undergoing changes, so that at different periods of their existence their properties are somewhat different. Thus they increase, develop themselves, arrive at maturity, and lastly decrease. The phenomena of *age*, therefore, are peculiar to organized beings.
5. The active properties are always the same in degree and kind.	5. The active properties vary in degree and kind: the distinction of *health* and *disease* which depends on this variation is, therefore, peculiar to organized beings.
6. The existence is not necessarily limited or definite.	6. Exist only for a limited period. The cessation of their existence is denominated *death.* The oldest organized beings on our planet are probably the *Baobab* trees (*Adansonia digitata*); the ages of some of which have been computed to be five or six thousand years. Though there is, perhaps, great exaggeration in this calculation, yet these trees are undoubtedly very aged.
7. All their actions are referrible to repulsion and attraction.	7. Repulsion and attraction are insufficient to explain the phenomena of living beings: we therefore admit other properties, called *vital*, which we refer to *life*, or the *vital force.*

We see, then, that the peculiarities of living beings, in reference to their actions or motions, are connected with their *origin*, their *development*, their *preservation*, and their *end.*

In considering the articles of the materia medica, it matters little whether we commence with those derived from the organized, or those from the inorganized kingdom. On the present occasion, I shall commence with those derived from the former.

Medicines derived from the Organized Kingdom.

This division includes two great groups, the animal and the vegetable, but which cannot be absolutely separated from each other. I propose to commence with the animal, and hereafter, when speaking of the vegetable, to throw into a tabular form

the principal characters which distinguish one from the other.

Animal Materia Medica.—The animal substances used in medicine were formerly much more numerous than now. At the present time they are far inferior, both in number and value, to either vegetable or mineral substances. The album græcum, mummies, the cranium hominum raspatum, viper's fat, &c. have long since ceased to constitute a part of our materia medica. With the exception of cantharides and leeches, we could easily dispense with the whole of them.

Zoological Classifications.—Although not very numerous, yet animal medicines require to be spoken of in some systematic order. Now many methods present themselves; we may, as some authors have done, divide them into whole animals, parts of animals, animal secretions, &c.; or we may arrange them according to the natural historical properties of the animals from whence they are obtained. I shall adopt the latter mode.

Now a natural-historical method must be founded on the properties of the bodies classified. But all the properties of an animal are not equally serviceable for our purpose; for some are accidental, some are temporary, others are variable, and comparatively only a few are so constant and invariable as to be available in natural history. The latter are *external form* and *structure*, and from the use we make of them, we call them *natural-historical properties.*

I ought, however, to premise, that although we talk of a natural arrangement, yet the only absolute and invariable distinction established by nature is that of *species.* But species possess so many characters in common, that it is impossible to distinguish them by any single trait of their organization; and thus, in order to diminish as much as possible the number of characters requisite to distinguish each species, naturalists have established groups, each containing those beings which are most analogous. Thus a group of species constitutes a *genus,*—the combination of analogous genera is termed a *tribe* or *family,*—the reunion of tribes forms an *order,*—while a *class* is the combination of orders.

The first systematic classification of animals was that of Linnæus, in 1735; and was as follows:—

			Classes.
Animals	having a heart with two auricles and ventricles: blood red and warm	viviparous	1. Mammalia.
		oviparous........	2. Aves.
	heart with one auricle and ventricle: blood red and cold......	having lungs	3. Amphibia.
		having branchiæ..	4. Pisces.
	heart with one ventricle: blood white and cold	having antennæ ..	5. Insecta.
		having tentacula .	6. Vermes.

The present course of lectures is not adapted for pointing out the defects of this arrangement; it is sufficient for me to state, that so early as 1795 Cuvier did this, in a *Memoir on Animals with White Blood;* and three years subsequently he proposed a new classification, the basis of which has been adopted by a large majority of naturalists throughout Europe. The following is a sketch of its leading divisions:—

General forms.

I.—Vertebrata.

Classes.

1. Mammalia.
2. Aves.
3. Reptilia.
4. Pisces.

II.—Mollusca.

5. Cephalopoda.
6. Pteropoda.
7. Gasteropoda.
8. Acephala.
9. Brachiopoda.
10. Cirrhopoda.

III.—Articulata.

11. Annelida.
12. Crustacea.
13. Arachnida.
14. Insecta.

IV.—Radiata, or Zoophyta.

15. Echinodermata.
16. Intestinalia.
17. Acalepha.
18. Polypi.
19. Infusoria.

Modifications of this arrangement, some of which are of considerable importance, have been made by different writers. Here are two—one by Goldfuss, the other by my able colleague, Dr. Grant.

Goldfuss's Arrangement.

XI. Mammalia.
X. Aves. IX. Reptilia.
VIII. Pisces.
VII. Mollusca.
VI. Insecta. V. Polymeria.
IV. Radiaria.
III. Annulata. II. Enthelmintha.
I. Protozoa.

Dr. Grant's Arrangement.

I. *Spino-cerebrata*, vel *Vertebrata.*

1. Mammalia.
2. Aves.
3. Reptilia.
4. Amphibia.
5. Pisces.

II. *Cyclo-gangliata*, vel *Mollusca.*

6. Cephalopoda.
7. Pteropoda.
8. Gasteropoda.
9. Conchiphera.
10. Tunicata.

III. *Diplo-neura*, vel *Articulata.*

11. Crustacea.
12. Arachnida.
13. Insecta.
14. Myriapoda.
15. Annelida.
16. Cirrhopoda.
17. Rotifera.
18. Entozoa.

IV. *Cyclo-neura*, vel *Radiata.*

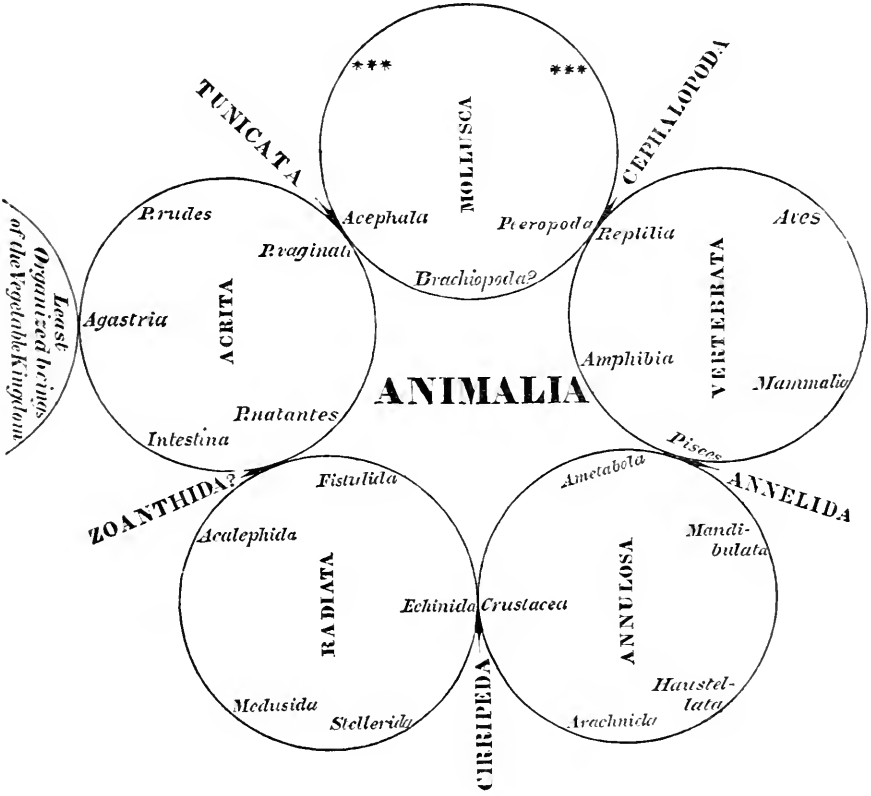
19. Echinoderma.
20. Acalepha.
21. Polypiphera.
22. Poriphera.
23. Polygastrica.

You will observe that the number, names, and position of the classes, differ from those of Cuvier's classification.

I cannot resist alluding to the *Quinary system*, proposed by Mr. MacLeay, in his *Horæ Entomologicæ*, being totally different from the Cuvierian method, and having obtained considerable celebrity of late years in this country. Every natural group is supposed by him to be resolvable into five natural groups, and each of these again into five other groups, and so on to the end of the term. Now this supposition, that nature always proceeds by fives, seems to me the most objectionable part of the arrangement. Mr. MacLeay imagines each group returns into itself, and he expresses this distribution by circles.

The following are his primary divisions, [Fig. 29]:—

Fig. 29.



You will observe that in Mollusca two classes are still wanting to complete the quinary arrangement of that sub-kingdom. The divisions acrita and radiata have been remodelled by Mr. Richard Owen.

For a further account of this classification, I must refer you to Mr. Mac Leay's work, or to the Report on Zoology of the fourth meeting of the British Association in 1834.

Schweigger, Oken, Blainville, Fleming, and others, have proposed new classifications, but in these lectures it would be improper to enter into any details respecting them. I shall follow the most generally adopted classification—namely, that of Cuvier, but adopting the modifications of Dr. Grant.

Arrangement of animals.—Some animals have a brain and spinal marrow: when this is the case, these parts are lodged in a bony cavity. These animals are the *Vertebrata* of several authors,—the *Spini-cerebrata* of Grant: for example, quadrupeds, birds, and fishes.

Animals having no spinal marrow are destitute of an internal skeleton: the nervous system possesses no osseous cavity in which it is lodged. These are the *Invertebrata* or *Evertebrata* of authors. They include the *Molluscous*, or soft animals, as shell-fish, slugs, &c.; the *Articulata*, or jointed animals, as insects and spiders; and the *Radiata*, or Zoophytes, as corals, star-fish, &c.

Animalia	Vertebrata.	
	Invertebrata	Mollusca.
		Articulata.
		Radiata.

Division 1. *Vertebrata*, or *Spini-cerebrata*.—They have an internal osseous skeleton, generally covered by muscles; their nervous system consists of brain, spinal marrow, ganglia, and nerves, the two first contained in an osseous envelope formed by the cranium and vertebræ. The blood is red; the heart muscular, with at least two cavities. The senses are five, the external organs for four being lodged in the head. The body is symmetrical, the limbs not exceeding four. The sexes separate. They are divided into five classes.

				Classes.
Vertebrata	hot blooded		viviparous: sanguineous globules circular	1. Mammalia.
			oviparous: sanguineous globules elliptical	2. Aves.
	cold blooded	in the adult state, respiration by lungs	heart with two auricles: squamiferous	3. Reptilia.
			heart with one auricle. In their early state have an aquatic respiration, and breathe by branchiæ: skin naked	4 Amphibia.
		respiration by branchiæ		5. Pisces.

Class I. *Mammalia.*—The animals of this class are born alive, and require a particular kind of food to nourish them in the first period of their infancy. Hence the mothers are supplied with *mammæ*, or breasts, for the secretion of milk; and from this circumstance these animals are called mammalia, or mammals. The bodies of most of them are furnished with hairs, and hence they have been called by Blainville, *Pilifera*. The characters of the class are the following: the blood is red and warm, having circular globules. The heart has two ventricles and two auricles, the circulation being double, that is, pulmonary and systemic. The respiration is by lungs, a diaphragm being present. We make nine orders of them:—

						Orders.	Examples.
Mammals	Piliferous: two pairs of members	unguiculated	three kinds of teeth	— hands	two	1. *Bimana*.	Man.
					four	2. *Quadrumana*.	Monkey.
				no hands	no abdominal pouch	3. *Carnaria*.	Dog.
					abdominal pouch	4. *Marsupialia*.	Kangaroo.
			not more than two kinds of teeth		no canines	5. *Rodentia*.	Beaver.
					no incisors	6. *Edentata*.	Sloth.
		hoofed	stomach simple (non-ruminants)			7. *Pachydermata*.	Pig.
			four stomachs (ruminants)			8. *Ruminantia*.	Musk
	Pisciform: naked skin: one pair of members					9. *Cetacea*.	Whale.

Order I. *Bimana.*—This order, which contains only one genus, *man*, is recognised by having hands (that is, extremities with thumbs free, and opposed to the fingers) on the upper extremities only.

Formerly, several substances obtained

from man were used medicinally. Even so late as the year 1782, in the fourth edition, by Schreber, of Linnæus's Materia Medica, we find mummies, the cranium hominis raspatum præparatum, human bones, and human fat, still placed among the articles employed as medicines. Thanks to the march of intellect, such disgusting and useless articles are now expunged from our catalogue of drugs. It is really quite amusing to read old Pomet's account of these articles. He first tells us how pretended mummies are manufactured, and then points out the characteristic marks of what he supposed to be genuine ones. "Choose," says he, "what is of a fine shining black, not full of bones or dirt, of a good smell, and which, being burnt, does not stink of pitch!" It seems also, according to the same authority, that the executioner at Paris was the grand purveyor of human fat "to those," as Pomet says, "who want it." Sometimes the *rock* or *stone lichen*, now called by botanists *parmelia saxatilis*, has been found growing on half-buried skulls: hence this was used in medicine under the name of the "*moss of dead men's skulls.*" Pomet tells us that the "English druggists generally bring these heads from Ireland, that country having been remarkable for them ever since the Irish massacre"

Without entering further into these absurdities, I may remark, that blood is the only article obtained from our species which is now employed as a remedial agent.

Transfusion.—The operation of transfusion consists in introducing the blood of one animal into the veins of another. It has been applied in the human subject to obviate the effects of hæmorrhage, and more especially that from the uterus. Although this operation will no doubt be brought under your notice by your surgical and obstetric teachers, yet there are one or two points to which I purpose drawing your attention.

Libavius, so early as the year 1615, speaks of this operation as practicable; and between the years 1660 and 1670, various experiments were made on the subject, by Lower, Denys, King, and others; but the practice fell into disuse. It must be recollected, however, that the old experimenters injected brute blood into the veins of man, instead of employing human blood; and that they resorted to the operation for the purpose of curing diseases, whereas it is now proposed as a remedy for hæmorrhage; and it is well established, that the blood of the same species only can be safely injected. This last fact has been proved by the experiments of Dr. James Blundell, to whom the profession is principally indebted for demonstrating the practicability of the operation.

It is universally known, that when animals are bled largely, they soon fall into a condition somewhat resembling death, and which is called syncope; and that if the quantity of blood lost be very great, *real* soon succeeds this *apparent* death. Now it has been proved that throwing into the veins a certain quantity of blood just drawn from another animal of the same species, will oftentimes avert the fatal event; but that if the blood of an animal belonging to a different, though neighbouring, species be employed, temporary resuscitation only is produced, the animal eventually dying. If the blood be taken from an animal whose structure, &c. is quite different from that of the animal who has lost the blood, temporary resuscitation even is not produced, but the blood thrown in seems to act as a poison.

These facts clearly prove that in each species the blood possesses peculiar properties. If we submit this fluid to a microscopic examination, we find it consists of a *serous liquid*, holding in suspension a number of *small, insoluble, and regular particles*, denominated *globules*, which seem to constitute the most essential part. Each of these has been described as consisting of two parts—namely, a *central corpuscule*, usually stated to be transparent or whitish, and a *membranous envelope*, having a gelatinous character, and generally said to possess a reddish tint. Now as these globules are peculiar to the blood, and as they vary in shape, number, and size, in different species of animals, it is probable that it is to their variations of characters we are to attribute the fact already stated—namely, that permanent resuscitation cannot be effected except by blood of the same species.

Confining our attention to vertebrata, we find in mammals the particles are circular; but in birds, reptiles, and fishes, elliptical (fig. 30). Observe the represen-

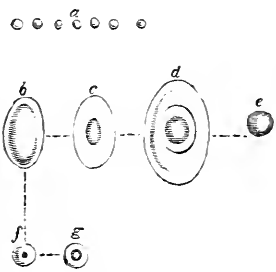


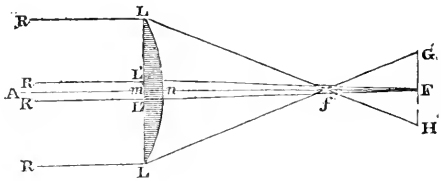
FIG. 29.

tation of the so-called globules of human blood (*a*), which are flattened circular discs, and not spheres, as their names would indicate. There are no central nu-

clei, as described by several writers; the appearances supposed to be nuclei are only optical illusions: such, at least, is Raspail's opinion. In confirmation of it we have the evidence of Dr. Hodgkin, who has been unable to find them; and I have likewise searched for them in vain by means of an excellent microscope belonging to Mr. Goadby.

The optical illusion is perhaps best explained by reference to what is called *spherical aberration*. The rays of light refracted by spherical surfaces do not all meet at one point or focus: those nearest the axis form a focus much more distant from the lens than those most remote from the axis. Let us take the case of a plano-convex lens, in which this is best seen:—

Fig. 31.



L L, The plano-convex lens.
A F, The axis of the lens.
R' L' and R' L, Rays near the axis, forming their focus at F.
R L and R L, Rays more distant from the axis, forming their focus at *f*.
The distance between *f* and F is the spherical aberration of the lens.

If we view a luminous body through such a lens as this,—at *f* we shall have the circumference well illuminated, but not the disc; while at F the phenomena will be the reverse: that is, the disc will be illuminated, but not the circumference. Now the globules of the blood, though not spherical, may produce an analogous effect on the rays of light: the rays of light passing through them will not meet at the focus, because of their thickness not being uniform, and thus the appearance of *f* or *g* (fig. 29) may be put on.

Here is a representation of the oval or elliptical particles of the frog (*b*). After being for a few minutes on the object-glass of the microscope, and diluted with a little water, they enlarge, and a kind of nucleus becomes evident (*c*, *d*.) The outer layer then disappears, leaving the nucleus (*e*), which also subsequently disappears, by dissolving, or undergoing some other change in the water. Such is the account given of these globules by Raspail; its correctness I have myself verified. He refers the nucleus here to the successive solution of the different layers of the albuminous globule.

A priori we might expect that elliptical globules could not with safety be employed to replace circular ones, nor, *vice versâ*, circular globules to replace elliptical ones. Now the experiments of Prevost and Dumas confirm this opinion. When blood having circular globules was injected into the veins of birds, it not only did not support life, but acted as a poison, causing immediate death, accompanied with symptoms demonstrating some lesion of the nervous system. When the blood of one species having circular globules was injected into another species also having circular globules, the death was not immediate, but took place after some time, in consequence, apparently, of imperfect nutrition.

The number of globules contained in a given quantity of blood varies in different species of animals, though, from their minuteness, it is perhaps impossible to ascertain accurately their relative quantities. Now it is not at all improbable that this variation may have an important influence on the animal; but as the size of the globules usually varies with their number, it is difficult to determine their separate influence. The following table shows the number of particles contained in 1000 parts of blood, according to the observations of Prevost and Dumas:—

	Man.	Calf.	Horse.	Dog.	Cat.
Globules	129·2	91·2	92·0	123·8	120·4
Water	783·9	826·0	818·3	810·7	795·3
Albumen and soluble salts	86·9	82·8	89·7	65·5	84·3
	1000·0	1000·0	1000·0	1000·0	1000·0

All the blood here referred to was venous, except that of the calf, which was a mixture of venous and arterial.

The size of the sanguineous particles varies in different species, and there is good reason for suspecting even in individuals of the same species, though this last observation is denied by Petit and Dumas, who state that the diameter is constant in the same species. Cavallo, however, says the diameter of the globules of human blood varies from three-ten-thousandths of an inch to four-ten-thousandths. Raspail goes beyond this, for he says they vary from two-ten-thousandths to four-ten-thousandths of an inch. Now if we admit these statements we have a ready means of accounting for the discordant results obtained by different observers. However, my principal object in noticing the variable size of the sanguineous globules in the different species, was to remark that probably it is to this circumstance, in part at least, that the blood of one species will not answer for transfusion into an animal of another species. The elliptical globules of the frog are considerably larger than those of man: Raspail says they are one one-thousandth, and that of the salamander are as much as thirteen-ten-thousandths of an inch, being the largest known.

Hitherto the cases in which advantage has been obtained from transfusion, are principally those of uterine hæmorrhage. In operating, great care must be taken to avoid the introduction of atmospherical air, for this fluid may cause death, by obstructing the pulmonary circulation; or if the quantity of air be larger, it becomes dilated by the heat, and thus distending the heart with frothy blood, gives rise to instant death, attended with convulsions. But it has been shown that a much larger quantity of air may be thrown with impunity into the veins more distant from the heart than had been previously supposed.

Another point deserving your particular attention is, that the blood should be injected immediately after it has been drawn from the body; for when it has been exposed to the air for some minutes, and probably before it has distinctly coagulated, it is unfit to carry on the vital functions.

It is remarkable that the quantity of blood to be transfused is very small compared with that lost. In one case, related by Dr. Waller, four ounces only were required; in another eight ounces. It will rarely, perhaps, be necessary to employ more than twelve ounces.

The operation may be readily performed by means of a three or four ounce syringe; or the common enema pump might answer. I must refer you, however, to Messrs. Mawe's Catalogue of Surgical Instruments for drawings of the different kinds of transfusion apparatus; as also to the last, or seventh edition, of Denman's Midwifery, edited by Dr. Waller, where you will find a detailed account of the method of conducting the operation.

DR. ELLIOTSON ON LIFE AND MIND.

To the Editor of the Medical Gazette.

SIR,

MY last letter was devoted to an examination of Dr. Elliotson's proposition, that it is the brain which thinks, as clearly as that the liver secretes bile; wherein I endeavoured to show that such a proposition can never be established, for a variety of reasons, but chiefly because it is opposed to the fundamental laws of human belief. I conceded, nevertheless, that when stated as an hypothesis, it admits of being supported by a number of plausible arguments. To these arguments, as advanced by Dr. Elliotson, I would now call your attention.

"The brain thinks, and feels, and wills*." "To call the human mind positively a ray of the divinity appears to me absolute nonsense. Brutes are as really endowed with mind, with a consciousness of personality, with feelings, desires, and will, as man. Every child is conscious that it thinks with its head; and common language designates this part as the seat of mind; *e. g.* a stupid person is honoured with the expressions numbskull, thick head, &c. Observation shows that superiority of mind in the animal creation is *exactly* commensurate with superiority of brain, that activity of mind and of brain are *co-equal*, and that as long as the brain is endowed with life, and remains uninjured, it, like all other organs, can perform its functions, and mind continues; but, as in all other organs, when its life ceases, its power to perform its function ceases, and *the mind ceases;* when disease or mechanical injury affects it, the mind is affected; inflammation of the stomach causes vomiting—of the brain, delirium; a blow upon the loins suppression or alteration of the urine; a blow upon the head stuns; if originally constituted defective, the mind is defective; if fully de-

* Page 39.

veloped and properly acted on, the mind is vigorous; accordingly, as it varies with age, in quality, and bulk, is the mind also varied. The mind of the child is weak, and very excitable; of the adult, vigorous and firm; and of the old man weak and dull, *exactly* like the body; and the character of the mind of an individual agrees with the character of his body, being equally excitable, languid, or torpid, *evidently* because the brain is of the same character as the rest of the body to which it belongs. The female mind exceeds the male in excitability as much as her body. The qualities of the mind are also hereditary, which *they could not be* unless they were, like our other qualities, corporeal conditions; and the mind is often disordered upon the disappearance of a bodily complaint, just as other organs besides the brain are affected under similar circumstances. The retrocession of an eruption may affect the lungs, causing asthma—the bowels, causing enteritis—or the brain, causing insanity. Phthisis and insanity sometimes alternate with each other, just like affections of other organs. The laws of the mind are *precisely* those of the functions of all other organs: a certain degree of excitement strengthens it, too much exhausts it, physical agents affect it, and some specifically, as is the case with other functions; for example, narcotics. The argument of Bishop Butler, that the soul is immortal, and independent of matter, because in fatal diseases the mind often remains vigorous to the last, is *perfectly groundless;* for any function will remain vigorous to the last, if the organ which performs it is not the seat of the disease, nor much connected by sympathy, or in other modes, with the organ which is the seat of the disease*."

It is not my intention to scrutinize minutely all the proofs adduced in these passages, although I think it might be shown that some, perhaps, ought to be rejected; that others are overstated; and that several are puerile. A few remarks on each will suffice to convey the opinion I am led to entertain of the value and character of the whole.

First. "Brutes are as really endowed with mind, with a consciousness of personality, with feelings, desires, and will, as man." Admitting this, what then? How does the fact that brutes possess mind prove that, in man, the brain is the mind? I see not how, unless the writer means to assume that it is certain, a thing universally admitted, that mind in a brute is nothing besides the brain. So far from granting such an assumption, I think there is reason to infer that brutes possess an immaterial principle, or entity. I say, reason to *infer* this, from observation; for I want, in reference to the nature of brute mind, other two kinds of evidence—viz. consciousness and testimony, which I possess with regard to the nature of the human mind. It ought to satisfy the inquirer, if I am able to show that in man, *that* which thinks is not the brain, without its being necessary that I should entangle myself with an extraneous question concerning the nature of inferior orders of mind. My knowledge of the minds of brutes is both inexpressibly limited and imperfect; even the end for which the greater proportion of animals exists at all, is, I confess, utterly beyond my comprehension. Although, therefore, I might be excused if I were to pass without notice this argument of Dr. Elliotson, I choose rather to entertain it, feeling that to deny to brutes the possession of an immaterial principle, in the face of a degree of evidence for the fact, *merely* because it may shock the prejudices of the vulgar to admit that brutes have souls, is unworthy of intrepid reason.

Second. It is incorrect to assert that "every child is conscious that he thinks with his head;" the fact being that every person, capable of reflection, is conscious that it is with his mind that he thinks. It is only by experience that he at length is aware that the head is the chief seat of the mind. Were the four senses which are placed in the head—hearing, sight, smell, and taste, situated more remotely from the brain, suppose on the thorax (and by imagining the prolongation of their nerves this is conceivable), probably it would not be so readily discovered, as it now is, that the head is in a peculiar sense the instrument of the mind. It is from having to move the head to accommodate the eye in seeing, and the ear in listening, and by the infinite variety of other movements the head is made to perform in the exercise of these senses—the prime ministers of the mind—together with the sudden interruption of the mental operations produced by blows on this part of the body, that every one insensibly acquires the conviction that the residence of thought is there. A person is not, therefore, conscious of thinking with his head; but he infers that the conscious mind employs the head in its operations.

Third. "Superiority of mind in the animal creation is *exactly* commensurate with superiority of brain." This is an incautious assertion, if by "superiority" is to be understood largeness, or some particular form of the head, since these are at present points confessedly of doubtful disputation, and likely so to continue. Besides, Dr. Elliotson is well aware that insects, the

* Page 37. It is only fair to remark, that the words in Italics are not in that character in Dr. Elliotson's text.

most reasoning, perhaps, of all the inferior creatures, have properly no brain, a slight enlargement of the upper extremity of the spinal marrow being all that stands for that organ *.

Fourth. " Activity of mind and brain is coequal." If the writer believes, as he affirms, that it is the brain which thinks, what sense is there in saying that two names—brain and mind—implying the same thing, possess co-equal activity? which, on his hypothesis, is as void of meaning as if a lapidary should announce, in reference to the same precious stone, that the hardness of the diamond and of the brilliant is co-equal! But if, on the other hand, it be not a settled point, and this Dr. Elliotson will surely allow, that the living brain is the mind; on what ground, supposing that the brain and the mind are distinct substances, could any one pretend to determine that they possess co-equal activity? or by what signs detect inequality in this respect?

Fifth. " When disease or mechanical injury affects the brain, the mind is affected; inflammation of the stomach causes vomiting—of the brain, delirium; a blow upon the loins suppression or alteration of urine; a blow upon the head stuns." How convenient is the term " affects!" The mind is affected by a blow upon the head, and the kidney is affected by a blow on the loins; therefore it is the head that thinks, as obviously as that it is the kidney that secretes the urine; a blow equally affects both, and thus is established a sound and perfect analogy between the two organs! But every thing in nature is liable to be *affected*: an earthquake " affects" a whole province; moisture affects a violin; and bad news affects the funds: hence, to be affected is no proof of there being analogy in the nature of the different affections. Although it be true that inflammation of the brain affects the mind with delirium, and inflammation of the stomach affects that organ with vomiting, what conceivable analogy is there between delirium and vomiting? Surely it is not on such confused reasoning as this that the student ought to admit the proposition that it is the brain which thinks? Doubtless it is true, in general, that disease and mechanical injury of the brain ultimately impair, suspend, or derange, the operations of mind; but every experienced pathologist is aware, that the effects on the mind resulting from diseases of the brain are wonderfully little uniform in their character. There is evidence that, in numberless cases of disorganizing disease of the brain, the mind remains apparently perfect until the malady has existed for weeks, or even for months, and produced extensive ravages. The opinion of so eminent a pathologist as Dr. Allison, in regard to the influence of organic cerebral disease on the mind, is well worthy of attention. "The very same kinds of alteration of the mental faculties," says Dr. Allison, " have often been observed from disease or injury of very different portions of the brain; and again, large and various portions of the hemispheres of the brain have been found, in other cases, manifestly injured, or even destroyed by disease, without perceptible alteration of the mental faculties, almost to the moment of death. Experiments and pathological observations would seem to indicate, therefore, that the manifestation of these mental phenomena depends not so much on the mere presence of any particular quantities of the nervous matter of the hemispheres, or the forms which it presents, as on some other conditions in that nervous matter *."

I am far from intending to insinuate that the nervous system is not the organ by which the mind, to a certain extent (we know not to what extent), operates; or that the same system is not necessary, in some inscrutible manner, to the manifestation of mind, as the attribute of a human being; but the deliberate opinion of Dr. Allison may serve to caution the student against reposing implicit confidence in vague unwarrantable comparisons between the disorders of the mind and the disorders of a secretory gland.

Granting that disease, malformation, and mechanical injury of the brain, variously affect the mind (for these are established facts), we ought not to forget that there are other facts of an opposite and countervailing kind, in reference to the influence of the mind upon the brain and the whole body, equally well established. If it be true that cerebral disease disorders the mind, the converse is equally true—namely, that certain purely mental affections disorder the physical frame. Emotions and other mental states—emotions, it may be, not excited directly by external circumstances, but originating in a process of reasoning, will, even in the most vigorous and healthy person, produce bodily disease, or sometimes death itself. Here it is obvious, that if, in delirium or stupor resulting from disease or injury of the brain, the state of the bodily organ stand

* Dr. Elliotson, as an instance of reason in brutes, mentions an anecdote of the wasp, on the authority of Darwin.

* Allison's Physiology, &c. &c. p. 238.

in the relation of *cause* to the mental disorder, equally true is it that in such disorders as syncope, convulsions, diarrhœa, palsy, fever, abortion, amenorrhœa, produced by sudden emotions, the *cause* is not a bodily but a mental state; the proof of which is not inferential, as in the instance of a mental disorder ascribed to cerebral disease as its cause, but matter of distinct consciousness. In disease of the brain, attended by delirium or insanity, a doubt may be insinuated as to whether the cerebral disease is the cause of the mental disorder, seeing that violent affections of the mind oftentimes result from disease of distant organs in the absence of disease of the brain; but in the instance of syncope, or any other disorder suddenly produced by emotion, no such doubt can be insinuated; because the person affected is conscious (it is not an inference); he is conscious that the cause of his bodily disorder—the first phenomenon in the series—was a mental state. In a political revolution, or in a great commercial crisis, how numerous the victims of disease and death, caused solely by mental affections! The practical observer of life knows well that mortified vanity, rage, critical suspense, disappointment, sudden poverty, bereavement of those who are dear, remorse, are every day producing their dreadful effects; in such forms, too, as even the page of fiction most inadequately pourtrays. M. Georget, the celebrated writer on madness, remarks, that "among one hundred lunatics, ninety-five at least have become such from the influence of affections and moral commotions."

It is not alone, however, to states of mind as causes of disease that we are to have regard; they produce effects of a curative and salutary kind. Restoration of health, the incredible endurance of toil, of hunger and thirst, of solitary imprisonment, of the extremes of heat and cold, calmness in the view of a violent death, resistance of the most powerful contagions, are all often witnessed as the effects of temporary states of mind influencing the bodily frame. These states of mind, it is well worthy of remark, will sometimes be strengthened in a tenfold degree in a moment, or as instantly annihilated, by a single word of hope or of discouragement whispered in the ear, or by a single thought suggested in the course of solitary reflection: the instantaneous result being increased boldness and firmer determination; or irresolution, feebleness, and despondency.

Upon the whole, therefore, it is clear that the fact that injuries of the brain disorder the mind, cannot be admitted as proof that mind is the function of the brain; seeing that purely mental states can equally disorder the brain and the entire body, and, indeed, affect the body in a great variety of ways infinitely more wonderful than bodily states ever affect the mind.

Sixth. "The character of the mind of an individual agrees with the character of the body, being equally excitable, languid, or torpid; *evidently* because the brain is of the same character as the rest of the body to which it belongs." Whether what is here asserted to be *evidently* true, is true or not, cannot be proved. To suppose that the brain might be naturally torpid, and the rest of the body naturally excitable, or to suppose the opposite of this, appears to me to involve something like absurdity. As little meaning is there in the assertion that "the female mind exceeds the male in excitability as much as her body;" for if the mind is only the property of the body, the remark is needless; and if the mind is not a corporeal property, by what means is excitability of the body, apart from excitability of the mind, to be discovered? Surely it is natural to expect that the body and the mind of the same individual will be adapted for the production of union and harmony, whether the mind be a property of the brain or something different.

Seventh. "Qualities of mind could not" be transmitted, if they were not "corporeal conditions." How does Dr. Elliotson know? Every one *knows* that qualities of mind do often descend from parents to their children, but no one can pretend to *know* that mental qualities are actually corporeal conditions. The "could not," therefore, is not altogether in the true spirit of philosophical caution.

Eighth. "The laws of the mind are *precisely* those of the functions of all other organs:" of those of the kidney, for example. Does the writer mean that the laws of volition, or of the religious emotions, which are laws of the mind, are precisely similar to those which regulate the functions of the kidney in the secretion of urine? If this is not his meaning, I know not what different construction the passage admits of. Perhaps he means that certain only of the laws of the mind are precisely those of the functions of other organs; for here are his proofs:—"The laws of the mind are precisely those of the functions of all other organs; a certain degree of excitement strengthens it, too much exhausts it, physical agents affect it: and some specifically, as is the case with other functions—for example, narcotics." The term "excitement" here plays the part that "effects" did in a former passage. A certain degree of ex-

citement—the company and conversation of an agreeable friend—strengthens the mind precisely as a certain degree of excitement—the operation of a gentle diuretic—strengthens the kidney. Too much excitement—for example, the fear of shipwreck or of bankruptcy—exhausts the mind precisely as elaterium exhausts the kidney. Such a physical agent as opium "affects" the mind, causing sleep, dreaming, elysian visions, or tartarean horrors; and the same drug "affects" the kidney, causing the urine to be red and scanty. I disclaim the desire to draw from the writer's arguments ridiculous conclusions; but, unless I am deceived, this result is inevitable; for surely no dispassionate reasoner will contend that the terms "excitement," "strengthens," "exhausts," "affects," applied to the mind, have any analogous meaning when employed with reference to the kidney.

Ninth. "The argument of Bishop Butler, that the soul is immortal, and independent of matter, because in fatal diseases the mind often remains vigorous to the last, is *perfectly groundless:* for any function will remain vigorous to the last, if that organ which performs it is not the seat of the disease," &c. The words of the Bishop alluded to are, I presume, that often persons affected with mortal diseases are seen to possess "apprehension, reason, all entire; with the utmost force of affection, sense of character, of shame, and honour, and the highest mental enjoyments and sufferings, even to the last gasp"—facts which he imagines render it probable that such diseases will not be the destruction of our reflecting powers. It is, perhaps, not so clear that the Bishop's argument is "perfectly groundless;" for every attentive observer must have noticed instances of persons in whom life was all but extinguished, combating with languor, manifesting at intervals lively emotion, and uttering the most weighty sentiments. In general this is not the case. Commonly the mental operations become languid as sensation and the power of voluntary motion decline, so that in the last hours of life the mind, equally with the body, would seem to be falling a prey to the disease. The cause is obvious. The mind of most persons during health is, in a great measure, habitually under the influence of bodily sensations, the predominant bias having reference to "what shall we eat, and what shall we drink, and wherewithal shall we be clothed." In others, however, in whom the reflective faculties, and the religious and domestic affections, have been habitually cultivated, the last scene is very different. Here we often witness the torpid declining body compelled to give utterance, it may be in the feeblest accents or signs imaginable, to the most sublime sentiments and the tenderest feelings. There is an evident struggle against the influence that is sealing up the senses. Perhaps there is a wish to bear a dying testimony to the verity of religious hope, to utter a last word of affectionate advice, or to express dissent from an obnoxious proposition. In such a case we may witness the mind *voluntarily* and *deliberately* striving to collect and concentrate its powers for a last effort—attempting again and again, it may be unavailingly, to utter something, there remaining obviously the power to *will.* It is this war of the intellect against stifling pain, and the most overpowering languor and exhaustion, as life is ebbing, —one kind of nature striving to rise above, and for a moment triumph over a different kind of nature, — to which, doubtless, Butler refers in the passage of the Analogy Dr. Elliotson has alluded to. Whether or not the Bishop's inference—that it is *probable,* on the strength of such evidence, that death will not be the destruction of our reflecting powers — is "perfectly groundless," the reader who has studied the Analogy, and bears in mind the scrupulous, the timid caution of that wonderful writer in all his deductions, may be left for himself to determine.

There is probably a source of confusion and fallacy in the mind of Dr. Elliotson, of which he is utterly unaware, arising from his allowing himself to imagine that those who argue for the mind's being an entity, must hold, as a thing of course, a variety of fanciful opinions concerning the nature and essence of mind. On no other supposition can I account for his bringing forward several of the arguments which have been already noticed. It would seem he considers that those who take the side of the argument I am endeavouring to support, hold the mind to be a spiritual, immortal, perfect, *quasi* angelic intelligence, of which almost every thing ought to be predicated that might be predicated of an angel. I, however, beg to disclaim all such hypothetical fancies, as, indeed, I before disclaimed them. My opinions of the nature of mind I am desirous of forming with the same caution and deliberation I exercise in the investigation of other subjects: in proof of which, I allow, with Dr. E., that the mind varies with age; that, in a sense, it is weak, ignorant, and excitable in the child; vigorous and intelligent in the adult; and in the aged frequently—by no means always—comparatively dull; that it is greatly and variously affected by diseases and inju-

ries of the brain, as well as strikingly influenced by innumerable other forms of material agency. I am further willing to allow that (revelation apart) I have not discovered evidence on which I can fully rest, whether it ought to be regarded as immortal, or as being of limited duration, like the body. But I am compelled to confess, that if it possesses any quality in common with organic matter, I am wholly ignorant what that property is. The facts Dr. Elliotson has adduced to prove that the brain is the mind, and a thousand other facts of a similar kind, may very properly, perhaps, be employed in overthrowing the fancies he *attributes* to theologians. Further than this, they do not serve his purpose.

Dr. Elliotson deprecates the idea that his opinions on mind tend to materialism,—a doctrine which may be thus defined: mind never being found but in conjunction with a brain, we ought, as philosophers, to conclude that mind necessarily exists in, and results from, that organ, unless it can be shewn to be incompatible with other known properties of the same substance *. These, it is true, are not Dr. Elliotson's words; but several passages in his work evince that the sentiments differ little from his. For example, the qualities of the mind are "corporeal conditions." Again: "when the brain dies, the mind ceases." And further: "at death our being is utterly extinguished, and we go back to the insensibility of the earth, whence we were taken." But such sentiments are not those of a materialist: quite otherwise! "The assertion," says he, "that the mind is a power of the living brain, is not an assertion that is material: for a power or property of matter cannot be matter." In my former letter, this notion that a property of matter—blueness, sweetness, or hardness—is not matter, but something superadded thereto, and, it is to be presumed, immaterial, was briefly noticed. It no doubt may be said that sweetness,—to take that property as an instance,—is not the name of a material thing, but merely the name of a sensation caused by matter; but this would be a mode of evasion leading to utter absurdity; for though it is true that a sensation of sweetness is not matter, but a mental state, nevertheless we instinctively regard sweetness as not existing in the mind, but belonging to the particular material body which causes in us this sensation: in a word, we mean that it is the material body which is sweet. To affirm that the sweetness is something different from the material body, is equal to affirming that every other property of matter is something different from matter, and that no such thing as matter is known to us; which every one feels to be absurd. If, therefore, the mind be said to be a property of the brain,—and this Dr. E. maintains,—the only meaning I am able to attach to such a proposition is, that when a person, by the help of his senses, (and how otherwise can he examine the *properties* of a body?) examines the brain, he discovers mind—acquires the idea of mind, just as he perceives the property of sweetness when he examines the saccharine body by means of the organ of taste. However, be this as it may, to assert that the mind is a property of the living brain, is obviously equal to asserting that the mind is as much matter as the living brain is matter. Whether or not the living brain, or any other living organ, is more than matter, needs not again to be discussed. Possibly this explanation may be regarded as uncalled-for repetition; but surely so grave a fallacy as that a property of matter is *something different from matter*, requires to be freely exposed, in order that this species of argument being stripped of the mystery which it appears is ever ready to envelop it, the sophistical disputant may be prevented from taking to his usual refuge—mere words.

The opinions on mind which Dr. Elliotson would inculcate on the rising race of physiologists,—for it is to the student that his book will chiefly be acceptable,—lead by no circuitous route to Spinozaism—the most odious of all systems of speculation, inasmuch as it is not one of pure Atheism, but leaves us a kind of Deity whom we must for ever abhor. Not that Dr. E. is himself an avowed disciple of this system,—that I am far from affirming,—but it is plain that his chief reason for regarding the brain as being the mind, differs little from the main argument of Spinoza. The former cannot conceive of mind without a brain, and Spinoza merely extends and amplifies the same principle: for, perceiving nothing besides matter to exist, he infers that nothing but matter does exist. Indeed, if the evidence for mind's existence as a substance or entity be regarded as baseless or insufficient merely *because* we have no experience of its existence dissevered from organization, then is the existence of a creating, sustaining intelligence, distinct from "the things which are seen" incapable of satisfactory proof; since it may be fairly argued that if the medullary substance, brain, is endowed with power to feel, think, and will, and, in the exer-

* This definition of materialism differs little from that of Dr. Priestley.

cise of this power, operates on matter (as we know the mind does) with wonderful skill and extensive effect, it is probable that the material universe possesses a similar, though infinitely ampler, power: thus the brain being regarded as the mind, the universe will necessarily be regarded as the Deity.

Dr. Elliotson wanders far, I presume to think, out of his proper line, when he enters on the inquiry as to whether revelation affirms that we are more than mere bodies,—a question requiring more space, and, perhaps, a greater fund of knowledge for its solution, than the writer of an elementary treatise on human physiology can be expected to afford.

In incidentally touching on points of human science or speculation,—and they are never touched upon otherwise than incidentally,—revelation employs chiefly, we have every reason to believe, the language of the unlearned of those times, without in a single instance pretending to enlarge our knowledge of such matters, which would have been wholly foreign to its avowed object—the making known to men, the learned and the unlearned alike, the will of God for their salvation. Nevertheless, in many passages our Saviour intimates, it might seem casually, the conscious existence immediately after death of such as were then, as respects their bodies, dying, or already dead *. And St. Paul, there can be no question, believed that the death of his body would not suspend the enjoyment of his conscious mind. "While," says he, "we are at home in the body, we are absent from the Lord," affirming that he is "willing rather to be *absent* from the body, and *present* with the Lord." Further, he expresses a desire "to depart and be with Christ, which is far better." In a number of passages which authoritatively reveal the resurrection of the body, it is true the language presents great difficulties, and might, if taken separately and alone, be supposed to favour the belief that all we are descends at death into the grave. It is unquestionable, however, from the passages of scripture which have been quoted, (and the reader may augment their number tenfold by selecting parallel passages), that the New Testament writers, without pretending to determine whether the mind be or be not material, *admit* and *assume as true* the common belief of all ages, that the mind of man continues in conscious existence after the dissolution of the body.

It may be said, "what if it be true that medullary matter thinks?—If, in fact, it does so, it must be the best possible substance for thinking: the question is one, therefore, of little or no importance." Some such argument has been employed, but it possesses not even the slender merit of being plausible; for on similar grounds the Atheist might argue that the question as to the existence of a God is trivial, it being reasonable to infer that if, in fact, there is no God, the universe is better without a presiding power. The question is not at all whether it be best to believe what is true, but whether it be proper to reject the evidence of our own consciousness, in order to admit a gratuitous hypothesis, which being admitted, tends to heap contempt on human nature, and is directly opposed to the consoling belief of thousands of the despised and the miserable:—to that belief which the ill-favoured, the misshapen, the loathsomely and the incurably diseased (to mention no other forms of misfortune, and to allude to no higher considerations) naturally cling, that there is *that* within which, partaking in no degree of the repulsive defects of the body, entitles them to their proper rank in society, or, at all events, secures to them the sympathy and the respect due to a moral and intellectual nature, which not an abscess or a tubercle in the brain, but vice and ignorance alone, can really degrade.

Yours respectfully,
JOHN ROBERTON.

Manchester, Nov. 14, 1835.

* Luke, xii. 4 5, xvi. 27-28: also xxiii. 43, 46. These passages, when duly weighed, will be found decisive of the argument.

ON THE

INTERNAL USE OF SEA-WATER

IN VARIOUS DISEASES.

To the Editor of the Medical Gazette.

SIR,

SHOULD you deem the annexed paper, on the internal use of sea-water, worthy a place in the Gazette, I shall feel obliged by your affording it admission.

I am, sir,
Your obedient servant,
EDWARD GREENHOW, M.D.

North Shields,
Nov. 7, 1835.

Sea-water seems to have been a popular remedy for a great variety of diseases from a very remote period; and although many medical writers have

noticed it in favourable terms, it has never been generally taken up by the profession, and consequently has never occupied that place amongst remedial agents to which I think it is well entitled. Amongst the ancients who have written in its favour, are Hippocrates, Celsus, Asclepiades, Aretæus, Pliny, Marcellus, and Serenus, besides a whole phalanx of others; and nearer our own times, are Russell and Saunders, &c.

Sea-water, taken internally, exerts a powerful influence over various organs of the human body; its most obvious and immediate effect is upon the bowels and kidneys, producing copious evacuations from the one, and a large flow of urine from the other; it also has an influence upon the circulation, producing an acceleration of the pulse, and an increased temperature of the surface of the body. But its action by no means stops here: it stimulates the liver, and exerts some peculiar influence upon the glandular and lymphatic system, rendering it a most useful remedy in the treatment of strumous tumors and ulcers; and although its use is not applicable to other diseases where an inflammatory tendency exists, this does not hold good as respects scrofulous affections. The progress and decline of these complaints is so slow, that it would answer little purpose to detail the cases in which I have employed it, in conjunction either with cold sea bathing, or with warm sea-water baths. Its effects I have found much more efficient than those of iodine, or any other remedy with which I am acquainted.

In dyspepsia and chronic affections of the liver, my first attention to sea-water, as a remedy, arose from being frequently consulted by the lead-miners from Alston Moor, who flock in great numbers to Tynemouth during the summer, their ordinary sojourn at the sea-side being from two to three weeks; and it is astonishing the change for the better which is wrought in their appearance during that period. When they arrive there, they are generally thin and sallow, suffering from loss of appetite and impaired digestion, together with a torpid state of the bowels. Their usual mode of proceeding is to drink copiously of sea-water every morning, so as to act briskly upon the bowels, and also to bathe in the sea, not once only, but generally twice or even three times in the day. Something, of course, must be ascribed to change of air and of habits, and perhaps also to sea-bathing; but I am inclined to believe that the chief benefit they derive is from the internal use of the sea-water; and this belief led me to try its effect in dyspepsia and cases of torpid liver, and with the most decided advantage.

In the summer of 1834, I was induced to take it for about forty successive days; and as my own case will serve to illustrate its effects, I will relate it. I had been suffering for many weeks from indigestion; having weight and distention after eating, constant dull headache, impaired vision, muscæ volitantes, a dragging uneasiness in the region of the liver, torpid bowels, evacuations pale-coloured, urine dark and scanty, a constant feeling of lassitude, every exertion requiring an effort; and, in short, a total loss of energy, passing restless nights, and rising in the morning wholly unrefreshed. Various remedies had been employed, such as blue pill at night, and a purge in the morning; also infusions of calomba, gentian, &c., with and without rhubarb, soda, &c., but without producing any permanent advantage. I then determined to try the effect of sea-water, which I had so often found useful in similar cases. I accordingly commenced its use, taking a full pint each day early in the morning, while still in bed. Its action upon the bowels was speedy and pleasant, generally passing off in a couple of hours; and instead of losing its effect by repetition, it rather acted more freely after a few days' use than it did at first; and although I continued to take it for forty successive days, it lost none of its powers. Before the end of a week the pain and confusion in the head were much mitigated, the appetite certainly better, the urine considerably increased in quantity, and of a natural colour, and the dragging pain in the side alleviated.

At the end of fourteen days the head nearly well, appetite good, and move about with much more alacrity; sleep tranquil; and from this time all the uneasy symptoms gradually disappeared; the alvine secretions became natural, and at the end of six weeks I found myself restored to my usual health and energies.

From this time I began to entertain a higher esteem for the medicinal powers of sea-water, and have tried it upon a more extended scale, and have every reason to be satisfied with the results. I subjoin an analysis, for those who have not before attended to the subject.

Mur. Sodæ	180·5
Mur. Magnes.	18·3
Mur. Calcis	5·7
Sulph. Magnes.......	21·6
	226·1

Iodine and bromine have also been detected in it. These are the relative proportions; and an imperial pint of sea-water contains somewhat more than half an ounce of these salts, subject to some slight variation.

NERVES OF THE TONGUE.

EXPERIMENTS SHOWING THAT THE FUNCTION OF TASTE IS DISTINCT FROM COMMON SENSATION.

To the Editor of the Medical Gazette.

SIR,

BY the favour of Mr. Ker, one of the surgeons to the Ardwick and Ancoats Dispensary, I have been enabled to ascertain some particulars of a case of facial paralysis, leading to the same conclusions, relative to the separateness of the functions of "taste" and "common sensation," as my former communication upon the same subject, which appeared in the Gazette about a year ago.

It may be in the recollection of those of your readers whose attention has been directed to this branch of physiology, that the case of Mrs. Williams, which you did me the honour to publish, manifested the existence of paralysis of sensation in one half of the tongue, whilst taste, on the paralysed side, was in a state of integrity. The case which I am about to detail forms almost the exact counterpart, taste being entirely abolished on one half of the lingual surface, whilst its common sensibility is but slightly impaired.

John Davis, 35 years of age, residing in Ordsall Lane, near the Regent's Road, Salford, was attacked with paralysis of both motion and sensation, in the left half of the head and face, about two years ago. He states that for two or three years prior to this attack he had suffered from pains of a rheumatic character, especially in his head; that the headache was not confined to either hemisphere; that latterly he has not been so much afflicted with these pains; and that, generally speaking, the paralysed condition has gradually improved. He was seen by me for the first and only time a few days ago, and his present condition appears to be as follows:—He complains of no particular pain, and the face is scarcely at all drawn to the right side. On protruding the tongue it is thrust to the right, as though its muscles were paralysed on the side stated not to have hitherto been generally affected; in other respects the right side of the face seems to be in a healthful state. Vision on the left side would appear to be only affected in a very slight degree, since objects, numbers, and characters, are at once recognised, though not with the ordinary distinctness, according to his own statement. The peculiar sensibility to light, which, on its too vivid approach, occasions the consciousness of dazzling, seems lost, as evinced by the absence of any symptom of intolerance, however near the light of a candle may be brought. Hearing, he states, is entirely gone on the affected side; and common sensation, by his own account, is not so complete on the left as on the right side, though, on any trial being made, he at once expresses the ordinary consciousness; and, in more immediate connexion with the object of the present paper, the sense of taste is entirely destroyed on the left half of the tongue, whilst its common feeling is not very notably diminished.

This investigation took place in the presence of Mr. Ker, through whose kindness I had the opportunity of witnessing this very interesting case, and of Mr. Walker, of this town, whose assiduous attention to the physiology of the nervous system is well known to the readers of the Medical Gazette; and these gentlemen concur with me in attestation of the accuracy of the experiments, of which I will now detail those relating especially to the separateness of the functions of *common* and *specific* feeling in the tongue.

The man during their whole progress was blindfolded; and, on protruding the

tongue, a piece of silver was lightly drawn around its margin, and it was at once recognised as a metallic substance, though somewhat more distinctly on the right side. The point of a lancet was gently thrust upon the mucous membrane of the left side, and the sense of pricking was declared to be felt. The corner of a silk pocket handkerchief was brought barely in contact with the margin of the same side, and a light soft substance was said to have been in contact. A silver spoon was placed on the affected half, and he was asked whether it was hot or cold: his reply accorded with the fact; it was rather cold. I applied the extremities of the fingers, and he recognised them, and also as being rather warm.

These trials were considered to be amply sufficient for determining the existence of common feeling, without any material diminution; and the experiments were next directed to ascertaining the condition of the sense of taste. And it may be well to premise, that the tongue was perfectly clean and moist, so as to afford the fairest opportunity for prosecuting the investigation.

Common salt was sprinkled upon the affected half, and allowed to dissolve, but no taste was perceived until it was pressed against the palate, when the salt was recognised, but whether in the palate of the affected side, or not, was difficult to determine, as the man himself considered the recognition to result from the impregnation of the saliva generally with the sapid ingredient. A portion of aloes was applied to the anterior margin of the affected half, and the application was continued for many seconds, but no taste was felt. On the same application being made to the opposite margin, its peculiar flavour was instantaneously perceived and declared. A concentrated preparation of the compound decoction of aloes was applied on each side with similar results; also a concentration of the lac ammoniaci, and the conclusion was the same. Lastly, a drop of the oil of peppermint was allowed to fall on each side, with perfect insensibility of taste on the left, and instantaneous and acute sensation of pungency and aroma on the right side.

These experiments were considered to render it clear that, in this case, whilst the common sensibility of the left half of the tongue is but little impaired, its specific feeling is lost.

I make no *comment*. This case is offered to the profession as an additional *fact*, tending most strongly, in the opinion of the present writer, to the conclusion that the function of taste is not a modification of common sensation; and that, in accordance with all the analogy which physiology affords, we must look for a separate nervous supply.

I am, sir,
Your obedient servant,
DANIEL NOBLE.

Manchester, Nov. 12, 1835.

DISLOCATION OF THE HIP-JOINT,

SUCCESSFULLY TREATED.

To the Editor of the Medical Gazette.

SIR,

I BEG to send you a statement of the treatment of a case of dislocation of the os femoris on the dorsum of the ilium, in corroboration of the utility of keeping up the action of the skin in contusion and laceration of the ligamentous and muscular structures.

September 29th, 1835. — Mr. John A——, aged 22 years, a strong robust farmer, was thrown out of a cart, by which fall he dislocated the hip-joint. After the reduction he was put into bed, and had the joint fomented with hot water at intervals; took a draught composed of two drachms of sulphate of magnesia and half a grain of tartarized antimony in mint-water every three hours; gruel diet.

30th.—The hip swollen and painful, with discoloration; pulse full and frequent; skin hot; tongue white; great thirst.

Abstract fourteen ounces of blood from the arm; continue the fomentation, draughts, and low diet. A mustard cataplasm to be applied on the hip, just over the trochanter major, for half an hour night and morning.

Oct. 1st.—The pain much lessened; the tongue moist. Pulse fallen and regular; bowels have been freely relieved.

Continue the mustard poultices, omit the fomentations, and take the

draughts twice a day, and one tea-cupful of mutton broth in the middle of the day.

2d.—Much improved; can flex the thigh a little. There is scarcely any pain on making pressure on the upper portion of the os femoris.

Ordered an embrocation to be used twice a day, of equal parts of Liq. Ammoniæ and Lin. Saponis, to the region of the hip, and along the outer part of the thigh.

3d.—Still better.

Discontinue the mustard cataplasms, continue the embrocation, and omit the medicine. Increase the diet to four ounces of meat per day. Use gentle flexion and rotation to the limb in bed.

4th, 5th, and 6th. — Gradually increasing the flexion and rotation of the limb.

7th.—Ordered him to get out of bed, and just bear the weight of the body on the injured leg.

8th.—Ordered him to walk three steps.

9th.—Increased the number of steps to twelve. Apply a flannel roller round the hips.

10th.—Still increase the steps.

11th.—He walked down stairs.

12th.—Sat up the whole day, occasionally walking across the room. I now applied a broad belt round the pelvis, which buckled in front by means of three straps, and two others passed under each thigh, so as to prevent it rising over the spines of the ilium; since which time he has been daily gaining strength in the joint, and is now able to walk two or three miles with little or no inconvenience; forty days only having elapsed since the accident.

I remain, sir,
Yours most respectfully,
John Grantham.

Crayford, Kent, Nov. 2, 1835.

ON DISCHARGE OF PUS FROM THE EAR,

AFTER INJURIES OF THE HEAD.

To the Editor of the Medical Gazette.

Sir,

In a recent number you have inserted an account of two cases of abscess of the brain, discharging by the meatus auditorius; in one of which, a patient of Dr. Macleod's, the natural termination of the disease was shown undisturbed, while in the other, a patient of my own, the rapidity of the disease was probably somewhat accelerated by a blow upon the head. They both of them show the comparative freedom from acute inflammation, and the sloughy appearance of the abscess which these cases usually present.

Discharge of pus by the auditory meatus sometimes takes place, however, in another way—viz. when a fracture of the temporal bone has been followed by suppuration between the bone and the dura mater. The two cases which I have sent to you from my hospital note-book are examples of this accident. In the second, dissection showed the manner in which the discharge had taken place through the fracture; and the first is, I presume, an instance of the same injury, for it can hardly be supposed that, in such cases, the discharge can take place by *ulceration* through the bone, as in the more common cases which you formerly related, since it occurs at too early a period after the accident to make such ulceration probable; and every day's experience shows that fractures of the bones of the skull, attended by symptoms of concussion only, are by no means unfrequent, and also, that the petrous portion of the temporal bone is a situation in which fractures are very often produced, especially where a person falls from a height upon the vertex or side of his head, so that the roof of the cavity of the tympanum, which is opened by the fracture, affords a ready exit to the pus, the membrana tympani being either ruptured at the time of the blow, or opened by ulceration at a subsequent period.

The *direction* of the fracture in the second case was obliquely from behind forwards, so that the seventh pair of nerves escaped; but in other cases of the same accident, it is across the base of the skull, implicating the sphænoid bone, and often both temporal bones at once. In such severer injuries, extensive discharge of blood through the meatus, or between the dura mater and the bone, or into the cavity of the cranium, arises from laceration of the lateral sinus; and the hæmorrhage from the ear is accompanied by partial paralysis of the same side of the face, or by

deafness, from injury to the divisions of the seventh nerve, as it lies within the temporal bone. In the first case deafness existed; but it was probably, from its temporary nature, produced by inflammation only, and not by direct injury to the nerve; and perhaps the reason why it was not occasioned in the second case, might have been from the suppuration having taken place *behind* the superior angle of the petrous bone, *i. e.* in connexion with the cerebellum, instead of the more usual situation in front of this angle, and in connexion with the cerebrum; the organ of hearing being nearer to the latter situation than to the former.—I am, sir,

Your obedient servant,
CÆSAR HAWKINS.

31, Half-moon-Street,
Nov. 14, 1835.

CASE I.—*Injury of the Head—Symptoms of Pressure—Discharge from the Meatus.*

James Cook, æt. 14, was admitted into St. George's Hospital, June 4th, 1830, with the following history:—About a month before his admission he fell from a first-floor window upon some loose earth, striking the right side of his head in his fall. He was stunned for ten minutes, and vomited on his recovery, and was confined to his bed three weeks with *acute pain* in the head, during which time he was bled and cupped with relief. Then he walked about apparently well for two or three days, till, on the 31st of May, he was seized with his present symptoms.

He is unable to stand without assistance, and the movements of the legs are staggering and uncertain, so that the legs cross one another, and he would fall down if not supported. He has dimness of sight, wandering of the eyes, dilated pupils, scarcely acting at all when the candle is brought near them, especially the left, which is almost entirely insensible. Severe pain in the forehead; bowels constipated; pulse 108, rather full and sharp; tongue white, but moist. Skin cold on admission, from his ride to the hospital, but soon rather hot and dry.

V.S. ad ℥xj. Appl. Lotio frigida fronti.
℞ Pulv. Ant., Hydr. Submur. aa. gr. iij. M. vespere.
℞ Haust. Sennæ mane.

5th. — Blood not buffed. Bowels opened with difficulty, after a repetition of purgatives, and the evacuations dark-green and offensive. Countenance more calm, and he breathes easily, and says he is better. Pulse 112, soft and compressible; tongue white in the centre, red elsewhere; skin very hot and dry.

Hirud. x. fronti. Enema Oleos. statim.

6th.—No flush, no heat of skin; tongue moist; pulse only 68, but with now and then a double beat, and rather thready; bowels open twice. Pupils not so sluggish, nor so dilated; the left now contracting more than the right, instead of being less influenced by light. He complains much of pain about the right ear, near the seat of the blow, but the general pain of the forehead is gone, and he thinks himself partly deaf on the right side.

℞ Cal. gr. iij. P. Jalapæ, gr. xij. statim.

7th.—Slept well, and has less pain. When taken out of bed he evidently had more power over the legs, though they still crossed one another. Pulse 68, soft and quiet; tongue clean and moist.

H. Sennæ 3tiis horis.

8th.—Empl. Lyttæ nuchæ.

12th. — Considerable difficulty has been experienced in overcoming the torpor of the bowels by repeated purgatives; the evacuations, however, have become more healthy. Symptoms all lessened. Copious *suppuration* from the *right ear*, the pain around which has lessened.

19th.—Rep. Empl. Lyttæ, et appl. vesic. Ung. Hydr. fort.

Discharge from the ear still copious. Bowels continue torpid. Legs stronger, and directed properly, but tottering; pupils somewhat dilated, and still sluggish.

26th.—Rep. Empl. Lyttæ nuchæ, et postia appl. Ung. Sabinæ.

From this time the improvement in his gait steadily continued, and the discharge from the ear lessened, and nothing of consequence was done except his taking for three or four days a grain of calomel twice a day; and on the 20th July he left the hospital quite well, walking as strongly as ever.

CASE II.—*Fracture of the Temporal Bone—Suppuration in the Lateral Sinus—Discharge from the Meatus.*

Philip Burns, æt. 37, was admitted

into St. George's Hospital, July 27th, 1835, soon after the following accident. While scuffling with another man, being intoxicated at the time, he fell on the floor, and struck his head against the edge of a stone step, by which a scalp wound an inch and a half long was inflicted behind the left ear, the bone beneath (the angle of the parietal, just at its junction with the occipital and temporal) being a little denuded. He was said immediately after his fall, while still on the floor, to have had some convulsion like epilepsy.

After his admission he had no symptom of injury of the head, except a little giddiness on the 30th; the wound nearly healed, and he was made an out-patient on the 5th August.

August 27th he was brought to me again, and I re-admitted him into the hospital, at which time he had much irritative fever, with considerable anxiety of countenance, with a weak pulse and cold extremities, as if he had been half-starved, which, indeed, he complained of having been, the tongue being white but moist. It appeared that he had experienced much pain in the head since he left the hospital, and that yesterday considerable *purulent discharge* had taken place from the *left ear*. This still continued; and on examining the wound behind the ear, which was foul, and the edges undermined, the bone was felt exposed, and the probe passed some way into it through the intervals of the suture; and what seemed to be the end of a fissure in the temporal bone joining the suture was also felt.

> Three grains of Calomel and of Antimonial Powder, with one of Opium, were given, and a Senna draught in the morning.

28th.—He slept well, and feels better; the pulse has more power, though without much sharpness. Complains of pain in the head. V. S. ad ℥viij. Blood inflamed; faintness produced.

> ℞ Haust. Salin. ℥iss.; Magn. Sulph. ʒj.; V. Antim. ʒss. M. 6tis horis.

29th.—Had rigor twice last night; slept badly; discharge from the ear increased.

30th.—Was very restless last night, and required an opiate. Pulse weaker. Had one rigor.

31st.—There is now no pain in the head; pulse 80, weak; tongue inclined to have a brown fur; discharge free from the ear; has profuse perspiration following repeated rigors; looks anxious and distressed both in mind and body.

> Omit. H. Salin. Ordered some beef-tea and light nourishment.
>
> ℞ Mist. Camph. ℥iss.; Ammon. Subcarb. gr. vj.; Sp. Æther Nitros, ʒss. M. 6tis horis.

September 1st.—Less perspiration; no rigor; no pain, and he feels better. No affection of the mind nor of the muscles, except indistinctness of speech from apoplexy two years since.

3d.—A little delirium. Occasional vomiting after food; perspiration continues. He is evidently weaker.

The operation of trephining had been several times agitated since his admission into the hospital, but as the discharge from the ear was quite free, and the exposed bone had not the appearance of having purulent matter below it, and there was an entire absence of symptoms indicating either direct irritation or pressure upon the brain, the operation was not hitherto thought advisable by my colleagues or myself. The symptoms were not those of confined matter connected with the brain, but only of suppuration of a low kind *somewhere*; and the suppuration might, perhaps, be in the lungs or liver, or in some other situation, from secondary deposits, with as much probability as in the head. If the confined matter was situated in the head, it might be *within* the dura mater, with as much or even greater probability than *below* the bone; as what was perhaps formed below the bone in the situation of the injury, appeared to have so ready an exit through the ear. As, however, he was evidently sinking, unless some relief could be afforded, we now thought it better to give him the chance, though small, of some pus below the injured bone not being readily discharged.

The trephine was accordingly applied in the centre of the exposed part of the parietal bone, so as to include the end of the fracture in the temporal, where it reached the suture of the three bones at their junction. The bone, however, was only dead on the surface; it bled freely during the operation, and afterwards, from a large meningeal branch, and the dura mater was perfectly healthy below.

He was, of course, not relieved by the operation. The perspiration continued, with occasional slight delirium;

the tongue became brown, and rather dry. He then had drowsiness and inclination to stupor, though he was capable of answering rationally almost to his death, which occurred in the afternoon of September 6th.

On examination after death, it was found that the aperture made by the trephine included the end of the additamentum suturæ lambdoidalis, and the end of a fracture of the temporal bone, which extended from the suture to the meatus, and across the petrous portion to the junction with the sphænoid bone. The portion removed was the end of the parietal bone, just above the superior angle of the petrous portion of the temporal bone, and was consequently less than a quarter of an inch from the tentorium and the lateral sinus, where the horizontal portion of the latter turns down into the deep fossa of the temporal bone. The dura mater, where exposed by the trephine, and on the upper part of the temporal bone, was quite healthy, but below the level of the tentorium was inflamed, and coated with lymph and pus on both its surfaces. The suppuration seemed to have commenced in the lateral sinus in the temporal fossa, as it was obliterated by coagula of blood, which adhered to its inner surface; and between it and the bone were two small ulcerated openings, while another ulceration had taken place on its internal or cerebral aspect. From this opening matter escaped, so as to cover the neighbouring part of the cerebellum to some extent. Around the external opening more lymph had been effused than around the internal, preventing the separation of the dura mater from the bone to a greater extent; and thence the matter had escaped either along the line of fracture posteriorly into the meatus, or perhaps through the fracture in the roof of the tympanum, from which cavity it made its way suddenly the day before his second admission into the hospital: ulceration having then, perhaps, taken place in the membrane.

EXTRAORDINARY CASE OF ABSTINENCE.

To the Editor of the Medical Gazette.

Sir,

It will give me much pleasure if you find the following case of abstinence for twenty-three days worthy of recording. I hope you will excuse the imperfection of the report, as, owing to the distance, (about seventeen miles), at which the case occurred, and the short time the man survived, I was able to see him only once. I am indebted to the kindness of Drs. Blain and Vass, who were in the more immediate neighbourhood, for many of the particulars; the rest were gathered from the unfortunate man himself and his companions.

I am, sir,
Your obedient servant,
C. F. Sloan, M.D.

Ayre, Nov. 11, 1835.

John Brown, aged 65, of a spare habit of body, and uncommonly vigorous for his time of life, about four o'clock in the morning of the 8th ult., went to his work in the Kilgramie coal-works, Ayrshire. He did not breakfast before leaving home, and had nothing in his pockets except about a quarter of an ounce of tobacco. Shortly after he had commenced work, the alarm was given that the pit was falling in; he unfortunately disregarded the warning, and, alone, of all his companions, was unable to effect his escape. From some circumstances which it is unnecessary to mention here, he was supposed to be not crushed under the ruins, but confined in one of the galleries. This surmise proved correct, and on the 31st of the same month, about 7 A.M., he was discovered alive, having thus existed twenty-three days and some hours without food. The place in which he was shut up was sixty yards long, six feet high, and twelve feet broad. At some distance from where he was found lying there was a collection of water, strongly impregnated with iron. The air was so bad, that the lamps of the workmen were extinguished the moment they entered this gallery; and it was with difficulty, from the giddiness and weakness induced, that they made their way to him, directed by a feeble sound that he emitted. He was quite conscious when found, and on being brought to the light, readily recognized and named his deliverers; his voice was sunk to a whisper; he was so weak as scarcely to be able to approach his hand to his mouth; he was so much emaciated, as to excite the surprise of his fellow workmen by his extreme lightness. His friends gave him some butter, which he seemed to relish very much, and some

milk, which last he asked for himself. So well aware was he of the danger of his situation, that he firmly refused more stimulating food which was offered him. Medical attendance was procured as soon as possible. He was ordered small quantities of soup and wine negus at intervals. His bowels, which were obstinately constipated, were relieved by injections of warm water and soap. Under this treatment he gained strength, and was able, on Sunday, the 1st Nov. to give a distinct account of his sufferings. He stated that for the first two days, according to his calculation, hunger was his most urgent symptom; the tobacco which he chewed relieved this sensation; he had some oil in his lamp, which he did not use, as he had found on a former occasion, when confined for three days under similar circumstances, it had produced sickness. Hunger now passed off, and he began to suffer from severe thirst; this the water mentioned above afforded him the means of allaying. For what he supposed the first ten days, he could walk about, but after that period he became so weak as to be unable to move, and he had unfortunately lain down at some distance from the water. He remained in this situation till discovered. He slept but little, and never soundly, never entirely losing the consciousness of his situation. His bowels acted only once when in the pit, but he made water freely, both during his confinement and after his release.

The stimulant was occasionally changed for porter, sweetened with sugar, and whiskey toddy, both cautiously administered; arrow-root and oatmeal porridge were also given, but in very small quantities, as his appetite seemed entirely gone. The matter brought away by the injections was black, like meconium, and very fœtid. He continued to improve till the evening of Monday, the 2d Nov. when a change for the worse was observed.

The following was his state when I saw him on Tuesday noon:—His features were sharp and pale; his eye sunk; the parietes of the abdomen seemed to touch the back bone, which could be distinctly felt through them; his body generally presented more emaciation than I had ever seen produced by disease: he had altogether a dried appearance, very much like the natural mummies sometimes found in catacombs. His pulse was gone at the wrist; his voice a whisper, resembling the *vox cholerica*; respiration easy; tongue clean, moist at the edges, dry in the centre. He complained of uneasiness, increased by pressure, in the region of the stomach. He had no stools except from injections, which still brought away the black matter before described. His intellect was perfectly sound, and remained so till death. He never vomited, or had nausea. His pulse, on first coming out of the pit, was 60, moderately firm, and never rose much above this till Tuesday, when it rose to 70, but feeble. He was ordered half an ounce of port wine, made into negus, every hour, with a table spoonful or two of soup in the intervals.

He continued to sink, and died at 11 o'clock that night. No inspection of the body could be obtained.

It was scarcely to be expected that Brown could have recovered under any circumstances; though perhaps in an hospital, with a medical man in constant attendance, and the aid of experienced nurses, he might have lingered longer.

This case proves that delirium, so much insisted on by some writers, and particularly by the historian of the wreck of the Medusa frigate, is not a constant result of prolonged abstinence; that there is some truth in the aphorism of Hippocrates, which says, that when a person in health abstains from food for seven days, even though he received nourishment at the end of that period, he never survives; that thirst, not hunger, is the most distressing symptom. It also serves as a warning to workmen, in similar circumstances, not to despair of recovering their companions alive, though a considerable time may have elapsed.

It is possible that the air he respired was more pure than at the spot the workmen entered the gallery, as supposing the passage to slope upwards towards him, there might be a stratum of carbonic acid of some thickness, which, in a distance of sixty yards, would gradually disappear. An atmosphere slightly impure, by lowering the vital powers, might tend to prolong life under privation of food. The blackness of the stools might arise from the quantity of iron in the water he drank.

With regard to his measurement of

time, I may only allude to the power some people possess of estimating the lapse of time unassisted by artificial means.

ON THE

CURE OF PROLAPSUS UTERI.

By G. O. Heming, M.D. Glasg. F.L.S. &c.

The readers of the Medical Gazette will probably remember the case of Prolapsus Uteri which was published in the ninth volume, in November 1831, by Dr. Marshall Hall; and which was effectually cured by an operation which I performed at the suggestion of that gentleman.

This subject has recently occupied the attention of the surgeons in Paris, and several notices have appeared in the Reports of the Proceedings of the Académie Royale de Médecine, and in the Parisian journals, relative to it. I think an account of these discussions cannot fail to interest the members of our profession in England.

The first of these notices of which I shall give an account, is that of two communications made to the Académie on the 11th of August: one by M. Velpeau, the other by M. Bérard, jun.*

"M. Velpeau communicated the case of a woman, about fifty years of age, who had for a long time been affected with prolapsus uteri. There was also cystocele, pushing the uterus backwards. An operation was performed, according to the plan of MM. Marshall Hall, Heming, and Ireland, though with the following modifications:—In order to effect a considerable tightening, three shreds of the mucous membrane of the vagina were removed—one anterior, the other two lateral, beginning at the os externum; each of these shreds was ten lines in breadth, and two inches and a half in length. A difficulty is usually found, after removing the shreds, in making the suture. M. Velpeau took the precaution to fix the thread previously. The operation was not attended with any untoward event; the hæmorrhage and pain were very slight, and cicatrization ensued by the first intention. Some colic pains, arising from the retention of some fæcal matters, were subdued by laxative medicines. The patient was cured two months ago, and the cure promises to be permanent. The operation is at once free from pain or inconvenience, and appears most effectual. M. Maingault objected, that, in the case of young women, likely to become mothers, this operation would involve serious inconveniences. M. Velpeau observed, that this objection might appear well-founded at first sight, but that there were facts to shew that cicatrices of the vagina may yield sufficiently during labours to admit of the passage of the fœtus. M. Bérard, jun. related the case of a woman, in which he performed the same operation with entire success. Out of three instances, in which he had himself been the operator, two persons were completely cured."

Since that day, M. Bérard has repeated the same operation. He proposes to designate it by the term "Elytroraphie."

To this brief account of the proceedings of the Académie, I beg to add that of a Clinique by M. Velpeau, published by Dr. Dufresse, in the Journal Hebdomadaire, for August the 29th, tom. iii. No. 35, p. 275:—

"There came under my care (says the Professor) a woman, aged 58, of dark complexion and well formed, affected with prolapsus uteri to such a degree that this organ projected through the os externum. The cervix was neither inflamed nor ulcerated. The patient experienced no pain, and she could reduce the tumor herself.

This affection is rather an infirmity than a disease. It may, however, become a source of serious disease, in consequence of the dragging which is produced by it on the parts contained within the pelvis. It may also be the cause of peritonitis, inflammation of the cellular membrane of the pelvis, and consequently of abscess; and the intestines may be drawn into the *cul de sac* which is thus formed. It may also be the cause of strangulated hernia; and, lastly, of ulceration of the neck of the uterus.

The surgical means which have been hitherto employed are only palliative. They consist of pessaries, by the use of which the patient is subjected to numerous accidents. There is considerable variety in the form of these pessaries;

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* Archives Générales, tom. viii. Series ii.

some are oval, others elliptical, or "en gimblette." They are with difficulty retained in their proper situation, and are ill calculated to support the uterus. They imbed themselves deeply in the parietes of the vagina, and produce inflammation; in consequence of which it is frequently necessary to withdraw them. Others have the form of a "bilboquet." It has a stem, which projects out of the vagina, and produces much inconvenience when the patient sits down. This is often broken, and then the cup remains in the vagina, where it becomes covered with a calcareous concretion. Pessaries have also been known to perforate the recto-vaginal septum, and the sores thus produced to become fistulous. There are, besides, "pessaires élytroides." These are with difficulty kept in the vagina.

The insufficiency, then, of these means, and the inconveniences which follow their application, justify the efforts which have been made to obtain a radical cure; which has been effected in some cases.

There is one principle only, but this is effected in various ways. It has been founded upon that which is adopted for the cure of prolapsus ani. This consists in contracting the orifice of the anus to a greater or less extent. Hey was the first to adopt it in England, but M. Dupuytren, in France, described it with much more accuracy, and ought justly to be considered as the first who gave rules for its performance. Thus, from the success obtained in cases of prolapsus ani, it has led to a belief that the same benefit might be expected in cases of prolapsus uteri, by contracting the vagina.

The first idea of this operation is due to M. Girardin, who described it in a memoir which he presented to the *Société de Médecine de Metz* or *de Nancy*. He proposed to contract the vagina, and, if necessary, even completely to obliterate it, in women in whom the catamenia had ceased. He found many opponents to his ideas, which were rejected. The manuscript remains unpublished, the journal of the society having given only an analysis of it, which I have now before me.

These ideas have since been renewed, reduced to rules, and adopted with success. M. Dieffenbach, of Berlin, has employed this method. In 1831 many of our young surgeons went into Poland, and they addressed letters to the *Gazette Médicale*, in which they give an account of many cases of procidentia uteri, in which there had been performed an operation similar to that adopted by M. Dupuytren for the cure of prolapsus ani.

Two years ago M. Laugier tried to cure a case of prolapsus uteri which came under his care, by contracting the vagina. I believe this case has not been published.

M. Tanchou was acquainted with this method, but kept it a secret. He has published nothing upon it; he has only put in his claim against that of the English surgeons, who wish to consider the priority in reference to this operation as due to themselves.

M. Dieffenbach is contented with removing the folds of the vagina near the inferior orifice.

M. Laugier cauterized a broad strip of the mucous membrane with the nitrate of mercury.

MM. Marshall Hall, Heming, and Ireland, in England, have performed the operation with complete success. Their method consists in removing an elliptical shred of the mucous membrane from the internal surface of the vagina, an inch in breadth, and several inches in length. The wound is then united by the interrupted suture. In some cases two such shreds have been removed, one from the left, and the other from the right side.

In the case which I have described, the catamenia had subsided for a long time. The uterus projected two inches externally; the tissues were not hypertrophied, nor had they undergone any other alteration of structure.

The plan which we propose is as follows:—First to remove the anterior column of the vagina from the inferior part to the superior, before reducing the prolapsus; then to reduce it, and remove from the lateral parts of the vagina, both right and left, and along the whole length of this canal, a band of the mucous membrane; and, lastly, to unite the anterior wound by the interrupted suture.

In our opinion it is much better, when the procidentia is not very considerable, to insert the sutures before removing the anterior shred of mucous membrane, so that the threads may be situated about a line from the edges of the wound. We advise this to be done

in consequence of the pain causing the parts to contract, when the excision is made before the sutures are inserted; and in many cases when this happens it is exceedingly difficult to insert them. In the dissection of the anterior shred considerable caution is necessary lest the vesico-vaginal septum be completely cut through.

The woman who forms the subject of this communication experienced no inconvenience; and some days since she walked in the wards of the hospital, and in the gardens, without the least tendency to a return of the prolapsus. We may now, therefore, calculate upon almost certain success by an operation."

What I have to add is a little amusing. It seems M. Girardin has written to the "Académie," to remind its members that in 1822 he had proposed to cure prolapsus uteri. It is added that M. Girardin "a voulu par cette communication assurer à l'opération son origine, et maintenir à la *Chirurgie Française (!)* la priorité de l'invention, sinon de son execution*."

It seems that the profession in France remained steady in their opposition to this measure, and that M. Girardin slept upon his mere proposition. I do not imagine, therefore, that either will gain much by any attempt to deprive Dr. Marshall Hall and myself of whatever merit there may be in having both *devised and executed* this simple, almost painless, almost bloodless, yet important operation. To compare our operation to that of Hey, or that of Dupuytren, for prolapsus ani; or to those of M. Dieffenbach and M. Langier, for prolapsus uteri, neither of which *could* succeed, is equally futile. As to M. Tanchou, who could keep his mode of proceeding secret, he does not deserve a moment's notice.

I hope soon to transmit some other cases, with remarks upon the best mode of performing this operation, and upon the circumstances of age, &c. by which it might appear to be limited.

Without attaching too much importance to it, I cannot but regard this operation as a valuable addition to our curative means. Prolapsus uteri is frequently a great calamity. The mode of cure proposed is at once effectual, and free from either pain or danger if properly performed.

Manchester-Square,
Nov. 17, 1835.

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* See Gazette Médicale, t. iii. p. 538.

CASE OF HYDROPHOBIA;

WITH REMARKS.

To the Editor of the Medical Gazette.

SIR,

AMONG the various diseases incident to the human body, there is none more distressing and formidable in its aspect, or that more perplexes the skill and arouses the sympathy of the medical practitioner, than hydrophobia. In the present state of our knowledge it appears to be so obscured by difficulties, that the question very naturally suggests itself whether our opinions regarding the subject be correctly formed or not; and inasmuch as it proves so rapidly fatal in the majority of instances, it is deeply to be deplored by all that the malady is so little under the control of remedial art.

It is a most singular circumstance that the poison which becomes inoculated by the bite of a rabid creature should not, like other animal poisons, produce an almost immediate effect upon the constitution; but that the virus, on the contrary, should lurk in the system, and not give rise to the direful symptoms until the wounds themselves had fully healed, and the patient had banished from his mind all fear of danger. Such, however, is the fact; and it is not unreasonable to inquire how the virus acts so as ultimately to be the means of destroying life. *Is the matter taken up by the absorbent vessels, or does it exert its fatal influence through the medium of the sentient extremities of the nervous filaments?* The principles upon which each practitioner philosophises, and by which he is guided in the use or application of remedies, must depend upon the view which he takes with respect to the *modus operandi* of the poison; and this, in my humble opinion, satisfactorily accounts for the diversified methods of treatment which have been adopted by different practitioners. Upon a rational solution of the inquiry just now proposed, a subject of intense interest and importance hangs suspended.

The following case having lately occurred in my practice, I have forwarded it to the Gazette, not so much with a view of placing it on record, as with the hope of eliciting information relative to these points from some of the more ex-

perienced members of the profession. During the continuance of the case, it excited feelings of most painful interest; and I was excessively disappointed when the result proved the total inefficiency of the means employed, because I had every reason to believe that at one time they were signally beneficial in alleviating, if not in arresting, the progress of the more violent symptoms. It will be seen that, in consequence of the lacerated state of the limb, and the proximity of the wounds to the tendo Achillis, excision could not be performed to the extent usually recommended, without rendering the patient for ever a cripple; and the operation did not seem imperatively required, as the accident was generally attributed to the boy's having struck the dog upon the mouth while suffering from the effects of severe wounds. Had there been more unequivocal marks of the animal being rabid, I should at once have proposed amputation, as being the only means of ensuring life; and the issue has convinced me that such a measure would have been altogether unjustifiable. But the father of the boy was so satisfied that the dog was not in the state to which I have alluded, that he was led to express great anger that he was so cruelly destroyed.

Before concluding these remarks, I should like to inquire whether any of your readers are acquainted with *well-authenticated cases* of cure by excision when hydrophobia had *fully* made its appearance; and how far I should have been justified in resorting to amputation after my patient had manifested all the symptoms of this most dreadful malady.

With every wish for the continued prosperity of your journal, permit me to subscribe myself,

Your obedient servant, and
constant reader,
CHARLES THORNHILL.

Darlaston, Staffordshire,
Nov. 13, 1835.

Edward Lloyd, ætatis nearly 11, was bitten, on the 16th of June, by a dog, which was supposed by some to be rabid. The dog belonged to the father of the child, and was usually of quiet disposition. He had been fighting the previous day with a badger, and during the contest had received several severe wounds about the head and neck, from which he recovered with difficulty. He had always been allowed to run at large, and up to the day of the fight was attached to the different members of the family; but on the morning of the accident he was observed to be rather irascible, and to snap at the various articles of furniture that were arranged about the house. In the afternoon, whilst the little boy was sitting at the kitchen door nursing his infant sister, the dog advanced towards the threshold, and on entering the house, stopped to lick the face of the young child. The boy, fearful of consequences, motioned him off, and in so doing, hit him upon one of his recent sores; this so infuriated the animal, that he turned upon and seized him by the leg, which he shook and lacerated in the most shocking manner. When driven off, the dog took shelter in the house; but being compelled to quit his retreat, he ran out and scampered across the fields. He was, however, quickly pursued by the neighbours, and in the course of half an hour was dispatched by a gun-shot.

Two hours after the accident, I was requested to see the boy. The leg was literally covered with wounds, both before and behind; but the principal bites were at the lower part of the calf. A portion of the tendo-Achillis was laid bare, and that part of the gastrocnemius muscle in connexion with it was very much torn. Several loose fragments of muscle were hanging about the leg, and the wounds in general presented a ragged appearance. After freeing them of all adventitious portions, and excising them where it could be done with propriety, I dressed them very freely with the nitrate of silver, covered them with digestive ointment, and strapped them over with adhesive plaister. The dressings were changed every day. The sloughs produced from the application of the caustic were considerable. The caustic was reapplied on several subsequent occasions. The wounds healed kindly; and at the expiration of five weeks the leg was perfectly free from sore.

From the time of the accident to within a few days of the occurrence of the fatal malady, the patient continued to enjoy himself in the fields about home, and seemed fond of frequenting the banks of the canal, where at times he was engaged in fishing for hours together. I have since learnt from his mother that he had recently bathed in the canal, and whilst therein, was seized with a peculiar constriction of the throat, which occasionally returned for some days afterwards. My brother met him in the fields on the 4th of September, when he appeared to walk lame on the leg which had been bitten; and as we had not seen him for more than a week, he was desired to walk up to the surgery the following

morning. His mother came with him, and stated that he had been rather heavy and sleepy during the last day or two, and that his appetite had become more capricious than ordinary. The boy did not appear so lively as usual, and complained of slight pain in his leg, but as yet the symptoms were not sufficient to excite alarm or apprehension in the minds of his relatives. I, however, ordered him as follows:—

℞ Hydrar. Submur. gr. iij.; Pulv. Scammoniæ, gr. iv.; Pulv. Jalapii, gr. vi. Misce. Fiat pulvis. Statim sumendus.

℞ Ammon. Carbon. ℈ij.; Sodæ Carbon. ℈iv.; Confect. Aromat. ʒss.; Misturæ Camphoræ, ℥vj. M. Capiat cochlearia duo 4ta quâque horâ.

11 P.M.—A message was sent to request my attendance. I was engaged at a midwifery case at some distance from home, but my brother went immediately to see him, and called to communicate with me on his return. The boy had been delirious for some hours, and had been attacked with tetanic twitchings during sleep. The mouth was fast locked, the hands were clenched, and his arms tossed about with great vehemence. He had complained of severe pain across the forehead, but his mother had firmly bound a handkerchief round the temples, which afforded a speedy relief. The following was prescribed:—

℞ Pulv. Opii, gr. vj.; Hydrar. Submur. ʒss.; Confect. Rosæ. Caninæ.
Pil. vj. Capiat i. omni horâ.

Being liberated between three and four o'clock on the morning of the 6th, I hastened to visit him in company with my brother. The boy was much agitated on hearing us go up stairs. On our entrance into the room he fetched a heavy sigh, and attempted to hide his face underneath the bed-clothes; at the same time a convulsive motion took place in the upper part of the body, and particularly about the neck and throat. The countenance was distrustful, and indicative of great anxiety and distress. The eyes were much sunk, and perpetually rotating in their sockets, while both pupils were dilated almost to their fullest extent. A little frothy saliva was discharging itself at each angle of the mouth; and the tongue, *which was protruded with a sudden jerk*, was white, and covered with a viscid tenacious mucus. The breathing was hurried, and the inspirations were remarkably quick and short. The pulse was weak, and beating at about 75 in the minute.

It was with the greatest difficulty that I could get him ro respond to any question; for the attempt to speak occasioned him to make a deep sigh, which produced a recurrence of the spasms. On suggesting that he should swallow a little water, he seemed to be frightened, and began to cry out. He turned suddenly in bed, and was simultaneously seized with a momentary clonic spasm of the trunk, greatly resembling *emprosthotonos*; however, by kindly encouraging him, he soon manifested a willingness to accede to my wish; but the sound of the water, as it was poured into a tea-cup, again brought on a similar convulsive action. The very sight or mention of water appeared to excite uneasiness. At length, by putting a little into a spoon, he raised himself up to drink it; but when he had received it into his mouth he was unable to swallow it, as spasm came on, and obliged him to expel it from his mouth to a considerable distance. With difficulty he managed to swallow a small pill in my presence. His hands and arms were copiously bedewed with a cold clammy moisture; but the heat in other parts of the body was not reduced below the natural standard.

On interrogating the mother, I found that the first circumstance which attracted her notice was his repugnance to the sight of water. Having occasion to wash the kitchen on the previous evening, she carried in a pail of water for that purpose, when he was attacked with spasms and convulsive sobbings. As these symptoms did not cross her mind as being of an urgent nature (for they disappeared as soon as the water was removed), she did not deem it necessary to apply for assistance; nor did she feel the slightest alarm until he had become delirious and ungovernable. I also learnt that, on his return home from my surgery, he called at a house to get a little water, but on placing it to his lips he found that he could not drink it.

The pills were ordered to be continued every hour, and the following embrocation to be freely rubbed upon the neck and chest on each recurrence of the spasms:—

℞ Ung. Hydrargyri, ℥iij.; Tinct. Opii, ʒv.; Linim. Saponis, ℥j. M. ft. embrocatio.

9 A.M.—Symptoms much the same as at the last visit. His abdomen was distended, and somewhat tense; he was perfectly sensible and rational, but the head was heated, and wet with perspiration. He showed an unwillingness to submit to the employment of friction, and threw himself about in the most violent manner, sometimes tossing back his head against the bed-board, and at others enveloping himself in the bed-clothes. At every motion he was seized with convulsive twitchings. For some time he strongly resisted every effort that we made to keep him quiet; but

after being assured that he would not be requested to swallow any liquid, he at length yielded, and allowed himself to be rubbed with the liniment.

11 A.M.—Convulsions not so strong as at the last visit; countenance not so wild, and less expressive of pain and distress. Pupils less dilated. He was fully alive to every thing that was going on in the different parts of the chamber; the sense of hearing was morbidly acute, and the slightest noise or movement of any kind made him restless. When questioned he complained of no settled pain; but on pressing gently with my finger upon the upper part of the trachea, pain was produced, and spasms of the neck simultaneously occurred. He was not so much agitated as heretofore at the sight of water; and, on making the attempt, was enabled to swallow nearly a tea-spoonful at a time, though it required considerable effort, and was attended with an involuntary inspiration and spasm. When water was put to his lips he was amazingly eager to partake of it; but in drawing it hurriedly into his mouth, the face became contorted, and the head was drawn back to some distance.

Rep. Embrocatio.

3 P.M.—The spasmodic action of the muscles has not been near so violent or frequent since the friction has been regularly employed; and the patient states that he feels himself much better. His countenance brightened up when he saw me approach his bed-side, but still it is wild and unnatural. His tongue is moist, and stained by some kind of preserve which he has been eating. For the last three hours his thirst has been excessive; and during this time he has drunk, in small quantities frequently administered, at least a pint of toast and water. He is evidently under the influence of the opiate which has been so freely exhibited; and the tetanic symptoms have been materially diminishing for some time.

Having some professional engagements during the remainder of the afternoon, and the patient residing rather more than a mile from me, I was not enabled to visit him again until between six and seven o'clock, when, to my great astonishment, I ascertained that he had been dead nearly half an hour.

It appeared from the account which I obtained, that at about four o'clock the convulsions returned with redoubled violence, during which he made frequent attempts to injure himself; and, to use the remark of one of the attendants, "if he had not been held down by two or three of those who were about him, he would have torn himself to pieces." After having continued in this state for about an hour, he sunk exhausted upon the bed; nor did he move from the position which he had assumed until death released him from his sufferings.

It was much to be regretted that a post-mortem could not be obtained.

ON THE NATURE OF THE NERVOUS INFLUENCE;

IN REPLY TO THE OBSERVATIONS OF DR. WILLIAMS,

In the last Number of the Medical Gazette.

To the Editor of the Medical Gazette.

SIR,

I THINK Dr. Williams will, on reflection, admit that a reference to the opinions of others is no reply to a position which professes to be a legitimate inference from acknowledged facts.

In the first part of his reply, Dr. Williams refers to the observation of Dr. Alison, that "the province of the nervous system in producing the phenomena of life, was more accurately understood by Haller" than in the present day. Do we not at present possess all the knowledge of Haller, with whatever has since been obtained in addition to it? And in the immediately preceding part of the same sentence, Dr. Alison is directly contradicted by Dr. Williams himself; who observes that no one denies that succeeding experimental researches "have greatly increased the precision and extent of our knowledge in these matters."

Dr. Williams then enumerates the opinions of various writers, which can be of no avail in the question before us. The facts and the reasoning founded on them are placed before him. His reply, to have any weight, must either disprove the facts, or point out in what respect the reasoning is fallacious. Whatever relates not to one of these heads, tends only to obscure the truth, and therefore increase our difficulties. The question is not what opinions have prevailed, but what is the true opinion; a point not to be determined by authority, or the majority of voices.

With regard to what Dr. Williams says respecting the diminution of temperature produced by destroying portions of the spinal marrow, depending on an effect produced on the circulation, it appears from M. Le Gallois' experi-

ments, and still more fully from experiments detailed in my Inquiry into the Laws of the Vital Functions, that the gradual destruction, whether of a part or the whole of the spinal marrow, the mode adopted in the experiments I refer to, produces no effect whatever on the circulation. "The only apparent effect of the operation," that is, of destroying the lower half of the spinal marrow, "was the loss of power and feeling in the lower part of the animal: it seemed to be otherwise in health." (Page 138, third edit.) It is only by the sudden destruction of this organ that the circulation is affected: the cause of which it is not difficult to explain.

The great peculiarity in the functions of the ganglionic nerves is, that each nerve, in consequence of its connexion with a chain of ganglions, which receives nerves from every part of the brain and spinal marrow, conveys the influence of every part of those organs, which it appears from direct experiment, is necessary to the healthy state of the assimilating functions in every part of the system. Hence it is that an effect produced on any minute part of these organs, is too trifling to be in the least degree felt, either in the circulation or any other of those functions. I found that the most powerful agent which could be applied to a minute part, either of the brain or spinal marrow, had no sensible effect either on the heart or vessels; while the slightest cause affecting any considerable portion of them, not only influenced the action of the heart but that of the capillary vessels also, even in the remotest parts of the system.

Now when any considerable portion of the brain or spinal marrow is suddenly destroyed, the cause at once affecting a large portion of these organs, the action of the heart and vessels is influenced in proportion to the extent of the part destroyed; and if the whole brain or spinal marrow be thus destroyed, it instantly ceases. But if either of those organs be even wholly destroyed very slowly, so that the offending cause acts on minute parts in slow succession, no effect whatever is produced in the action either of the heart or capillaries. The effect is then the same as when the brain and spinal marrow are simply removed, which produces no effect whatever on the circulation.

It was farther ascertained that the power of the capillary vessels has no dependence on that of the heart, but for a supply of blood. Till the supply of blood from the larger arteries begins to fail, which is not for a considerable time, the action of the capillaries remains unimpaired even in the newly dead animal, provided its death has been occasioned neither by loss of blood, nor a cause which suddenly destroys the brain or spinal marrow, although a ligature be thrown round all the vessels attached to the heart, and this organ removed.

With regard to the effect on the temperature, of bruising the semilunar ganglion, or throwing a ligature round the aorta, it could easily have been foretold that either would greatly impair it: because the one deprives the blood of a great part of the nervous influence, by the action of which on this fluid animal temperature is maintained, and the other prevents the due supply of the blood itself; and with regard to the blood, even out of the body, still for a short time in some degree possessing the power of maintaining its temperature—that is, while its vitality and the influence it has derived from the nervous system remain, and a supply of oxygen is afforded: this also is only what we are prepared to expect from every thing we know of the cause of animal temperature.

The researches of Dr. Faraday enable us to place the question respecting the nature of the nervous influence in a very simple point of view. If we deny that the nervous influence is a modification of electricity, we must either maintain that the chemistry of the living animal depends on a principle altogether different from that of inanimate nature; for example, that the principle on which depends the combination of oxygen and carbon in the blood, is altogether of a different nature from that on which the same combination depends in the laboratory of the chemist, although the resulting substance is in both instances the same; an opinion which, I think, Dr. Williams will allow to be too extravagant even for the most visionary; or we must refute the electro-chemical doctrine of Dr. Faraday, according to which, chemical changes are necessarily the effect of electric action; which will not be found an easy task.

How strikingly is this simple statement illustrated by the experiments which prove that all the functions of the nervous influence, many of which are of a complicated nature, may be as effectually performed by voltaic electricity, operating under the same circumstances in which the nervous influence operates, as by that influence itself, and that this influence can be made to pass through other conductors than the nerves. Experiments the results of which will hardly be questioned, when it is stated that they were publicly repeated, both in London and Paris, and witnessed, and their accuracy admitted, by Sir Humphry Davy, Mr. Andrew Knight, Sir Benjamin Brodie, MM. Breschet and Edwards, and others of the most accurate physiologists of our day.

Is there any physiologist so hardy as to maintain that a vital principle, properly so called, can exist in any texture but that to which it belongs in the living animal; or that, in its functions, any of the powers of inanimate nature can be substituted for it?

Shall we admit that voltaic electricity can perform all the functions of the nervous influence, and yet maintain that the two principles are of a different nature; while, at the same time, we allow that we have no knowledge of any principle of action but by its properties?—for what I have said on this subject we shall find is admitted by Dr. Williams.

The more successfully we study the operations of nature, the more their simplicity appears. In what instance do we find the same phenomena originating from different principles? Nay, do we not find the most dissimilar phenomena originating from modifications of the same principle? What error of reasoning can be greater than to maintain that the properties being the same, the principles are different?

The only difficulty here, has arisen from our having confounded the sensorial and nervous functions. The moment they are distinguished, all difficulty disappears. The nervous functions, properly so called, although modified by the vital principle, are all more or less of the same nature with those effected by electricity in the inanimate world: they are all evidently the effects of a chemical agent.

PHYSIOLOGY is much indebted to those who have shewn that the nerves of motion and those of sensation, although often inclosed in the same sheath, have different origins.

It is evident that the ganglionic nerves differ from both these classes of nerves, their most evident characteristics being their connexion with the ganglions, and their being more peculiarly the nerves of the vital organs; to the whole of which, it appears from experiment, where the parts are too minute for the labours of the anatomist, they are distributed. It will tend to elucidate what has been said in the present communication, to enter more minutely into the nature of these different classes of nerves. Without this, indeed, it is impossible to have a clear view of the whole line of distinction between the sensorial and nervous functions.

It appears from what is said above, that each of the ganglionic nerves, after they leave the chain of ganglions, conveys the influence of every part of the brain and spinal marrow. This is the distinguishing property between these nerves and the two former classes of nerves, the influence conveyed by each of which, were it not for too much prolonging this communication, it would be easy to show has relation only to particular parts of the brain and spinal marrow. But the question which is of most consequence to the practical physician, is the nature of the influence conveyed by these different classes of nerves.

From the foregoing statements the conclusion appears unavoidable, that the influence conveyed by the nerves of motion and the ganglionic nerves is voltaic electricity; and we know from the phenomena of electric animals, that the nervous system is capable of collecting and applying, even according to the dictates of the will, the electric power.

Now let us try by the same test, the only one by which it can be tried, the nature of the influence conveyed by the nerves of sensation.

What are the properties of the influence conveyed by this class of nerves?

It is necessary, in order clearly to understand the answer to this question, to observe, that the nerves of sensation, (in which are included, of course, the nerves of the external organs of the senses) and those parts which are the imme-

diate organs of the sensorium, are not parts of a whole, but distinct parts; for they have distinct localities, and functions of a wholly different nature; that is, the sensorium does not pervade the whole system, but belongs to particular parts. In man these parts are almost wholly confined to the brain, sensations being referred to particular parts of the body by experience alone; so that infants are not aware of the part of the body in which the cause of any particular sensation originates; and when a limb has been lost, at whatever part the separation is made, we still continue to refer to the lost part sensations excited by causes affecting the nerves of the stump.

The following points, then, are made out from the phenomena of every day's experience, that the organs of the sensorial powers, and the nerves of sensation, are distinct parts; the former being the immediate organs of these powers, the latter the organs which convey the influence which excites them.

We have seen that the influence which excites the muscles and maintains all the assimilating functions is an influence which operates in the external world.

Are the properties of the influence conveyed by the nerves of sensation the same with those of any of the principles which operate in that world?

It is enough to say that the only property of this influence is its co-operation with the immediate organs of the sensorial power. To such a property we find nothing analogous in inanimate nature. The nerves of sensation, therefore, belong to the sensorial, not the nervous power; and the only analogy which can be traced between their function and the operations of inanimate nature, is in that property, by which the impression they receive is propagated along them, — a property which they owe to their capability of being excited by agents belonging to inanimate nature, and to which we are indebted for all our knowledge of the external world. The action of the immediate organs of the sensorial power, on the other hand, being excited by one vital part acting on another, and by its vital properties alone, all analogy with inanimate nature here disappears.

While all the other functions are the results of inanimate agents acting on vital parts, or vital parts on them, the sensorial functions are the effects of vital parts acting on each other. Hence the analogy between the former and the operations of inanimate nature, and, with the exception just pointed out, the total loss of all such analogy in the latter.

The foregoing statements being satisfactory to my own mind, and to the minds of those with whom I would not presume to compare myself, I have no doubt that time, as in other similar cases, will remove the confusion which has arisen from the hasty opinions of those whose prepossessions, and in many instances whose habits, but ill prepare them for the consideration of such a question; and here, unless Dr. Williams should adopt very different grounds, I leave the discussion*.

I am happy to observe that Dr. Williams and myself are agreed respecting the second part of my last communication, but with regard to the quotation he, in the latter part of his present reply, gives from that communication, he will recollect that I have explained what I mean by a knowledge of the nature of any principle — namely, a knowledge of its properties; and that we cannot, in this sense, say that we are ignorant of the nature of vitality.

I am, sir,
Your obedient servant,
A. P. W. Philip.

Cavendish Square,
Nov. 17, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

An Inquiry, Physiological and Pathological, into the Proximate Cause of Cholera. By Protheroe Smith, Member of the Royal College of Surgeons, &c.

There was a time, and that not so very long ago, when books, tracts, pamphlets, and broadsides, on cholera, came upon us thick as the leaves in Vallombrosa. The case is altered: an essay on that well-discussed subject has now

* From a passage in an early part of Dr. Williams's paper, he appears to think that an expression in my last paper was meant to apply to him. I can assure him that this was so far from being the case, that if I had conceived that such an idea could have occurred, I should have worded it in such a manner as to render so unjust an application impossible. I spoke only of general and natural biases of the mind, as I do in the paragraph to which this note is appended.

become as rare as green peas at Christmas. Whether it has happened that publishers have grown tired of the speculation, or authors found themselves run dry, we know not; but we rather congratulated ourselves that nothing in the shape of cholera—either the disease itself, or books about it—visited us of late. Yet when the present pamphlet came upon us, a few days since, we had the curiosity to look into it, and were glad to find that it had much more than its mere rarity to recommend it. There is a good deal of merit and ingenuity in the author's attempt to ascertain the "proximate cause:" he describes what he himself witnessed, and even experienced in his own person; and he proves sufficiently that his opportunities of observation were not misapplied.

As far as we can gather from a hasty perusal, the "proximate cause" of cholera, according to Mr. P. Smith, consists chiefly in the state of the nervous centres: the symptoms, almost from the very setting in of the disorder, may, he thinks, be traced to the reaction consequent upon a strong impression made at an early period on the nervous system. In accordance with this theory, the several stages of the disease are pourtrayed, and cases adduced which tend strongly to support the author's views.

The cases given in the pamphlet have a more than ordinary interest, owing to the fact of their being among the first—one of them said to be the very first—that appeared within the walls of London: so that even in a historical point of view, the author acted judiciously in preserving his notes, and laying them before the public.

MEDICAL GAZETTE.

Saturday, November 21, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PAUPER POLICY OF THE LEGISLATURE.

MR. BUCHANAN, his Majesty's Consul at New York, not long since broached a project, which, were it put in execution, would spare certain functionaries a deal of trouble, for which they are not peculiarly fitted, and the public generally not a little annoyance and vexation. He proposed that a depôt should be formed in Upper Canada, where the whole pauper population of England might be disposed of. The worthy Consul was serious, and full of benevolence towards his poorer fellow-men; he had no intention whatever of making them any thing but comfortable by the change. His plan may seem at the first view harsh and summary; but surely if the "be-all and end-all" of legislation about the matter be the cheap and humane disposal of the pauper class, it were a thousand times better they were deposited in the wastes of Canada, than pinched and ground down to the starvation point by Poor-Law Commissioners, the very tenure and condition of whose office is, that they should "save to the greatest possible extent in the expenditure on the wants of the poor."

How long will the legislature of the country be guided by this "penny wise and pound foolish" policy in matters of so momentous a nature? for to them we must attribute such a blind and unwise proceeding, when we find them intrusting the fullest discretionary powers regarding the lives of his Majesty's poorer subjects to men who, utterly regardless, or incapable of looking around them, cannot see that there are circumstances in which even an *increase* of expenditure might not only be just to the sufferers, but *ultimately beneficial* to the community.

Our valued correspondent RURICOLA, in his letter last week, set this subject in a clear light; and we are happy to introduce him again to our readers in the present number, following out some of his details.

He stated in his former letter some

facts which are deeply deserving of attention. The extent to which medical men are employed among paupers, throughout the parishes of England, and the treatment which they receive in return, would scarcely be credible, were it not vouched for by so well-informed a writer. Above *a million* cases of sickness annually pass through the hands of the medical attendants of the poor; and what do our professional brethren earn by such ample occupation? Only the scorn, we fear, and the contempt, of the Poor-Law functionaries. " The *parish doctor*, exposed to the summer's sun and winter's blast, toiling by night and day, on a pittance that a footboy would spurn, appears to be as fair an object of their animadversion—perhaps I may add," says Ruricola, " their unwarrantable vituperation—as the overseer who was accustomed to pay his own bills from the rates, or the able-bodied impostor who bullied the frightened farmer into a handsome weekly allowance for doing nothing."

In such a state of things it is clearly hopeless to expect redress, except through the interference of Parliament, and that chiefly by a legislative measure, fixing the remuneration of parish medical officers. The system of *contracting* for medical attendance, and of procuring through " tender" a set of needy adventurers to undertake the charge of the sick through large and densely-peopled districts, must be abandoned, as unjust to the unfortunate poor, and degrading to the respectability of the profession. But how is this desirable consummation to be effected? To hope for a spontaneous change from the functionaries who have doggedly persevered in their plans, were weakness and rashness in the extreme. The co-operation of the profession at large throughout the country affords the only prospect of effectual remedy; while the method of exposure (through the aid of the press), of the actual working of the system, cannot but be productive of much good. We also anticipate important results from the formation of the Committee which we announced a few weeks ago. The answers to the queries proposed by that body, will, we doubt not, contain a mass of evidence, the weight of which, we hope, will be irresistible.

Meantime facts of a deplorable nature are daily being brought before the public, illustrative of the blessings of the new legislation. Even as we write, a letter appears in the *Times* newspaper (November 18th), detailing a case of crying hardship. The author of the letter gives his name—Mr. Hempson Denham, of Wickam-Market, Suffolk: we extract some of the details. The circumstances took place at Petistree. On the 5th instant (Thursday), a poor child was taken ill; and not getting better, the mother on the 8th (Sunday) applied to the gentleman who had been parish-surgeon previous to the passing of the Poor-Law Amendment Act. He gave her some medicine, but mentioned, that as he was no longer parish-surgeon, she would have to apply to Mr. Welton, of Woodbridge, who had obtained the appointment. On the following morning, (Monday,) the father of the child applied to Mr. Walker, the guardian of the parish, but was told, at first, that no assistance could be given, as he was not on the parish books: ultimately, however (but not till another day was lost), he was referred to the *relieving officer*, who lived two miles off. Upon going there, the officer was not at home, but some of his relatives advised the father to go to Mr. Welton *without* the order. Application was therefore made to the latter gentleman about 8 o'clock in the morning, and he was requested to see the child immediately, as it was extremely ill. Mr. Welton said he would attend, but neglected to do so. " The

child continuing rapidly to get worse, the anxious mother, about three o'clock in the afternoon, hastened to the father, who was threshing in a barn about half a mile from his house; who then obtained a horse, and went again for Mr. Welton, and between five and six o'clock in the evening he visited the child. The father had again to go to Woodbridge, the same evening, for the necessary medicine; thus making the poor man travel, at a moderate calculation, 22 miles for the doctor and medicine. Mr. Welton directed the parents to let him know early the next morning (Wednesday) how the child was. The father accordingly got to his residence by eight o'clock, but was detained there till ten, without seeing Mr. Welton, who told him, *if* the child was alive when he returned, to have it put into *a warm-bath.* Death, however, had rendered this unnecessary: the poor child had died soon after the father left home!"

And this is the system of parish *relief* which at present prevails in England, under what is called an *amended* poor-law! There was no inquest neither, it appears; though most undoubtedly there ought to have been one—when a jury of honest Englishmen might have an opportunity of expressing their opinion on the gross grievance, the criminality, attaching to the originators of such measures. But these are points which perhaps rather belong to the province of the general press; our field is limited to considerations more strictly of a professional nature, and our object now is to inquire how this odious system may be altered in any way for the better; preserving, of course, for all medical men concerned, their respectability at least, together with a more equitable chance of adequate remuneration. Viewing the subject in this light, we confess we cannot suggest any thing preferable to the mode pointed out by Ruricola, for ameliorating the condition of parish medical officers. We have every confidence in the thorough information which our correspondent possesses, and we look upon his suggestions as founded on actual experience—the best earnest of their practicability.

In the letter which we publish to-day there are several valuable hints, which we hope will attract the attention and support of the profession generally.

The scheme for a fair remuneration of parish surgeons, for example, seems one that by all means ought, if possible, to be carried into effect. What can be more equitable than that the payment of the medical attendant should be arranged according to a fixed scale—the amount depending on the population of the parish, its density, and the proportion of the pauper to the other inhabitants? The position also of the surgeon within the sphere of his duties, or his residence *at a distance*, should be taken into account; as well as a liberal allowance made for his outlay in providing proper medicines, and his time and trouble in their application.

The case above narrated by Mr. Denham exemplifies, in a marked manner, the mischief—oftentimes fatal—which may arise from the present mode of procuring and granting *orders.* The niggardliness, combined with the obstinate ignorance of a *relieving* officer, may be productive of irreparable injury.

We see, in the remedy proposed by Ruricola, nothing but what is reasonable and proper. "The person to whom all applications for medical relief should in the first place be made, is *the medical officer himself*, and he should be authorized, in cases of emergency, to give immediate assistance; but as he cannot be expected to judge of the pecuniary circumstances of the patients, they should be required, within a specified time after their application to

the surgeon, to send to the relieving officer for an *order*: nor should this functionary be required to give it, unless the circumstances of the applicants entitled them to parochial relief, and unless they produced a certificate from the medical officer, stating that their cases needed attendance."

By following so simple a plan as this—which, after all, is only that which is commonly practised at most hospitals and dispensaries—precious time, in the incipient stages of disease, would be saved—humanity would be consulted, in sparing the bandying-about which too frequently falls to the lot of the unfortunate patient before he can have a chance of being relieved—and we question if it would not ultimately prove to be the cheapest plan, if the Poor Law authorities would but take the trouble to bethink themselves, and give it a fair trial.

In all that our correspondent says respecting the summary and uncivil manner in which medical opinions are at present sought by the Boards of Guardians—namely, by *summoning* the medical officers to give their special attendance, sometimes on the shortest notice—we cordially concur. It has long appeared to us a degrading and unworthy mode of proceeding, and one which should be protested against in every case — if not wholly scouted. Why not make the medical officers *honorary members* of the Boards, and thus treat them as men of liberal education—or even as respectable members of society? The only possible objection to this might be that questions are sometimes entertained by those Boards respecting the *conduct* of the medical officers. But, as Ruricola says, in such cases—if common delicacy would not prompt them (the medical men) to withdraw, it would be easy to provide a rule for their temporary exclusion on such occasions.

We scarcely dare hope, however, that a reform of the system to this extent will ever be effected, as long as the body of the profession acts without concert; as long as a number of its members are found acting upon a selfish principle; and above all, as long as the Boards can find individuals not only acceding to the principle of "tender," but degrading themselves to the rank of menials and humble retainers. To persons of other callings and professions, who are not immediately conversant with parish proceedings, it must seem incredible that medical men should be subject to such petty humiliations; they cannot believe that men liberally educated would stoop to such unworthy treatment, or accept the wretched pittance offered to them—almost as if they were themselves paupers. Surely the length of time spent in this miserable bondage must have wrought a sad change in the body professional, when the iron entering into the soul is not felt, or, if felt, is endured without a murmur.

EXPOSURE OF MATERIALISM.

In our last number we left Mr. Roberton to speak for himself; and those who read his first able letter, will not need prompting to move them to a perusal of the second. But such readers as may not yet have opened that part of the journal (if there be any such) will find a rich treat in store for them in the admirable pieces of criticism to which we allude.

PARIS SCHOOL OF MEDICINE.

The celebrated M. Broussais delivered the introductory oration on the recent opening of the Faculty of Medicine in Paris, and appears to have been received with no small degree of enthusiasm by his audience. After an eulogy on Dupuytren, and some details regarding

the establishment of the new Museum of Anatomy which now bears the name of the illustrious deceased, he proceeded to give a sketch of the different medical theories which at present prevail. The gist of his discourse was to advocate free discussion, and may be comprised in one of his phrases—"*la polemique est l'âme de la science.*"

LECTURES

ON

SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

BY DR. LATHAM.

ON SYMPTOMS.

Direct Symptoms of Diseases affecting the structure of internal Organs, known chiefly by means of Auscultation—Modes of Auscultation—Preliminary Acquaintance with Morbid Processes essential to its successful Use—General estimate of its Value—General Directions for its Use—Auscultatory signs of Healthy Lungs—Auscultatory signs of Diseased Lungs; known in the acts of breathing, speaking, and coughing. These sounds consist in dry *sounds and* moist *sounds—Rhonchus, Sibilus, Crepitations.*

RECOLLECT for a moment the inquiry in which we were engaged, and how far it has proceeded. It is an inquiry into the nature of symptoms; and hitherto we have spoken of symptoms *directly* referable to the part affected; those which regard its sensations, and those which regard its functions; and we have endeavoured to estimate the information which these are respectively calculated to convey. We have spoken also of those which directly regard its structure; and are now considering the information to be gathered from them.

This information, it should seem, is necessarily very limited in regard to internal organs in general; but the thoracic organs are excepted from the rest, because, being within the cognizance of another sense, and thus subjected to a method of investigation peculiarly applicable to themselves, all their actions and conditions are more clearly perceived and known. This is the method of auscultation.

There are different modes of performing auscultation. In one mode we apply the ear itself to the surface of the chest: this is (what is called) immediate auscultation. In another, we apply the tube to the chest, and the ear to the tube: this is mediate auscultation. In another, without applying the ear to the chest either immediately or mediately, we strike its walls with our fingers, and listen to the sounds which result: this might be properly called auscultation by percussion. But percussion and auscultation are often spoken of, as if they were different things, whereas they are only different modes of appealing to the same sense; for we gather our information equally from what we hear, whether we strike the chest, or apply our ear to it, or use the instrument.

I have often taken occasion to point out to you the importance of pathological knowledge to the just diagnosis and the successful treatment of disease; and, as a part of pathology, I have laid especial stress upon the knowledge of morbid processes. The use which you will or will not be able to make of auscultation, will depend upon your knowledge of the pathology of those organs to which it is applied.

The sounds which reach the ear through the walls of the chest during breathing, or speaking, or coughing, varied and modified by divers diseases of the organs within, are easily discriminated. Any person not deaf will soon learn that there is some distinction of these sounds. But we may distinguish them correctly, and call them by right names, and make a musical scale of them, if we please, and still know nothing of the morbid conditions which they indicate, and out of which they arise. These cannot be discovered by a discriminating ear *only;* they must first be known what they are in themselves. By means of auscultation, various diseases of the heart and lungs are capable of being detected with wonderful certainty; but the power of so detecting them belongs to those only who have studied these diseases in all the processes of their formation, and progress, and results.

It is with hearing as it is with the other senses. When they are taxed to give intimations to the mind concerning the objects by which they are impressed, it is necessary that the mind should have a previous knowledge what those objects are. Place a man within sight of London, and give him a telescope, and tell him to look for St. Paul's. St. Paul's he will undoubtedly see, and many a striking object besides; but he will not be able to distinguish it from Westminster Abbey or the Monument, unless he is previously instructed what sort of building St. Paul's is.

A child will at once perceive a difference between the fragrance of the vio-

let and of the rose; but it must know the violet, and know the rose, and smell them both singly, and by turns, before it can assign to each its peculiar sweetness.

Any man can discern a difference between the sound of a trumpet and of a drum; but he must have been where trumpets have been blown, and drums been beat, ere he can tell which sound belongs to each. My voice is different from yours; but a man must be familiar with you and with me, and have heard us speak a hundred times, before he can distinguish us by our voices.

So diseases of the chest have, as it were, different voices; but we must first be familiar with the diseases themselves, and then be accustomed to hear them speak, ere we can tell one disease from another by its voice.

What *are* diseases of the chest? Pneumonia, pleurisy, phthisis. And *do* we mean that auscultation can distinguish each of these from the other? Yes, truly; and we mean more than this—much more.

Pneumonia, pleurisy, and phthisis, are only the complex of several morbid processes and results. There is no such thing as a pneumonic, a pleuritic, or a phthisical sound. Pneumonia, pleurisy, and phthisis, have no sounds that are peculiar to themselves *as such*: but the sounds that we hear in these diseases result from certain morbid processes going on, and certain changes wrought upon the structure of parts; which processes and changes make up the complex to which we give a name. We hear the sounds denoting that this part of the lung is loaded with fluid, that part condensed with solid matter, and another hollowed with cavities. Thus we get at inflammation; thus we get at phthisis. We anatomise by auscultation (if I may say so), while the patient is yet alive, the very processes and changes of structure of which inflammation and phthisis consist: and so of other diseases.

Auscultation professes to make us acquainted with the actual condition of the lungs in many of the most important diseases incident to them—their actual condition at any *particular time*, and their changes from one condition to another *from time to time*.

I am not aware that, before auscultation lent its aid to diagnosis, we could do more than speak generally concerning the diseases of the lungs during the life of the patient. We could affirm generally that the lungs were inflamed; and, knowing, from our acquaintance with morbid processes, that it was the tendency of inflammation to produce such and such changes of their structure, we were aware what perils it involved, and could anticipate with tolerable accuracy what we should meet with when the patient died. So, too, we could affirm generally that there were tubercles or vomicæ in the lungs, and, understanding the forms and processes of phthisical disease, we could foretell in the main what we should find after death.

But auscultation anticipates the disclosures of morbid anatomy. Nearly all that dissection can unfold, it tells while the patient is yet alive. It does more: it brings us acquainted with diseases long before they have reached their fatal stage. By dissection we come in with our knowledge *at last*, and gain assurance of the disease from its ultimate results. By auscultation we are often—very often—enabled to make our knowledge keep pace with the disease from its least and earliest beginnings, through all the stages of its progress to the last. By auscultation we contemplate a living action going on, and have cognizance of it while it is yet at work. By dissection we contemplate the ruin as it is left, when all action has ceased.

I am not going to give you a regular didactic discourse upon auscultation: you can only learn it for yourselves, by the use of your own ears, in the wards of the hospital. And even by your own ears it is hardly possible to learn it any where except in the wards of a hospital; for you must have many patients to practise upon at the same time; and, moreover, you must have many fellow-students engaged at the same time in making the same observations with yourselves, that you may compare notes together, and agree about what you hear. I am quite sure that no man can arrive at any useful or safe conclusions from auscultation, if he studies it alone. I speak from experience when I say this. When I first turned my attention to auscultation, I found so many sources of deception connected with it, that I determined to admit no fact which was not attested by others besides myself; and I would advise you to proceed at first with the same scrupulous care. "That every thing is easy when you know it," sounds like the simplest of truisms; but, indeed, it is a very wise apothegm. It imports that, be a thing ever so difficult, you may, by taking the necessary pains, obtain such a mastery over it, as to be surprised that you ever thought it difficult at all. Auscultation surely is not the most difficult thing in the world; neither is it the easiest. It is beset with many perplexities, and requires much time, and labour, and patience, and caution, to master it perfectly: but, being mastered, it becomes the safest, simplest guide, within its proper sphere, to a just diagnosis.

But auscultation, I have said, can only be learnt within the walls of a hospital. Yet, perhaps, I may be able to give you some general directions how to proceed, which may be of use to you; and I wish to speak of auscultation at present as it respects the lungs only.

Now before you seek to acquaint yourselves with the sounds which indicate diseases of the lungs, you must learn those which are expressive of their healthy state; for the healthy sounds must be your standard of comparison in judging of the unhealthy.

It is useless for me to attempt to describe (what is called) "the healthy respiratory murmur:" I could only tell you that this sound is like some other sound with which you might be more familiar. But in a few weeks you will know the respiratory murmur so well by experience, that it will itself become the most familiar of all sounds. The pure perceptions of sense cannot be made clearer by descriptions and similitudes.

I would recommend students to practise auscultation upon each other, for the sake of learning what the healthy respiratory murmur is; and to do it often, and upon many individuals. The respiratory murmur is, I believe, the same in kind in all men who have healthy lungs; but it has differences of degree belonging to it in different men, which are somewhat puzzling at first. The respiratory murmur does not reach the ear with the same clearness and loudness in the fat and the lean man. Fat and muscle damp the sound, where they abound above measure, as effectually as coats and waistcoats. Ausculting a man who is very fat and muscular, is like ausculting a man with his clothes on: you must make the same allowance in both cases.

But still the reason why the healthy respiration is more or less audible cannot always be found in the integuments of the chest. It often happens, that in a thin spare man, whose lungs are perfectly sound, you can scarcely hear it at all, while in a fat man you hear it most distinctly; and what is more remarkable, in a fat woman, even through the mamma.

People seem to me to differ very much in the mode and intensity of their breathing: some fill their lungs at every inspiration: the air appears to go further, and to dwell longer, within their lungs. They breathe as if they had a luxury in breathing; and your ear seems to follow the air through every cell and vesicle as it goes in and out. Some, on the contrary, let the air just enter into their lungs, and come back again. They breathe as if they were afraid of breathing; and your ear can hardly detect any respiratory murmur except when they breathe with a forced effort.

It is probable that these diverse modes of breathing, in people perfectly healthy, are required by peculiar states of the circulation; and that they are natural and necessary provisions, not only consistent with health, but essential to it.

In children the respiratory murmur is far more audible than in adults; and on this account it would be well for those to whom auscultation is new, to make their first trials upon children, that they may know what the healthy respiratory murmur is in its full and complete developement.

That the parietes of a child's chest are thinner, there can be no doubt; and this *may* be one cause why its breathing is more audible. But the mode and intensity of the breathing itself is the chief cause; and this peculiar breathing of a child is in obedience to some natural necessity, and that necessity is probably respective to its circulation.

In adults (even in fat and muscular men and women) the respiratory murmur is sometimes as loud as in a child. But then it is only in *some part* of the lungs that it is so; and when this is the case, it is the result of disease, and the disease is demonstrably of a nature to require that a larger quantity of air should be received into that portion of the lungs whence the louder respiratory murmur issues, and that there should be a more energetic act of respiration.

All this you will soon be able to verify for yourselves, by numerous cases in the wards of the hospital.

Having learnt the natural respiratory murmur, the sound which indicates that the lungs are healthy, you have got your standard of comparison, and are now prepared to judge of the sounds which denote their disease, as far as they are connected with the respiration. But you have got more than a mere standard of comparison by which to try the quality of other sounds. You are enabled to appreciate *simple* defects and failures of the respiratory murmur itself: and, indeed, it is as important a part of the business of auscultation to learn the extent to which the respiratory murmur is absent, as to discriminate the kind and character of the *new* sounds which are present and have superseded it. Besides, the diseases of the lungs are neither few nor inconsiderable, in which auscultation finds no new or unnatural sounds whatever, but only the natural respiratory murmur abated, or abolished: and these *privative* signs are as valuable helps to the diagnosis of pulmonary disease, as any that are most positive and real.

But, after all, let it be borne in mind, that the auscultatory signs of pulmonary disease are not all developed in the act of breathing: many occur in speaking or coughing; as will hereafter be shewn.

It seems to me that it would be enough for all practical purposes, if the unnatural sounds referable to the lungs, whether in breathing, speaking, or coughing, were divided *generically* into two; into *Dry* sounds and *Moist* sounds.

By dry sounds I mean those which result when bronchi, vesicles, or pulmonary cavities, present impediments, or rebounding surfaces, to the passage of air, and thus become sonorous or vocal from reverberation. By moist sounds I mean those which result, when bronchi, vesicles, or pulmonary cavities, present fluid to the passage of air, and thus yield a crackling or bubbling noise from the mingling of air and fluid together.

Of these sounds, the dry and the moist, I will point out such well-marked varieties as (I conceive) need to be understood, and will endeavour to give to each an appropriate name; taking care in the meantime to treat the subject as little artificially as possible, while I state fairly and faithfully, from my own experience, *how* I have used, and *what benefit* I have derived from using, this newly invented key to the diagnosis of thoracic diseases.

But before I employ any terms to designate particular sounds, I would remark that the language of auscultation is not yet uniform. All writers do not use the same terms to designate the same things; and, until they do, some inconvenience must continue to be felt. Under these circumstances, I shall take the liberty of using those which have become current in St. Bartholomew's Hospital, and have had here a certain meaning attached to them.

In considering the direct symptoms of diseases of the lungs derived from auscultation, I will begin with that part of their structure which is most obnoxious to disease; for there *is* a part in which disease is found most frequently to begin, and to which, wherever else it may begin, it is almost always found ultimately to reach. This is the mucous membrane of the bronchi and their ramifications.

It is essential to the healthy respiratory murmur, not that the bronchi and their ramifications be merely free and pervious in every part, but that their surface be equal and smooth, and lubricated with moisture, and that the moisture be not in excess. If the surface be unequal, rough, or unlubricated, dry sounds reach the ear in the act of respiration; if there be excess of moisture, the sounds that reach the ear are those of air mingling with fluid. The dry sounds thus proceeding from the air-passages I will call *Rhonchus* and *Sibilus*, and the moist sounds *Crepitations*.

This Rhonchus and Sibilus, and these crepitations, are always produced in breathing, not in talking or coughing. And first I wish to speak of what they are in themselves, and of how they interfere with the healthy respiratory murmur; and then I will endeavour to estimate their pathological import.

The terms Rhonchus and Sibilus are perhaps as intelligible in themselves as they can be made by further description. Rhonchus is the larger and hoarser sound; Sibilus the smaller and shriller. And, from what you must familiarly know of the sounds produced by blowing into a pipe of larger or smaller size, you will readily conceive that Rhonchus proceeds from the bronchi in their first divisions, and Sibilus from them in their minute ramifications, or the vesicular structure of the lungs.

Rhonchus often occurs alone. It is often the only unnatural sound that is heard; and then the affection is of the bronchi in their first or larger divisions exclusively. In this case, to whatever degree the rhonchus supersedes the healthy respiratory murmur, it does so not in the sense of preventing it *from taking place*, but in the sense of preventing it *from being heard*. The Rhonchus overpowers the respiratory murmur. The greater sound overpowers the less; but the less is extant notwithstanding.

The reason is, that the bronchi in their first divisions have nothing to do with producing the respiratory murmur; it does not arise in *them*, but in the lesser ramifications and vesicular structure beyond them; therefore they have no power to hinder the respiratory murmur, except when they suffer such impediments as absolutely preclude the access of air even to themselves, and consequently must prevent its further progress.

But in point of fact it seldom happens that the Rhonchus is loud enough to overcome the murmur altogether; and while they exist concurrently, the ear has often a distinct perception of both. There is a loud hoarse sound in several parts, and there is also, perhaps even in the same situations, a clear respiratory murmur. The murmur is, as it were, heard through the rhonchus. In such cases some of the *larger* bronchi contain the cause productive of the dry sound, but offer, nevertheless, hardly any impediment to the free passage of air, which, reaching the lesser bronchi and vesicles of the lungs, and finding them healthy, glides through them, and produces as it goes the murmur which is the best evidence of health.

So, too, Sibilus often occurs alone; it is often the only unnatural sound that is heard during respiration; and then the

affection is of the bronchi in their lesser ramifications, or in the vesicles of the lungs. But in this case, to whatever degree the Sibilus supersedes the healthy respiratory murmur, it does so not in the sense of preventing it from being heard, but of preventing it from taking place. And the reason is, that *the parts* which produce the Sibilus and the respiratory murmur are the same; but the *conditions* under which they produce them are different. Consequently the sounds themselves are incompatible with each other, and cannot co-exist.

But Rhonchus and Sibilus, though each often occurs alone, do just as often occur both together. And nothing more is wanted to this event than that a bronchus through its several divisions and ramifications, large and small simultaneously, should contain the cause capable of modifying the vibrations of the air in its passage.

The moist sounds occasioned by the mingling air and fluid in the bronchi and their ramifications during the act of breathing, which have been variously denominated, I call by one name, Crepitations; and of Crepitations I only make the distinction of *Large* and *Small*.

Sounds so produced are ever without variety, and can only differ in being greater or less. And according to their largeness or smallness, and the space to which they are extended or confined, they become important signs in all those diseases where a separation of fluid from the mucous membrane of the air-passages is a pathological ingredient.

The *Large* Crepitation is occasioned by the mingling of air with fluid in the bronchi, at their first divisions. It arises from the same parts as the Rhonchus, but results from a different condition.

The *Small* Crepitation is occasioned by the mingling of air with fluid in the bronchi at their lesser ramifications, or in the vesicles of the lungs. It arises from the same parts as the Sibilus, but is owing to a different state of those parts.

The Large and Small Crepitation have the same effect of suppressing or superseding the respiratory murmur that the Rhonchus and Sibilus have; and each after its own manner respectively. The Large Crepitation, proceeding from the same parts as the Rhonchus, may overpower the murmur, but cannot prevent it from taking place; whereas the Small Crepitation, proceeding from the same parts as the Sibilus, is instead of the murmur, which it abolishes altogether.

All the bronchi in their primary divisions may contain an excess of fluid, and *Large* Crepitation may be heard over every part of the chest; and yet, through that *Large* Crepitation, a practised ear will be able to detect the respiratory murmur; obscured, indeed, by the louder sound, but itself genuine and healthy.

So, too, all the bronchi in their lesser ramifications, and the whole vesicular structure of the lungs, may contain an excess of fluid, and *Small* Crepitation may be heard in every part of the chest; but, in the meantime, no ear is subtle enough to catch the natural respiratory murmur, for no such murmur exists.

The Large and Small Crepitations may co-exist together in every variety of combination. You may have both Large and Small in every part of both lungs; or Large in one lung, and Small in the other; or Large and Small in different parts of the same lung. And with Large and Small Crepitation thus differently combined, Rhonchus and Sibilus may be still intermingled, and some natural respiratory murmur be here and there distinguishable among all the rest.

I wish now to consider the pathological import of the several auscultatory signs which have been specified, before I proceed to others; for they are practically the most momentous of all. They are few, but they convey vast information, according to the manner and combinations in which they occur. Do not be surprised at this. There are but twenty-six letters in the alphabet; yet these compose all language; and language conveys all knowledge. Think of knowledge, its vastness, its variety, its multitudinous particulars! Yet language has compassed it all; language has delineated it all; and language is daily furnishing to you and to me little pictures of such portions of it as we desire to survey. Still the wonders of language are comprised in twenty-six letters.

Let it not, therefore, seem strange that many conditions of disease are signified by a few simple sounds.

ST. GEORGE'S HOSPITAL.

CASES; WITH CLINICAL REMARKS.

Acute Rheumatism, treated with large doses of Calomel and Opium—Case cut short at once by Bleeding—Severe Painter's Colic, cured in forty-eight hours — Chorea cured by Arsenic, after Iron had failed.

NUMEROUS cases of rheumatism, in its different forms, are at all seasons to be found in the wards of this hospital, and we shall from time to time illustrate the treatment adopted by the different physicians, which we observe varies a good deal, even in cases apparently analogous. We last week gave two cases of the *arti-*

cular form of the disease, in which colchicum is so efficient, at least in the early stage of the disease; this week we give two cases of the *diffuse* rheumatism, which yielded very speedily to calomel and opium; and one which appears to have been *extinguished* by a single large bleeding.

On another occasion we shall give some cases of the disease in its chronic form, whether affecting the common fibrous or bursal textures.

Henry Gibson, ætat. 21, a groom, admitted October 28th, under the care of Dr. Macleod. Has acute pain, with considerable swelling and redness, of both hands, particularly in the spaces between the wrists and knuckles, which are very tense and exquisitely tender; pulse 90, bounding; tongue white. Was exposed to cold ten days ago, soon after which he was attacked with pain and swelling of the feet, the pain extending to the knees and hips. Two days ago the lower extremities began to get better, and his hands to become painful.

Mittr. Sanguis, ad ℥xii.
℞ Calomel, gr. x.; Opii, gr. ii. M. fiant pilulæ duæ hora somni sumendæ.
Haust. Sennæ, cras primo mane. Fever diet.

October 29th.—Pain somewhat relieved; blood buffy, and much cupped; pulse 76, less active; bowels confined, the senna draught having been inadvertently omitted.

Habt. Haust. Sennæ quamprimum. Repr. Pilulæ, h. s. et Haust. cras mane.

30th.—Bowels were freely purged yesterday afternoon, and have also acted several times this morning. Pains very much relieved; swelling and redness nearly gone; pulse 70, softer.

Repr. Medicamenta.

November 1st.—Gums becoming affected; bowels freely purged; pains gone.

Pulv. Ipecac. Comp. gr. x. om. nocte. Omit. alia.

10th.—No recurrence of rheumatism. Discharged cured.

Thomas Jacobs, ætat. 57, labourer, admitted October 28, under the care of Dr. Macleod. Was attacked, five weeks ago, with pain in the limbs, after exposure to cold. About three days ago the pain in the right hand underwent a severe exacerbation, the part becoming red and swollen all over. Pulse frequent, and bounding; skin hot; tongue white.

Mittr. Sanguis. ad ℥xiv.; Calomel, gr. x. Opii, gr. ii. hora somni; Haust. Sennæ, cras mane. Fever diet.

October 29th.—Pain mitigated, but still severe; blood very much cupped, with a thick and a cloudy mass like coagulated albumen floating in the serum; bowels have acted only once.

Habt. Haust Sennæ quamprimum. Repr. alia ut antea.

31st.—Gums becoming tender; bowels freely purged; pains much better; redness and swelling disappearing.

Pulv. Ipecac. Comp. gr. x. om. nocte. Haust. Sennæ, alterno quoque mane. Fish diet.

November 10th.—Nothing worthy of remark has occurred since last report, the convalescence having gone on progressively. Discharged cured.

Dr. Macleod remarked, in reference to rheumatism, that there was perhaps no disease with respect to which opinions and statements appeared to be more contradictory; but that much of this discrepancy depended on the want of a due attention to the form and stage of the disease. According to his experience in the acute *diffuse* form of rheumatism,—that in which the swelling is not most developed at the small joints, but involves all the contiguous fibrous parts,—no treatment is so effectual at the commencement as the abstraction of a moderate quantity of blood, with full doses of calomel and opium at night, followed up by free purging next day. In a first attack of very recent date, a full bleeding sometimes at once arrests the disease; this, indeed, is not generally the case, but a striking example of it occurred at an early period of the present year, in a patient under his care. He had also seen acute rheumatism at its onset arrested by calomel and opium without bleeding or purging; but for this purpose it must be very freely administered, and is apt to produce troublesome salivation. He had not tried the effect of purging without the calomel and opium; but he was satisfied that the relief was much greater when the bowels were freely acted upon, than when this was not attended to. The general effects of the plan were well illustrated by the cases given above, both of which were admitted, and both discharged on the same day.

We subjoin from the case-book, the example in which an attack of acute rheumatism was *extinguished* by one large bleeding. It is right to remark, however, that another case, in a less favourable subject, admitted the same day, and similarly treated, did not recover till the calomel and opium were administered.

John Hudson, æt. 35, a hair-dresser, of

robust habit, admitted January 21, 1835, under the care of Dr. Macleod. The left foot and ankle, as well as the hand and wrist of the same side, are much swollen, tense, and red. The parts are exquisitely painful, especially on the slightest movement. Pulse 105, full and hard; skin hot; tongue loaded; bowels open, from medicine taken this morning; urine scanty, and high-coloured.

Eight weeks ago had erysipelas of the face, followed by some pain in the chest, and cough, the latter of which still continues. Has had rheumatic pains flying about him for a week, which, the day before yesterday, became fixed in the parts above mentioned, and which soon began to swell, the pain becoming progressively more and more acute. Has not had rheumatism before.

Mit. Sanguis, ad ℥xxx. App. Lotio spirituosa partibus inflammatis. Fever diet.

22d.—Experienced great relief within an hour after he was bled, since which time the swelling of the hand and foot has progressively, but rapidly, diminished, and is now entirely gone; the skin being shrivelled, from the rapidity with which the tumefaction has disappeared. Has no pain whatever except when he moves. Blood buffy, and very much cupped; pulse 80, soft; bowels not open. The spirit lotion has been applied only to the hand, and its use was not commenced till the evening, when the pain and swelling were already rapidly diminishing.

Haust. Sennæ, quamprimum et repetatur cras mane si opus sit.

From this time he suffered no inconvenience except from his cough, which was relieved by oxymel of squills; and he was discharged cured on the 3d of February.

Another disease of a different kind, in which calomel and opium proved very efficient, was illustrated by a case which happened to be in the hospital at the same time as the two cases of rheumatism first given — namely, painter's colic. As the case was a good one, we subjoin it.

Thomas Bramage, a painter, ætat. 25, admitted October 28, under the care of Dr. Macleod. Complains of violent pain in the belly, extending from the umbilicus to the loins, and shooting down to the testicles. The pain, though never entirely absent, undergoes violent exacerbations, during which the patient rolls in agony, and presses his fists upon the abdomen with all his strength. Occasional fits of vomiting. Bowels habitually costive, and have not acted at all for two days. Tongue moist, with a thin slimy coating; pulse 64, soft; skin cool.

℞ Calomel, gr. x., Opii, gr. ij., fiant pilulæ duæ quamprimum sumendæ, et hora somni repetendæ.

Hab. Ol. Ricini, ℥j. cras mane, et sumat ℥ss. tertiâ quâque horâ donec responderit alvus.

Descendat quamprimum in Balneum calidum.

27th.—Still complains of pain in the belly, though not nearly to the same extent as yesterday. Bowels have acted twice freely; gums swollen; strong mercurial fœtor in the breath.

Omit. Calomel. Rep. Opium et Ol. Ricini ut antea.

Rep. Balneum vespere.

29th.—Free from all pain since yesterday. Bowels open; pulse 80; tongue cleaning.

Sumat Opii, gr. j. om. nocte. Ol. Ricini, ℥ss. om. mane.

Nov. 2d.—Discharged cured.

Another case which excited some interest from its severity and obstinacy, was one of chorea, which strikingly illustrated the fact, that arsenic will sometimes succeed in curing that disease when the carbonate of iron, in pretty full doses, has failed.

Mary Anne Gilling, ætat. 9, admitted September 2, labouring under chorea in a severe form. The convulsive movements nearly constant, affecting both the upper and lower extremities, and preventing her from being able to walk. She is pale, and looks out of breath; appetite bad; bowels torpid. Nearly a fortnight was spent in regulating the bowels, by means of calomel, castor oil, and aloes, variously administered. At the end of this time the action of the bowels was re-established, and her health was better, but the convulsions were in no degree mitigated. She had also—

Tinct. Ferri Ammoniat. ℨj. ter die, ex Aquæ pauxillo.

and the shower-bath was used every morning.

16th.—The symptoms of chorea remaining as above, she was ordered—

Ferri Carbon. gr. x. ter die. Haust. Sennæ al. quoque mane.

18th.—No change.

Ferri Carb. gr. xx. ter die. Rep. Haust. et Balneum.

22d.—No change.

Ferri Carb. ℨj. ter die.

28th.— Some temporary improvement appeared to have taken place for a day or two, but the chorea is now as bad as ever.

Ferri Carbon. ℨiss. ter die. Rep. alia.

Oct. 1st.—No change.

Ferri Carb. ℥ij. ter die. Rep. alia.

5th.—No change. The iron was now discontinued, and she was ordered—

Liq. Arsenicalis, ♏iij.; Spirit. Lavendulæ, ℥j. Aquæ, ℥vij. M. fiat Haustus ter die sumendus.

By the 9th the dose had been increased to ♏vj. ter die, when she was observed to be decidedly steadier. By the

14th the dose amounted to ♏viij., when her improvement became so marked, that it was not deemed necessary to increase the dose any farther. By the

19th she could sit up, having previously remained constantly in bed, from being unable to prevent herself from falling. By the

26th she was perfectly steady, and could walk about without any difficulty. Her general appearance had also improved prodigiously. In a few days afterwards she was discharged cured.

Dr. Macleod remarked on the 5th, when the iron was discontinued, that he had seen several instances in which chorea, which had resisted the use of iron, speedily yielded to arsenic. He did not mean to say that iron, particularly the carbonate, might not possibly succeed if persevered in for a certain length of time; but as he had never seen any ill consequence result from the liq. arsenicalis properly administered, and as it cut short a troublesome disease, which lingered for an indefinite period under other remedies, he did not think we were warranted in neglecting to administer it. Purging he only regarded as an auxiliary—one which was very seldom sufficient to cure the disease by itself. The shower bath ought always to be used, unless there be some strong reason to the contrary.

SUGGESTIONS TOWARDS A BETTER SYSTEM OF

PAROCHIAL MEDICAL ATTENDANCE.

By Ruricola.

To the Editor of the Medical Gazette.

Sir,

It has struck me that some misapprehension might arise from an apparent discrepancy in my last communication. The remuneration for parish surgeons is there proposed, in one place, to be *fixed*; in another, to *vary*. I would explain it thus:—Let there be a *fixed scale* of remuneration, adapted to the following *varying* circumstances—viz. to *the number of sick paupers*, which will, of course, depend partly on the "population of the parish," and partly on its proportion of pauper inhabitants; and also to the *labour of the medical attendant*, which will depend not only on the above circumstances, but on the "*density* of the population," and the "*distance* of the parish from his residence."

The next proposition I have to suggest with regard to pauper medical relief, is the adoption of a different method of giving *orders*. It is evident that so long as this depends on the caprice or convenience of a relieving officer, the poor must suffer a needless hardship.

Accident and sudden illness require, of course, much more immediate attention than applications for weekly pay or provisions: the circumstances are totally dissimilar. The officer who may be qualified and prepared to bestow sufficiently prompt attention on *one* set of applications, may be, on various accounts, quite unable, as well as unsuitable, to act as the necessities of the case may require in the *other*.

The person, therefore, to whom all applications for medical relief should, in the first place, be made, is the *medical officer himself*; and he should be authorized, in emergency, to give immediate assistance; but as he cannot be expected to judge of the pecuniary circumstances of the patients, they should be required within a specified time (say one or two days) after their application to the surgeon, to send to the relieving officer for an "order;" nor should this functionary be required to give it, unless the circumstances of the applicants entitled them to parochial relief, and unless they produced a certificate from the medical officer, stating that their cases needed attendance.

This arrangement will place the medical relief afforded to paupers nearly on the same footing as that supplied by hospitals and dispensaries; for in these institutions, also, the medical officers are the *first* judges, the boards of governors, or committees, the *second*, of the fitness of the applicants for assistance.

Of course, if the relieving officer refused an "order," the Board of Guardians would be the court of appeal; and if *here* the refusal was confirmed, the medical officer must consider the case as treated on his own account; and though he might occasionally *lose* in this way, it would doubtless be far better for him than the injury he now sustains, by the frequent withholding *slight cases* and *first stages* of disease, and so entailing on him subsequent long and expensive attendances; while for the

poor this plan would be incalculably more humane, as they would in all cases insure an early attention to their wants, by having to apply *at once*, and without any intermediate step, to the only person from whom, sooner or later, relief can be obtained.

Thirdly, The evils that have arisen from the present mode of appointing medical officers, naturally lead to the conclusion that the parties with whom the right of appointment at present resides are not the most fit to exercise it; and granting that, by the adoption of a fixed scale of remuneration, many of the inducements to appoint *too few*, or *unsuitable* medical men, would be removed, still there appears to be no good reason why the Boards of Guardians should continue to supersede the whole body of rate payers, in a matter where the latter must be allowed to have the best opportunity of judging rightly.

Indeed, if the power of reducing the salaries of the medical officers (by the tender system or otherwise) were taken away from the Boards of Guardians, they would probably be quite indifferent about retaining *the right of appointment*, which would alone remain.

The rate-payers of each parish, on the other hand, have the strongest inducements to select that surgeon who, from the confidence bestowed on him by the inhabitants, and from his previous long residence in or near their parish, might justly be esteemed the fittest person to take charge of their sick poor. The lower order of rate-payers would be particularly tenacious of exercising the right of election, feeling an additional interest in the proper treatment of those, among whom might be found their own relatives or connexions.

Tried professional character would then have its due weight; previous neglect of duties would meet with its sure punishment; and *fitness* for the office would be the sole consideration of the electors. The mode of conducting this election might be the same as that employed for electing the Guardians of the Parishes, which is the simplest and best that could be devised.

This desirable restoration of the privileges of the rate-payers, in connexion with the separate calculation of the medical salaries for each parish, would effectually destroy the present odious system of *medical districts*, so irrational in itself, so offensive to the profession, and so injurious to the poor.

As, however, the executive administration of parish affairs rests with the Boards of Guardians, it would be right that they should possess the power of suspending the medical officers, or of withholding their pay, for well-ascertained omission of duty.

Fourthly, There is still a question as to the proper manner of obtaining *medical opinions* at the Boards of Guardians. At present this is accomplished by *summoning* the medical officers. It must be confessed that this is rather an unceremonious proceeding towards members of a learned profession; and it may be fairly asked, what evil could result from making the medical officers members of the Boards of Guardians, *ex officio?* Since the remuneration given must, in the present state of the country, be far below the services rendered, there is no reason why the medical men should not be treated as benefactors to the poor, and, *in some measure,* as the honorary officers of parishes; and it would be but decorous to give them the opportunity of stating their opinions as members of the Boards, instead of sending a summons for them, as for any inferior functionary.

The idea of medical men being "servants" of the Boards of Guardians, must be relinquished, before the *respectable part* of the profession will have any thing to do with parish appointments.

It will not be denied that medical opinions are often of great consequence in the deliberations of the Boards. The state of the health of the paupers generally,—the approach or prevalence of epidemics,—the quality of nutriment or provisions,—the purity of water-springs,—the salubrity of the air in different parts of the union,—the supply of additional support to sick paupers,—the detection of imposture,—are all questions upon which the medical officer would be the best qualified to offer an opinion; and as to general information and intimate knowledge of the habits of the poor, these gentlemen may be presumed to be superior to most of the individuals at present composing the Boards.

There can only be one objection to the medical officers taking their seats at the Boards, and that is, the occasional necessity which might happen for investigating their official conduct. At these times, if common delicacy did not prompt them voluntarily to withdraw, a provision might easily be made for their temporary exclusion.

Besides, it is highly improbable that a medical man in full practice would have time and opportunity to attend oftener at the Board than when points were to be mooted which required his consideration.

These, sir, are the main alterations in the present system which I think it right to suggest, and shall be highly gratified if my feeble attempts in the cause elicit the opinions of any of your readers on the subject.

If a determined effort be not made to procure some amendment, the public

health and the interests of science will alike suffer by the introduction of an inferior race of practitioners throughout the country, who, if successful in maintaining their posts, will uproot the existing medical men; and, if failing in their projects, will leave the Poor Law administrators at the mercy of the established practitioners, and so possibly be the means of increasing the rate of remuneration to an amount which at present no moderate man would think of proposing.

But however the matter may be settled, there are resources for the poor, and for the established practitioners, to which one turns with pleasure, after considering the previous unwelcome subject; — these are Self-supporting Dispensaries, and other associations for mutual assurance, against the expenses of sickness among the poor.

To the benevolent individual who originated these institutions, and to those who, following his example, have in other parts of the country improved upon and carried out his principles, the community, the poor, and the profession, owe much gratitude.—I remain, sir,

Your obedient servant,

RURICOLA.

November 16, 1835.

LIGATURE OF THE INTERNAL ILIAC ARTERY.

M. BARONI, one of the Professors at Bologna, has just published the particulars of a case in which he tied the internal iliac artery. The following is a brief summary of the facts, taken from the *Gazette des Hôpitaux*:—

A peasant, 22 years of age, fell from a tree, and received a deep wound in the thigh from a sickle which he carried. The wound laid bare a portion of the os innominatum and sacro-sciatic ligaments. The bleeding, however, was speedily arrested, and the wound healed by the first intention; but the patient soon after became irregular in his diet, when fever came on, followed by a collection of pus in the site of the wound; this was evacuated by a surgeon. On the 14th day a severe hæmorrhage came on. After removing the clots a jet of blood issued from the bottom of the wound. The finger was applied so as to arrest it, and the parts laid open with the knife; after which, by means of a needle curved at the extremity, a ligature was applied to the internal iliac, above the wounded point, but the hæmorrhage very soon returned. A second ligature was now placed below the bleeding orifice. Eight days afterwards some bleeding took place from another vessel at one side of the wound, which required to be tied. The recovery went on favourably, and at the end of a month the patient was well.

NEW MEDICAL WORKS.

Manual of Practical Midwifery. By James Reid, M.D. 24mo. 5s. 6d. bds.

Outlines of Mineralogy, Geology, and Mineral Analysis. By T. Thomson, M.D. (being the third and concluding portion of his Chemistry.) 2 vols. 8vo. £1. 12s. bds.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

November 19, 1835.

Henry Coward, North Shields.
Thomas Laycock, Doncaster.
Joseph Goldstone, Bath.
Henry Isaac Lomax. London.
Gerrard Potter, Adlington, Lancashire.
William Foxton, Hayley.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Nov.* 17, 1835.

Abscess	3	Inflammation	27
Age and Debility	42	Bowels & Stomach	1
Apoplexy	5	Brain	2
Asthma	14	Lungs and Pleura	7
Cancer	3	Insanity	3
Childbirth	4	Liver, diseased	4
Consumption	63	Measles	6
Convulsions	30	Mortification	1
Croup	4	Paralysis	3
Dentition or Teething	4	Small-Pox	24
Dropsy	21	Sore Throat and Quinsey	1
Dropsy on the Brain	9	Spasms	1
Erysipelas	3	Thrush	1
Fever	5	Unknown Causes	6
Fever, Scarlet	10		
Heart, diseased	4		
Hooping Cough	4	Stillborn	17

Decrease of Burials, as compared with the preceding week . . . 57

METEOROLOGICAL JOURNAL.

Nov. 1835.	THERMOMETER.		BAROMETER.	
Thursday . 12	from 33	to 45	30·14	to 30·18
Friday . . 13	33	43	30·27	30·25
Saturday . 14	33	44	30·13	30 03
Sunday . . 15	37	46	30·02	30·03
Monday . . 16	33	45	29·97	29·91
Tuesday . . 17	33	48	29 87	29·83
Wednesday 18	40	54	29 78	29·68

Prevailing winds, N.E. and N.W.

Except the afternoon of the 12th, generally cloudy; with frequent showers of rain.

Rain fallen, ·1 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

"MEDICUS."—The Act is by no means retrospective; unless its respecting the rights of those in practice when it passed can be taken in that sense. From our correspondent's showing, its construction is entirely in his favour.

Notwithstanding our additional sheet, we have been obliged to postpone several papers for which we could not find space: the writers will be good enough to accept this as our apology; we hope to make them speedy amends.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, NOVEMBER 28, 1835.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE IX.

ON CASTOREUM.

GENTLEMEN,—As the second, third, and fourth orders of mammalia yield us no pharmacological agent, I proceed in this lecture to examine the fifth, which has been called *Rodentia* (from *rodo*, to gnaw), in consequence of the manner in which the animals of this order take their food. They have been also termed *Glires*, or *Hybernantia*.

You will observe that the *Rodentia* have no canine teeth (fig. 31); and hence

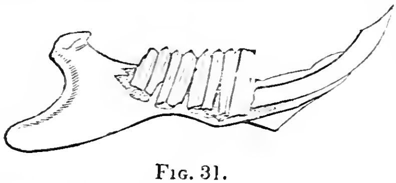

FIG. 31.

they are not adapted for seizing living prey. You see, however, that they have two long incisor teeth, which are remarkable on two accounts. In the first place they have a constant tendency to elongate, and only keep of the proper length by the friction and pressure of the antagonizing teeth of the upper jaw. In the Hunterian Museum of the Royal College of Surgeons is a beaver's skull, showing that, when the upper incisor teeth are removed, the lower ones continue to elongate. The second peculiarity is their chisel edge, which is produced by the unequal hardness of the anterior and posterior edges, in consequence of the enamel being deposited on the anterior surface only; so that the posterior edge wears down much faster than the anterior, which is thus left sharp and cutting.

Castor Fiber, or Beaver.

This animal yields a remarkable secretion, called *castoreum*, which was used in medicine by Hippocrates more than 2250 years ago. Its generic name κασтωρ. is probably derived from γαστηρ, the stomach or belly, on account of the swaggy or prominent appearance of this part. The Latins called the animal *Fiber*, because it resides at the extremities of rivers. It was also denominated the *Canis Ponticus*, or *Pontic dog* (fig. 32).

In examining the most interesting of its external appearances, I shall first direct your attention to the *fur* of the animal, which varies in its colour, so that we have brown, yellow, black, spotted, and even white beavers; but the two latter varieties are very rare. Richardson, in his *Fauna Boreali-Americana*, has never seen either of them, though he has met with black beavers, which were kept as curiosities.

The *tail* is a part peculiar to this animal, and by which it may be distinguished from the other Rodentia. It has so much of the fishy character, in consequence of being covered with greyish oval scales (fig. 32, *b*), that it is said to be allowed in some countries on fast days, when the body of the animal is prohibited. The great breadth (oftentimes five inches) does not depend on the width of the caudal vertebræ; for you observe in this skeleton (fig. 33) these bones are not more than two inches wide. The numerous strong tendons inserted la-

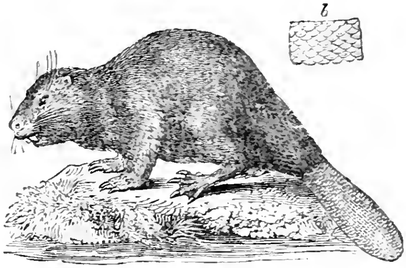

Fig. 32.

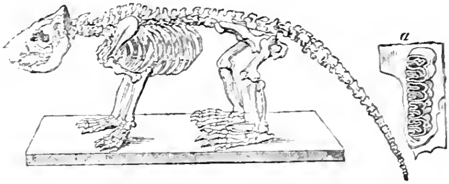

Fig. 33.

terally into them contribute to its breadth. It contains a quantity of gristly kind of fat, and is considered a great luxury by the native tribes in America, where the animal is caught.

You will observe the animal has five toes on each foot, those of the posterior extremities being webbed.

The incisor teeth of this animal are two in number; they are smooth and orange-coloured anteriorly, while posteriorly they are white. There are sixteen molar teeth, the crowns of which are flat, and the enamel being in the form of sinous ridges (fig. 33 *a.*)

I now proceed to examine the *position*, *structure*, and *functions*, of the *castor sacs*. The consideration of these subjects, however, involves a knowledge of the parts connected with them — namely, the sexual organs and the oil sacs. I must, therefore, briefly notice these.

Many persons have thought that the beaver was a *monotrematous* animal (so called from *μονος*, *one*, and *τρημα*, *opening*); that is, had a common opening for the evacuation of the urine and of the fæces, termed a *cloaca*.

But according to Brandt and Ratzeburg (and the preparations on the table bear out their statement), the beaver has no part which properly deserves to be called a cloaca. The error has arisen from the following circumstance. In the male animal the openings of the rectum, of the oil sacs, and of the prepuce, lie in a common hollow, inclosed by a large, wrinkled, somewhat hairy, cutaneous protuberance, which, according to Perrault, is easily contracted and dilated, not by a sphincter, as the anus, but simply like a slit. Carus, however, still continues to term the part a cloaca.

If you fancy the animal laid on his back, and the skin of the abdomen removed, four eminences will then be seen, placed between the pubic arch and this false cloaca. Here is a drawing from Perrault illustrating this (fig. 34.) The

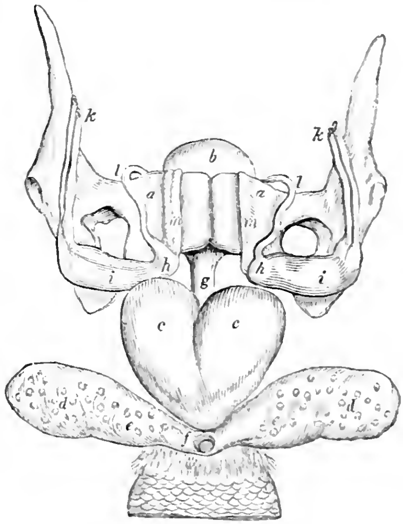

Fig. 34.

a a, Os pubis.
b, Bladder.
c c, Castor sacs.
d d, Oil sacs.

f, The false cloaca.
g, The commencement of the penis.
h h, The epididymides.
i i, The testicles.
k k, The spermatic cord.
l l, The vasa deferentia.
m m, The cremaster muscles.

two nearest the pubes are the *castor sacs*, while those next the false cloaca are the *oil sacs.* Between the two castor sacs in the male lies the *penis;* it is lodged in a long *preputial canal*, which terminates in the false cloaca, and has some analogy to a vagina; so that it is sometimes difficult to determine, until the skin be removed, whether the individual be male or female. The apex of the penis does not point towards the navel, as in the dog, but towards the tail. Here are several specimens of the bone found in the penis (fig. 35.) The surface of the penis is co-

Fig. 35.

vered with longitudinal wrinkles and pits; in each of the latter is found a dark-coloured warty-like body (fig. 36 *j*.)

The testicles, vasa deferentia, and vesiculæ seminales, present nothing remarkable. There is no scrotum. Like most other Rodentia, the beaver has *vesiculæ accessoriæ*, or blind ducts, which open into the urethra near its commencement. Just at the point where the urethra joins the penis are observed Cowper's glands.

The *castor sacs* open by a common aperture into the preputial canal. This aperture is about one inch in width (fig. 36, *f*), and is placed opposite the extremity of the *glans penis* in the relaxed condition of the organ, and about one inch from the orifice of the prepuce. Between this common orifice of the castor sacs and the glans penis is a semilunar fold (a probe, *v v*, is passed under it in fig. 37.) There is also a second, similar, but thicker, fold covering the rectum (*u u*, fig. 37.)

The *castor sacs* are pyriform and compressed. They communicate with each other at their cervical portion; but their fundi diverge outwards and towards the pubes. It was an ancient opinion that these sacs were testicles, and that when closely pursued by the hunter, the animal tore them off, leaving them behind him

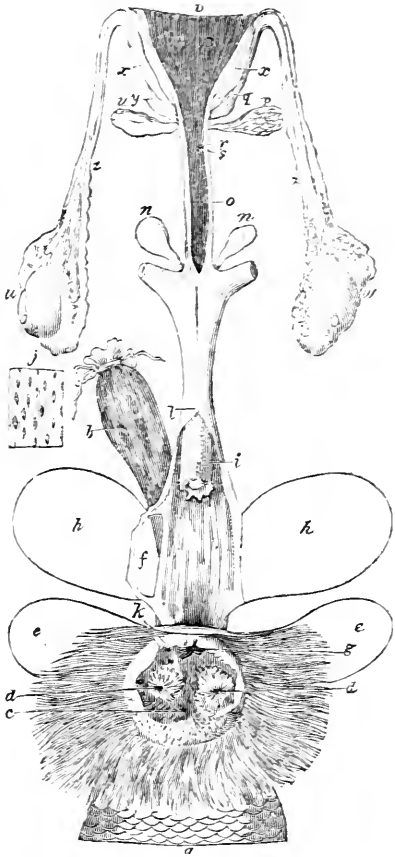

Fig. 36.

a, Under portion of the tail.
b, Rectum.
c, Anus.
d d and *k*, Openings of the anal glands.
e e, Anal glands.
f, Common opening of the two castor sacs.
h h, The castor sacs.
i, Glans penis.
j, Magnified view of a portion of the epidermis of the glans.
l, Penis.
n n, Cowper's glands.
o, Urethra laid open.
p, Left vesicula seminalis.
q, Left vas deferens.
r, Opening of the left vesicula seminalis.
s, Opening of the left vas deferens.
u, Right vesicula seminalis.
v, Portion of the bladder, showing the openings of the ureters.
w, w, Testicles.
x x, Vesiculæ accessoriæ.
y, Right vas deferens.
z, Spermatic cord.

as a ransom. Hence, it was said, arose the name of the animal, *à castrando*. Al-

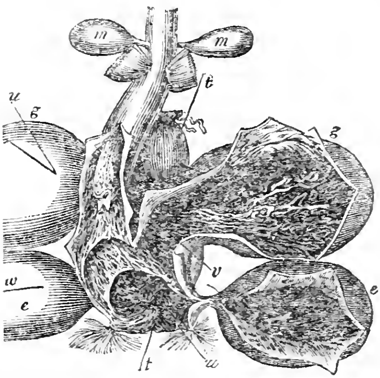

FIG. 37.

e, e, Anal glands.
g g, Castor sacs.
m m, Cowper's glands.
t t, Probe passing into the rectum beneath a semilunar fold which separates the common aperture of the castor sacs from the penis.
u u and *v v*, Two probes passing into the right castor sac, behind a second semilunar fold.

luding to the conduct of Catullus in a storm, Juvenal says—

——— "imitatus Castora, qui se
Eunuchum ipse facit, cupiens evadere damno
Testiculorum: adeo medicatum intelligit
inguen."

This absurd notion seems to have been long ago disbelieved; for Pliny tells us that Sextius derided it, and said that it was impossible the animal could bite them off, since they were fastened to the spine. Thus is one error confuted by another; the truth being, the testicles are so placed in the inguinal region, on the external and lateral part of the *os pubis*, that they are not discernible until the skin be removed: besides, female beavers also have castor sacs. We are, however, less perfectly acquainted with the anatomy of the female than the male animals; indeed, I am acquainted with three dissections only which have been published—namely, one by Gottwaldt in 1680, one by Hegse in 1684, and one by Mortimer in 1735. Here is a drawing from the latter (fig. 38.)

Mortimer says the animal had two ovaria, and an uterus dividing into two horns (uterus bicornis), as in the bitch. The bladder lay exactly over the body of the uterus. The meatus urinarius ran upon the vagina above two inches in length. Just below the *os pubis*, on each side of the vagina, above the meatus urinarius (supposing the animal laid on her back), a pair of pyriform bags were found, about $1\frac{3}{4}$ inches long, and 1 inch broad, diverging at their fundi, or broad ends, but approximating almost closely at their necks, or narrow

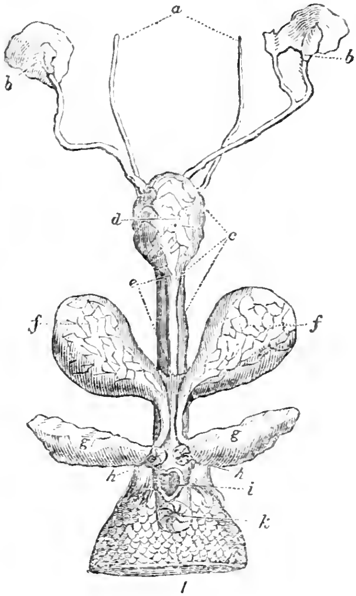

FIG. 38.

a, The two ureters.
b b, The ovaria.
c, The uterus lying under the bladder.
d, The bladder contracted and empty of urine.
e, The meatus urinarius, above two inches long.
f f, The castor sacs.
g g, The oil sacs.
h h, Common orifices of the castor ducts and oil sacs.
i, The vagina cut off.
k, The anus.
l, Part of the tail.

extremities, which were canals communicating with the adjoining glands. The membranes which formed these bags were tough, wrinkled, and furrowed, of a livid dirty colour. They were hollow, and capable of containing about an ounce of water. Upon opening them a small quantity of a dark-brown liquor, like tar, was found, having an odour like castoreum, and in addition a smell of ammonia. It is probable the emptiness of the sacs, and the unusual quality of their contents, arose from the youth of the animal. About an inch lower, on each side of the vagina, were a pair of glands, each about $1\frac{1}{2}$ inches long, and $\frac{1}{2}$ inch broad. Their form was oblong, but irregular, and having several protuberances externally; their colour pale flesh, like the pancreas. They seemed to communicate with the castor sacs, the sac and gland on each side opening externally by one common orifice, around which were long black hairs.

The structure of these castor sacs next deserves our notice. This can in part be made out by dissection of the most recent

sacs of commerce, macerating them first in water, and then in spirit. Each sac has five coats.

1. *An external* or *cellular coat*.—Its colour is greyish. Perrault says that it is clear and transparent, and that its colour is derived from the internal coat.

2. *Muscular fibres* are inclosed in the cellular coat. They are regarded as a continuation of the panniculus carnosus, and their use seems to be the compression of the sac.

3. *Vascular coat*.—This lies within the muscular coat. It not only covers the convolutions of the fourth coat, but also sends processes in between them in a similar manner to the pia mater, which dips down between the convolutions of the brain. To this cause must be referred the *ruminated* or *nutmeg* appearance when we make a section of some of the dried sacs. This membrane is very vascular; hence the name by which I have designated it. It is said to be very thick in the Russian sacs, but I have had no opportunity of verifying this statement.

4. *Scaly* or *glandular coat*.—It has been compared by some to a mucous membrane, but its structure is in several respects different. Externally it has a shining, silvery, and iridescent appearance; internally we observe a number of small scales, which overlap each other like tiles. Their shapes are lanceolate, oblong, or semilunar, and they are smaller towards the mouth of the sac. The margins of the scales are mostly toothed, but are sometimes entire. Beneath each, and lodged in a small cavity, is a *small brown body*, varying in shape, and supposed by some to be a gland; though this opinion is doubtful. This fourth coat is thrown into numerous folds or convolutions, which are largest and most numerous in the fundus of the sac.

5. *Epithelium*.—This coat is a continuation of the epithelium of the prepuce, and lines the inside of the sac, inverting the glands and scales of the fourth coat, and therefore the castoreum must pass through it to get into the cavity of the sac.

Having now finished the structure of the castor sac, let us proceed to the examination of its contents.

The secreted substance found within these sacs is denominated *castoreum*, though this term is commonly applied to the secretion and sacs themselves, as found in commerce. The quantity of this secretion varies considerably. I suspect that sex, season, and age, are among the circumstances which affect the quantity. In its recent state this substance is thin, fluid, and of a yellow or orange colour, but becomes deeper coloured by exposure to the air. By soaking one of these sacs in hot water, you observe the secretion becomes quite soft, and similar in appearance to that in the recent state.

The *oil sacs*, which I have already noticed, are two in number—one on each side between the castor sac and anus. They appear to be of the nature of conglomerate glands, each being made up of one or of several lobes. They occur both in male and female beavers. In this preparation each sac is about two inches long, and one and a half broad. Their shape varies, but for the most part is ovate. Each sac terminates in one or more ducts which open into the false cloaca on either side of the anus. The ducts are usually so small as to allow the passage of a fine bristle only; their length is about seven lines. The external aperture of the ducts is surrounded by a small swelling. Each sac consists of—1st, an *external* or *cellular* coat; 2dly, of a layer of interlacing *muscular fibres*; 3dly, of a *firm cellular coat*, separable into two layers, between which are found little *glandular* masses; and 4thly, an *internal*, very delicate milk-white coat, easily separable, and lacerable. The glandular masses just alluded to consist of heaps (each about one line thick, and from two to eight long) of conglomerate, yellowish, flesh-red glands, from which small *ducts* proceed: these unite to form one or more larger ducts, which open into the cavity of the oil sac beneath a small fold.

The contents of the oil sacs are a fatty matter, usually of the consistence of syrup or honey, though I have sometimes found it much harder, having a peculiar odour, and a yellowish colour. It is not an article of trade, but the Indians occasionally eat it, and also mingle a little with their tobacco when they smoke. It was formerly employed in medicine under the name of *Pinguedo seu Axungia Castoris*.

There are certain muscles which compress both the castor and oil sacs. In *Brandt* and *Ratzeburg's Medizinische Zoologie* is an account of the dissection of a beaver, at Wittenburg, in 1827; and the following muscles are described as common to both castor and oil sacs (fig. 39):—

1. A thin muscle, having an arched form, passing from the penis to the posterior side of the castor muscle. It is more superficial than the erector penis (fig. 39, *h*.)

2. A muscle, arising close to the former, and expanding itself over the under surface of the oil sac (fig. 39, *i*.)

3. Another muscle, lying beneath No. 2 (supposing the animal placed on his back). It expands over the upper surface of the castor sacs, while part of the muscle crossing its fellow is distributed to the castor and oil sacs of the opposite side (fig. 39, *l l*.)

4. A small bundle arises posterior to the above, and is distributed to the castor and oil sacs of the same side (fig. 39, *n*.)

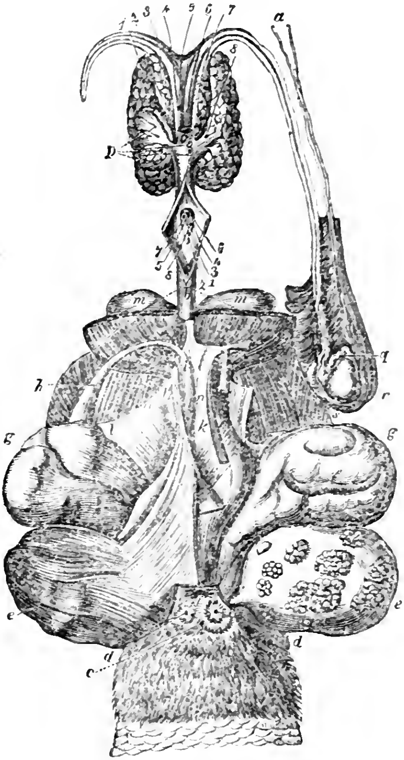

FIG. 39.

a, Spermatic vessels.
c, Anus.
d d, Openings of the anal glands.
e e, Anal glands.
g g, Castor sacs.
h, i, l l, n, Compressor muscles of the castor sacs and anal glands.
k, Penis.
m m, Cowper's glands.
o, Urethra cut off.
p, Lobes of the prostate gland.
r, Testicle.
The figures refer to the probes passing from the caput gallinaginis to the vesiculæ seminales and vasa deferentia.

5. Fibres of the cutaneous muscle interlace with the muscles already described.

A great deal of nonsense has been written respecting the *functions of the castor and oil sacs*. One writer tells us that castoreum promotes the appetite of the animal,—another, that it improves his digestion,—a third, that it serves him for food during the winter season,—and lastly, a fourth tells us the animal besmears himself with this secretion as a protection against the action of the water. All these are gratuitous hypotheses. The most probable view of the nature of these bodies is, that the glands of the castor sacs are analogous to the odoriferous glands of man and many other animals, while the oil sacs are anal glands: so that there is nothing mysterious about them; they are parts analogous to those found in many other animals, though here in a high state of development.

But it may be asked, of what use is the secretion of the odoriferous glands? In the human subject, the *glandulæ odoriferæ*, or, as they are sometimes denominated, the *glandulæ tysonianæ*, are placed around the neck and corona of the glans penis in the male, and analogous organs are found around the glans clitoridis in the female. They secrete a strongly odorous humor, having a tendency to solidify, and which has been denominated *smegma præputii* (from σμῆγμα, *soap*). They are usually described as having for their object the preservation of the sensibility of the glans, and to allow the prepuce to move backwards and forwards with facility. I confess I am not satisfied with this kind of physiology. As this secretion is peculiar to the sexual organs, we are naturally led to suspect that it serves some sexual purposes. When we also see these secreting glands more largely developed in the lower animals, and bear in mind the powerful influence which the odour has on the sexual feelings, we can hardly refuse to admit that the so-called *smegma præputii* is a venereal excitant. Dr. James Blundell seems disposed to carry this notion to a great extent; he refers the fondness of rats for oil of rhodium, and of cats for valerian, to the resemblance which these substances have to the sexual odours. Now doubtless castoreum emits one of these odours; for, in addition to the argument drawn from its strong smell, must be mentioned the position of the glands which secrete it, which is the same as the odoriferous glands in the human subject—that is, around the glans penis in the male, and at the commencement of the vagina in the female.

The functions of the oil sacs are very little understood. I am surprised that so late and intelligent a writer as Richardson, in the North American Fauna, should have adopted the notion that the secretion of these sacs affords a dressing to the fur, because small ponds which are inhabited by beavers are tainted with its peculiar odour. Nature has certainly placed the organs secreting it in a very awkard situation, if such be its use. An argument against this opinion is, that many animals—as the hyena, the dog, and the fox—which cannot be called aquatic, have analogous glands (fig. 40).

The secretion of the oil sacs of the beaver is so remarkable both from its position and qualities, that I can hardly regard it as being merely excrementitious. By its lubricating property, it may act as a defence to the skin from the fæces; but this cannot, I think, be its sole use.

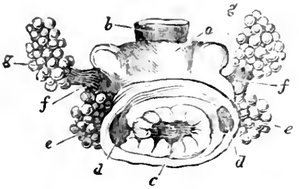

FIG. 40.—*Anal Glands of the Hyena.*

a, Glandular sac.
b, Rectum.
c, Its opening.
d, Glandular openings.
e, Glands.
g, Idem.
f, Excretory ducts of the glands.

The *geographical* distribution of the beaver was formerly more southern than now. It appears that they were formerly indigenous to this country, since it is so stated by some old writers; and the discovery of fossil remains of the animal has confirmed the assertion. It seems they were principally found in Wales and Scotland. The latest period at which they are known to have existed here is 1188, A.D. In the western hemisphere they are found from 67° or 68° north latitude (as the banks of the Mackenzie River, which discharges itself into the Arctic Ocean), to about 33° south latitude. Say mentions the most southern range as being the confluence of the Ohio and Mississippi rivers, about 37°. The east and west ranges of this district are from one side of the continent to the other, the barren districts excepted. In the eastern hemisphere, Brandt and Ratzeburg fix the northern range at 67°, and the southern at 36°.

Few animals present more interesting facts connected with their habits than beavers; but pharmacological lectures are not adapted for such subjects. I may, however, remark that these animals have been principally celebrated for the instinct (almost amounting to intelligence) evinced in the construction of their habitations, and in the building of dams to increase the depth of the water, and thereby to prevent its being frozen to the bottom. Hearne has published a good account of the beaver, and to his work, therefore, I must refer you for further information: as also to the 65th number of the Penny Magazine, for April 6th, 1835. It is a curious fact, that, when much disturbed, these animals change their mode of living: they no longer construct dams and houses, but merely burrow at the banks of the rivers; and, instead of living in colonies, they live isolatedly. We have evidence of this in America and Europe. The animals are then very absurdly called *Castor Terriers.*

The capture of beavers is effected in various ways: sometimes the animals are caught in traps, sometimes in nets. But the usual method is to break up the houses, when the animals retreat to their bank-holes, in which they are easily taken.

The physical properties of the castoreum of commerce next deserve our notice. Two principal kinds are described; one brought to us from America, by the Hudson's Bay Company, and another obtained from Russia. I was told, at the house of the Hudson's Bay Company, that the average annual importation of castoreum from America to this country, is about 2000lbs.; and of this quantity, at least 1600 lbs. are exported for continental use. The quantity of Russian castoreum brought into this country is very small.

1. *American castoreum.*—On the continent this is called *English* castoreum, because it is obtained through our merchants. It is also frequently termed *Canadian*, or *Hudson's Bay castoreum*, though these two last terms properly apply to distinct varieties.

American castoreum is that commonly, or almost universally, met with in the shops. It usually consists of two isolated sacs, frequently wrinkled, and which are connected so as to form two parts, like a purse, or like two testicles connected by the spermatic cords. The size of the sacs is liable to considerable variation. They are elongated and pyriform. The penis or the oil sacs, or both, are sometimes attached to them. The colour and other external characters are variable. In December, 1834, I examined between three and four thousand pounds of castoreum, which was offered for sale by the Hudson's Bay Company. A considerable quantity of it was covered externally with a bluish white mouldiness, while the remainder was of a brownish colour. The brown colour, however, varied considerably; sometimes being dark, in some cases yellowish, or even reddish. Some castor sacs are found nearly empty, and present, in their dried state, a very fibrous character: these are of inferior quality. Others are found gorged with unctuous matter, and, when quite dry, break with a resinous character, presenting no fibres until they have been macerated in spirit of wine. In many well-filled sacs the castoreum is quite soft.

In English commerce, two varieties of American castoreum are made: one called the *Hudson's Bay*, the other the *Canadian* castoreum. Both are imported by the Hudson's Bay Company. The *Hudson's Bay castoreum* is usually considered the finest variety. The specimens of it which I examined at the house of the Company, in December 1834, come from York Fort, and Moose River. The finest were cer-

tainly superior to any of the Canadian kind, though the average quality was much the same. Jobst has described a third variety, which he calls *Missouri castoreum:* it was of good quality.

2. *Russian Castoreum.*—Most pharmacological writers distinguish a kind of castoreum under the name of *Russian, Siberian, Polonese,* or *Muscovite* castoreum; and in the London Pharmacopœia we are ordered to employ Russian. This substance, however, is exceedingly rare, fetching a very high price, while its medical properties are not superior to those of the American kind. The specimen which I here show you cost me 2*l.* per ounce, while the finest kind of American castoreum may be bought at about the same sum per pound. In France, also, it appears to be still scarcer, for Professor Guibourt acknowledges that until 1831 he had never seen it. Forty ounces of it were, in that year, brought to Paris, from Moscow, and the price asked was 80 francs (that is, 3*l.* 4*s.*) per ounce; but, with the exception of half an ounce purchased by Guibourt, none of it was sold.

I have met with no description of Russian castoreum which precisely agrees with the specimens before us; but the best account is that given by Geiger, in his *Handbuch der Pharmacie.* The following are the principal characters (as far as I have ascertained them) by which it is distinguished from the American kind.

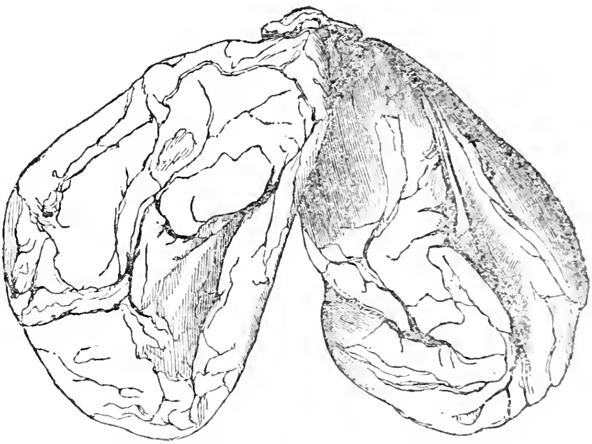

FIG. 41.—*Russian Castoreum.*

The sacs are *smaller* and more rounded than the American: they are compressed. The specimen before us, consisting of a pair of these sacs (fig. 41, natural size), weighs 557 grains. The specimens which I have seen had neither penis nor oil sacs attached; but Geiger says the latter are sometimes present. The colour is ash-brown. The odour is remarkable and peculiar, and instantly distinguishes it from the American kind. I should describe it as being empyreumatic: some persons have compared it to the odour of Russian leather. The internal appearance of the sacs is very different from that of American castoreum: the colour is brownish fawn, and quite dull, not having the least shiny or resinous character. American castoreum softens under the teeth like wax, and has an acrid taste; whereas Russian castoreum breaks down like starch, at first has little taste, then becomes somewhat bitter and aromatic. One of the most striking distinctions between the two kinds is, dropping a piece, about the size of a pea, into dilute muriatic acid: no obvious effect is produced on the American, whereas the Russian castoreum effervesces apparently as much as if you had dropped a lump of marble into this acid. This effervescence arises from the large quantity of carbonate of lime present. [The experiment was here shewn.]

The tincture of Russian castoreum, formed by digesting one part of this substance in sixteen of alcohol, is of the colour of deep sherry wine; whereas that made with American castoreum is nearly as dark-coloured as London porter. When added to water, the former produces a very slight milkiness only, and which is hardly altered by caustic ammonia; the latter, however, renders the water very milky, and when I add ammonia, a bright yellow-coloured liquid is obtained. [The experiments were performed, and the results shown.] The aqueous de-

coction of Russian castoreum is yellower than that of the American. Tincture of galls produces, as you see, no marked effect on either: the tincture of muriate of iron renders them both slightly turbid, and gives them a greenish tinge.

The castoreum mentioned by Guibourt, under the name of Russian, I have never

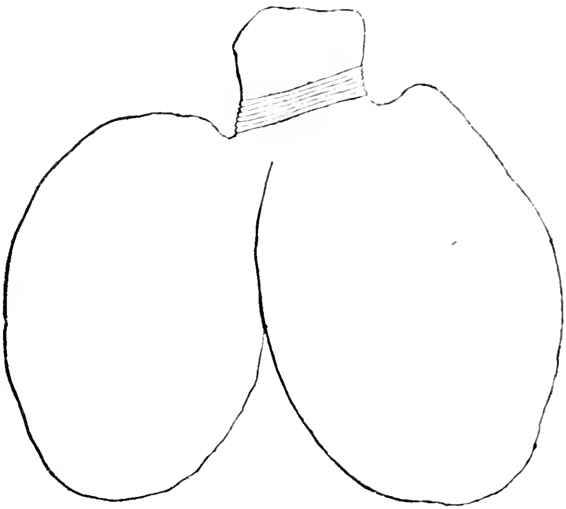

Fig. 42.

seen (fig. 41). It is evidently a different substance from that which I have described. The pods were full, rounded, broader than they were long, and didymous, or formed of two pods blended into one. Out of the whole forty ounces imported, only one specimen consisted of two ovoid pods, separated to the extent of three-fourths of their length (fig. 41). It seemed to have undergone some preparation: it hardly coloured alcohol, not only because it furnished little soluble matter, but because it wanted the colouring matter.

Chemical properties of Castoreum. — The most important substances in castoreum for me to notice, are the *resin*, the *volatile oil*, and the substance termed *castorine.*

1. *Volatile oil.* — Bonn states, he obtained 34 per cent. of volatile oil from Russian castoreum; but there must have been some error. Brandes obtained only one per cent., which I think is about the average quantity. By distilling the same portion of water several times, with fresh quantities of castoreum, we obtain a small quantity of oil, having a pale yellow colour, the odour of castoreum, and an acrid bitter taste. Here is some *aqua castorei*, holding a little of this oil in suspension. It is now nearly two years since I prepared it, yet it is apparently unchanged.

2. *Castorine.*—Boil castoreum in alcohol, and filter while hot. As the liquid cools a slight turbidness is observed, and a crystalline fatty matter is deposited, which has been denominated castorine. I have sometimes obtained it by boiling castoreum in pure æther: the castorine readily dissolves, while the resin remains undissolved. Russian castoreum yields a much larger quantity than any specimen of the American kind I have tried; but I do not think that even in this it would amount to one per cent. [Castorine obtained from the two kinds of castoreum was shown.]

Castorine was regarded by Fourcroy as adipocire; by Berzelius it is considered as nearly allied to æthal; by Gmelin it is considered analogous to the concrete volatile oils, and is termed *castoreum camphor.* It is a crystalline, fusible, non-saponifiable, fatty matter. In the liquid state it floats on water; when pure, is quite white; it is soluble in æther and in boiling alcohol. By long-continued boiling with nitric acid, it is converted into *castoric acid;* a yellow crystalline substance, soluble in water. This acid combines with ammonia to form a yellow, crystallizable, super-castorate of ammonia, which precipitates the salts of silver, lead, and of the protoxide of iron, white, and the salts of copper green, but does not throw down the salts of barytes, strontian, lime, or magnesia.

3. *Resin.*—If we digest castoreum in

rectified spirits of wine, we dissolve out the resinous matter. You will observe that when I add a little of this mixture to water, there is produced a milkiness, owing to the deposition of the resin. If this tincture be evaporated, we obtain a dark-brown extract (impure resin), which is to be washed with boiling water, and then redissolved in a little cold alcohol (by which some urate of lime and potash is separated). By evaporating this solution we obtain the pure resin. Here is some of it: it has a dark-brown colour, an acrid and bitter taste, and a slight odour of castoreum. It is insoluble in pure æther.

4. *Carbonate of lime.*—I have already alluded to the enormous quantity of carbonate of lime contained in Russian castoreum. Bonn states he obtained 24 per cent. I should think the quantity much larger. Buchner says that American castoreum contains $52\frac{8}{10}$ per cent. of chalk; and Brandes found $33\frac{6}{10}$. I have never seen any American castoreum which yielded any thing like this amount. For little or no effervescence is produced by dropping it into muriatic acid; and by incinerating 60 grains of good American castoreum in a platinum crucible, placed in a muffle, I obtained only $1\frac{2}{10}$ grains of ashes. Now if the whole of these were lime, and existed in castoreum in combination with carbonic acid, the quantity of chalk would be very little more than $3\frac{1}{2}$ per cent. Buchner mentions, in one pair of castor sacs he found stony concretions composed of

Fatty wax (castorine?) and resin	6·9
Extractive matter, with salts (sulphate and muriate of lime), and an organic acid	2·6
Volatile oil, animal fibre, and water	27.7
Carbonate of lime	52·8
Phosphate of lime	10·0
	100·0

5. *Other constituents.*—I have very little to say respecting the other constituents of castoreum. *Water* constitutes nearly 24 per cent. of this substance: *ammonia*, about 1 per cent. The presence of *urates* and *benzoates* (*urobenzoates?*) in this secretion, is a remarkable circumstance. Other substances, described as constituents of castoreum, may perhaps belong rather to the coats—such as *albumen* and *osmazome*.

Analysis of Russian Castoreum, by Bonn.

Æthereal oil	31 (?)
Fat (castorine?) with resin	23
Carbonate of lime	24
Cellular tissue	19
	100

Analysis of American Castoreum, by Brandes.

Ætherial oil	1·00
Castorine	0·70
Castorine (with carbonate, urate, and traces of benzoate of lime)	0·35
Resin	12·25
Resin (with traces of benzoate and urate of lime)	1 60
Albumen (with traces of phosphate of lime)	0·05
Osmazome (with traces of lactic acid and salts)	0·20
Mucus	2·30
Animal matter	2·30
Carbonate of lime	33·60
Phosphate of lime (with organic matter)	1·40
Carbonate of magnesia	0·40
Sulphates of potash and lime, and phosphate of lime	0·20
Carbonate of ammonia	0·82
Skin	19·20
Loss, water, &c.	22·93
	100·00

This analysis does not agree with the results of my experiments on *American* castoreum. I find a much smaller quantity of carbonate of lime, but a larger portion of resin, than is here stated. If it were not distinctly said to be the American kind analysed, I should have supposed, from the results, that it were Russian.

Physiological effects. — More than two thousand years ago, we find castoreum classed among medicines "quæ ad uterum faciunt." For many years past, however, its uterine effect has been doubted, and in this country is now for the most part disbelieved. By some writers it is asserted that castoreum causes a sensation of heat in the stomach, and accelerates the pulse; and hence it is called a stimulant. A soothing or narcotic effect has also been ascribed to it.

In the year 1768, Mr. Alexander, of Edinburgh, undertook a series of experiments, in order to determine what are the real effects of this substance. He tried it on himself in various doses to the extent of two drachms; and the only effect he could discover was the production of eructations. It neither raised the temperature of the body (as determined by applying the thermometer to the pit of the stomach), nor increased the frequency of his pulse. Alexander therefore concluded, that if this drug possess any virtue, it should be given in much larger doses than is usually done.

In the year 1825, J. C. G. Jörg, Professor of Midwifery in the University of Leipsic, undertook a series of experiments to determine the effects of certain energetic

medicines. For this purpose he established a kind of society, on the members of which the substances were tried. It consisted of twenty-two physicians or students, the youngest being 21, the eldest (Professor Jörg), 45 years of age; two lads, one 14, the other 16 years old; and three females, one 45, a second 18, and a third 12 years of age. One of the substances tried was castoreum. The results were as follows:—

In doses of from five to twenty grains the only effects produced were disagreeable eructations. The experiments were tried on the members (including the three females) most susceptible of the action of medicines. On the females not the slightest effect was produced. Professor Jörg took half a drachm fasting, and in an hour repeated the dose; but the only effects produced were disagreeable eructations, with the odour of castoreum (which were not allayed by breakfast or dinner, and only ceased at night when sleep came on), and a slight uneasiness in the epigastric region. The appetite, the circulation, and the respiration, were unaffected. The results of these experiments, then, agree with those of Alexander, made nearly sixty years previously; and notwithstanding that they are opposed to the statements of some other writers, I am disposed to adopt them, because, in the first place, in both cases the experiments were made with the greatest possible care; secondly, though made in different countries, and at distant periods, the results agree precisely; and, thirdly, they accord with the experience of most medical men.

But if we admit the accuracy of the above experiments, and adopt the conclusion of the feeble action of castoreum in the healthy condition of the body, we have no right, it has been said, to transfer this conclusion to morbid states. Cinchona bark, which has such a powerful influence over the system when suffering from intermitting fever, has comparatively a slight effect in the healthy state. If I were to judge from my own experience, I should say castoreum has very little therapeutic power, for I have not seen much benefit from its employment in those cases to which this remedy is said to be adapted. On all practical points I place great reliance on the experience of Dr. Cullen. Now he declares that, when given in large doses, castoreum is certainly, on many occasions, a powerful antispasmodic.

It appears that the odorous particles of castoreum are absorbed, for they have been detected by their smell in the urine.

In classifications of the materia medica, we find castoreum sometimes called an antispasmodic; sometimes a stimulant, or excitant; at other times it is said to be a tonic, or nervine-tonic. Barbier thinks it has a specific influence over the brain, spinal marrow, and great sympathetic system; and hence the power which he asserts it possesses of relieving some diseases called spasmodic. Vogt asserts its principal operation to be on the ganglionic system, while its action on the spinal marrow is much less, and on the brain scarcely any. The facts which I have already detailed show that we have very little positive information regarding the action of castoreum, and, therefore, that many of the assertions just alluded to are for the most part hypothetical.

Uses.—Castoreum was formerly in great repute in those affections of the nervous system denominated spasmodic, such as hysteria, epilepsy, and catalepsy, more especially when these diseases occurred in females, and were attended with uterine disorder. In those forms of fever called *nervous*, this medicine has also been recommended. In the northern parts of Europe it is used to promote the lochial discharge, and the expulsion of retained placentæ. It is, however, little employed here, partly, perhaps, in consequence of its disagreeable taste and smell, its variable quality, and its high price; but for the most part, I believe, because practitioners consider it an almost inert remedy.

Mode of exhibition.—If you use castoreum, always give it in substance, either reduced to powder, or made up into pills. The dose should be at least one or two drachms. The *tinctura castorei* of the London Pharmacopœia contains only one drachm of castoreum to one fluid ounce of rectified spirit, so that two fluid ounces would be required for a dose, if we considered the castoreum only. The quantity of castoreum, therefore, is much too small.

ABSTRACT OF CASES

RELATING TO

SEVERAL MODES OF OPERATION FOR ERECTILE TUMORS.

By F. Lallemand,
Professor at Montpellier.

The following remarks on the treatment of Nævus, by Professor Lallemand, are extracted from the Archives Générales de Medécine, tom. viii. Serie ii. The plan proposed is, in principle, and almost in itself, the same as that first related in this journal* by Dr.

* Vol. vii. p. 677.

Marshall Hall. M. Lallemand leaves his needles in the tumor; Dr. Hall repeats the acupuncturation, and leaves in the threads: the object of both is to induce inflammation — to substitute cicatrix for erectile tissue. The process is *slow*, and hence the supposed *difficulty* in the cure; for there appears to be none if time be given, and the operations, which are neither attended by pain nor hæmorrhage, be duly repeated.

Case I.—*Tumor of the Gum—Excision fruitless—Trephine and actual Cautery successful.*

Etienne Beloux, nine years of age, and of a strong constitution, entered the hospital of Saint Eloi, January 17, 1833, for a fungous tumor of the gum of the lower jaw, extending from the last right incisor to the first large molar on the left side; with loss of the small molar, the canine, and the two incisors of the left side. The tumor, which was two inches long and one inch in height and in thickness, was indolent, of a deep violet colour, mammelated at its surface, of a soft consistence, and fluctuating. The slightest pressure indented it, and rendered it pale, though it immediately recovered its volume and colour. The portion of the lower lip corresponding to the middle part of the tumor, was of a violet-red colour, and traversed by very distinct and varicose vessels.

Excision and cauterisation had been tried three times in the space of two years; on each occasion the disease had increased, and for some months had made rapid progress.

On January 25th, the face being exposed to a strong light, I divided the lower lip by an incision, commencing at half an inch from the left commissure, and reaching to the under part of the jaw. I then made another incision, perpendicularly to the former, near the inferior border of the maxillary bone. The two upper strips of this crucial incision were dissected back, and kept apart by a contrivance which compressed the labial arteries. I then applied the trephine to the two upper layers of the surface of the maxillary bone. I cut off, with the curved scissors, the portions of the mucous membrane which were highly injected and varicose, and concluded by applying the white-hot cautery to those parts of the bone, or gums, which I had reason to suspect. The two upper strips of the lip were united by three needles and the twisted suture, as in hare-lip; the lower strips were brought together by two fastenings of the common suture.

On the 30th the union was complete, and in a few days more there were no signs of the incisions. On the 10th, a curved or semicircular plate of bone was extracted. Cicatrisation proceeded regularly in the interior. The patient continued a month at the hospital, and all went on well; the second teeth proceeded to grow, the face was perfectly regular, and the mastication unattended with any pain.

Case II.—*Nævus of the Upper Lip removed by Incision and the introduction of Needles.*

George Fischer had, from his birth, a red spot, of the size of a large lentil, on the middle of the upper lip. As he grew up, the spot increased, and gradually extended to the partition of the nasal fossæ, and to the interior of the nostrils.

On the 5th December, 1832, the patient entered the hospital of St. Eloi. He was then five years and a half old. The upper lip was red, thick, prominent, and extended over the lower about an inch. The swelling and redness terminated insensibly towards the commissures, and extended upwards to the mucous membrane of the nostrils. All these parts were soft and elastic, and turned pale upon the slightest pressure, but quickly resumed their colour. The swelling rapidly increased, from impatience, irritation, crying, &c.

After due consideration, I came to the following conclusions: — Accidental erectile tissue is only morbid in reference to its seat, inasmuch as it resembles in its organization the tissues which occur in the normal state: it has the same properties, and would accordingly be similarly affected under similar circumstances. On the other hand, lesions of the common erectile tissues are capable of cure by means of a cicatrix—a mode of treatment very injurious to the functions of the organ, as is plainly observed to result from wounds or lacerations of the corpora cavernosa, or spongy tissue of the urethra: the penis, during erection, bends towards the injured part, the inflammation having there substituted a harder tissue than

previously existed; or, in other words, the erectile tissue having been replaced by the fibrous.

Why should not the accidental erectile tissues be also capable of inflammation, of reunion, and of a similar change?

In the case of Fischer, the erectile tissue of the lip was perfectly soft and elastic, resembling, in this respect, the corpus cavernosum in its most healthy state. On the 24th December I removed, from the middle part of the fungus, a slip, from eight to ten lines in width at its base, by two cuts of the scissors converging towards the partition. Blood gushed out vehemently from numerous vessels, and the slips, immediately sinking down, assumed instantaneously the volume and nearly the ordinary colour of the lip. They were immediately seized by the fingers of several assistants, and the hæmorrhage was presently stopped. I quickly united the two slips, by four needles fastened at a distance from the wound, and passing through its entire substance. After the insertion of each needle, a waxed thread was employed to bring the lips of the wound together, and all loss of blood arrested by uniting the divided surfaces face to face: the space between the needles was occupied by numerous twistings.

On the following days there were *red* and painful swelling of the parts adjoining the wound, fever, and headache; but not a drop of blood was lost. On the third day, profuse epistaxis reduced the inflammation, and caused the fever to subside. On the fourth, I withdrew the two upper needles, leaving the threads in their place. On the fifth, I removed the remaining two needles: there was a slight discharge of blood and pus from the punctures, which continued for some time. On the eighth, the swelling of the lip and nose was sensibly decreased. On the 25th the threads were detached, presenting to view a solid deep cicatrix, drawing the loose edge of the lip five or six lines towards the partition of the nasal fossæ.

These changes took place *slowly*, but *continued several months after the operation.* The upper lip recovered merely its regular volume, and no longer lay over the lower one; all the parts involved in the operation were *changed* into a kind of *fibro-cartilage.*

Yet the partition of the nasal fossæ remained tumefied, and also the adjoining mucous membrane: this was, however, to be expected, as these parts were not included in the operation.

After the effect produced by the needles, on the parts into which they had been inserted, I thought of passing several of them through the partition, to excite inflammation and the subsequent formation of as many cicatrices. The first of these needles passed readily through the cartilage; but those which I attempted to insert beneath, were prevented from entering by the cicatrix, and were bent like a fish-hook. This occurred on both sides; showing that the median cicatrix was harder than the partition of the nasal fossæ, and fully justifying the principle of the treatment.

Fifteen days after the application of these needles, a cartilaginous substance, like a lentil-stone, was felt in the track of their insertion. I applied others in the intervening space, and a similar change ensued. It would undoubtedly have extended further had the inflammation been more acute; but I was obliged by the patient's uneasiness to withdraw the needles after two or three days.

Case III.—*Nævus of the Cheek in an Infant treated similarly with success.*

Jean Amédée was observed to have, at her birth, a reddish spot about the middle of the left cheek, about three lines in diameter, and slightly prominent; its surface had the appearance of a raspberry; it became tumid and livid as soon as the infant cried. Its increase was rapid.

In the beginning of July, 1834, I made an incision into the tumor through its whole substance, and in the direction of its greatest length. I immediately united the two lips of the wound with four needles, which were kept in their places by numerous twistings of waxed threads.

On the next and following days the portions of the tumor which were unoccupied with the threads were considerably swollen, and became of a deeper and deeper violet colour; the surface was tense and glossy.

On the fifth day I removed the needles. Some drops of blood flowed, mingled with pus; this discharge continued several days through the holes of

the needles, forming an incrustation with the threads, of a volume and hardness which kept increasing. In about fifteen days this incrustation separated, exposing a cicatrix marked by the holes of the needles, which continued to suppurate for a long time.

The small wounds were frequently cauterized with the nitrate of silver, and the appearance of the surface gradually changed its character. The parts which had not been incised or pierced by the needles sunk down, and assumed a less deep, and afterwards a rosy, colour. In proportion as the cicatrization of these small seats of suppuration advanced, the adjacent parts spread, and became more and more free from colour. Finally all these cicatrices hardened in time, presenting only a pale, uniform, glossy surface, like those of the regular thin cicatrices which follow after a superficial burn of the skin.

In *five months* there only remained of the erectile tumor some red spots about the circumference, at too great a distance from the seat of inflammation to have experienced its effects. They became more pale and depressed, and would, perhaps, have disappeared. I then pierced them with very fine pins, and the inflammation thus produced transformed them into fibrous tissue. The whole of the cicatrix is thin, pale, and tense, like that of a burn.

Case IV.—*Nævus of a large size over the Scapula — Inflammation excited by the introduction of* 120 *pins — Cure.*

Dolores, of Villa Parma, an infant of very delicate constitution, was born with a red spot, about an inch in diameter, near the middle of the left shoulder-blade. It increased rapidly. In three months it was about three inches in length and two in breadth, and projected from the surface of the skin more than three lines. Its surface was like that of a raspberry, which it also resembled in colour. Compression had been tried in vain.

I applied a corset and pad; and in the beginning of December, 1834, I applied a dozen fine pins to the inferior extremity of the tumor, and covered the spaces between them with numerous twistings of waxed thread. The child cried but little, and was soon quieted. Three days afterwards I performed the same operation on the opposite side of the tumor. I operated in a similar way upon the whole circumference of the tumor, leaving the pins in their places until they had produced sufficient inflammation, *i. e.* seven or eight days or more.

In forty days the whole circumference having fallen, or being in a state of suppuration, I proposed to proceed in the same way with the centre, when I observed that it was livid, tumefied, and very hot. The health of the child was rather deranged, and I waited in consequence a few days. But, *to my great surprise*, I observed the parts which had been subjected to the previous treatment gradually suppurate, or shrink and become pale; so that in fifteen days the erectile tissue of this central part was completely changed into a flat and uniform cicatrix, of about two inches in extent.

It remained for me now merely to operate upon several points of the circumference, which had escaped the inflammation produced in the adjacent parts.

In two months and a half of this treatment the tumor had entirely sunk down; nine-tenths of its extent were replaced by a uniform and glossy cicatrix, which became every day of a paler hue. The parts which were last operated upon alone presented a degree of prominence and redness; they, however, shrunk, and acquired such a consistence that fibrous tissue formed along the track of the insertion of the needles.

In the course of the two months and a half 120 pins were inserted, yet not a spoonful of blood was lost; and the little patient, so delicate in health, was only slightly indisposed for two or three days.

It is also remarkable that the central part of the tumor, which had not been traversed by a single pin, partook of the inflammation of the adjacent parts, and underwent the same change.

Remarks by M. Lallemand.

These cases show that acute inflammation of the accidental erectile tissues, by whatever cause it is produced, is sufficient to effect a cure.

Without endeavouring to explain how this vascular net-work continues to extend itself, sometimes with excessive rapidity, from the part first affected to the adjacent parts, it is evident from

what has gone before that the inflammatory congestion deposits, in the first instance, gelatino-albuminous fluids in the meshes of this vascular net-work; and that, the water which held them in the liquid state being absorbed, they became thick, and continually denser, assuming the form of a new tissue of a harder kind, and consequently more impervious, than that which previously existed.

This principle explains the livid swelling, softening, and suppuration, which occur in the first period of the treatment, and the unexpected results which successively take place in the second.

As absorption continues to act upon these materials for a long time, with an energy decreasing as the matters become less aqueous, it takes a considerable time to ascertain the entire result. The longer the operation has been performed, the more favourable is the aspect of things.

The preceding cases point out a mode of cure, when compression is out of the question, and entire removal of the affected parts dangerous; and when entire removal is unattended with danger, but is likely to produce serious deformity, the means suggested are unattended with danger, or even acute pain.

The most effectual mode of producing inflammation of a diseased part is undoubtedly *acupuncturation*. The following are the results of my own observations on this subject.

I at first used very fine sewing-needles, with a view to introduce them more easily; but they are very inconvenient, as they can be neither cut nor bent, and must therefore be kept in their place for a long time. The erectile tissues are so soft, that it was easy for me to replace the needles by very long and fine pins, such as are used for fixing small insects; these are made to enter in every direction without any difficulty, and they can be easily cut with common scissars, or their extremities bent with pincers; they are very easily managed, and in every respect most convenient.

Finally, we should never forget that inflammation is the essential agent in the change which is induced, and that it always brings on this result, if sufficiently acute, whatever means may be employed to excite it, or to raise it to the proper degree.

A CASE OF

LARGE TUMOR REMOVED FROM THE SCAPULA.

By Henry Earle, Esq. F.R.S.

(Read before the Royal Medical and Chirurgical Society, Tuesday, Nov. 10, 1835).

The following case, which occurred lately at St. Bartholomew's Hospital, may perhaps, from its novelty, possess sufficient interest to deserve the attention of the Fellows of the Society:—

James Jackson, ætat. 55, by trade a comb-maker, was admitted on the 10th September, in consequence of a large tumor situated beneath the left scapula. He stated that about thirteen months previously his attention had been first called to it in consequence of the pain which he experienced in the left shoulder, which extended down his arm. About ten weeks before his admission he perceived a swelling under the scapula, which gradually increased up to the time of his admission. It had by that time acquired the size of an ostrich's egg, and was situated immediately beneath the scapula, extending beyond the base of that bone about three inches, towards the spinous processes of the dorsal vertebræ. It partook of every motion of the scapula, and was evidently in contact with it. The patient was still able to move the arm and shoulder, when the whole tumor moved freely on the surface of the ribs. He had been a very free liver, and his general aspect was bloated and unhealthy.

The base of the scapula could be distinctly traced, and, as far as could be ascertained, no absorption or alteration of structure had taken place in that bone. A cast was taken of the tumor, and it was accurately measured.

Leeches were applied, his arm was kept quiet in a sling, and his general health was attended to. From week to week the tumor perceptibly increased, and he suffered constant pain down the arm.

Several consultations were held on the case; but the nature of the tumor, and the extent of its attachment to the scapula, could not be satisfactorily ascertained, which led some gentlemen to negative any operation, from an ap-

prehension that it might be malignant, and that it would probably require the removal of the greater part of the scapula. On an attentive consideration of all the circumstances of the case, I was induced to entertain a more favourable opinion; and as the patient was very anxious to undergo any operation, I undertook to perform it.

The circumstances which influenced my opinion were the healthy state of the integuments over the tumor, and the apparently natural condition of the outer surface of the scapula, which would, I conceived, have been included in the disease, and have undergone some change in the course of thirteen months, if the disease had been malignant. The tumor was smooth and regular on its surface, and very firm, having no feeling of elasticity like fungous disease. But the circumstance which weighed most with me was the fact that the serratus magnus anticus could be put in action, and its several digitations could be distinctly felt at the side of the chest. From the situation of the tumor it was certain that the fibres of this muscle must pass round the whole mass to be inserted into the base of the scapula, and that the tumor must be situated between this muscle and the under side of the scapula. As this muscle was still capable of being put into full action, I inferred that it could not have acquired any intimate connexion with the tumor, and I entertained hopes that the latter might not have formed any very firm adhesions with the scapula; and that, although it was in great part covered by that bone, still it was not any morbid growth proceeding from it, an opinion which the natural condition of the upper surface of the bone appeared to warrant.

On Saturday the 3d of October, having fully stated all these circumstances, I proceeded to perform the operation, with the understanding that, if found to be necessary, any part of the scapula that might be included in the disease should be removed. I made a very free lunated incision from above the spine of the scapula round the base of the tumor, to below the inferior angle of the scapula. The integument was dissected up towards the scapula. The trapezius and the two rhomboids were successively divided and reflected. The fibres of the serratus magnus then came into view, being greatly elongated and spread out over the tumor. I next divided these near their insertion into the base of the scapula. On endeavouring to detach the tumor from beneath that bone, I found it firmly connected near the inferior angle, but readily separable beyond this part. It was only necessary to divide a few fibres of condensed cellular membrane, and with my finger and the handle of a scalpel I succeeded in removing the whole tumor, with the exception of a small portion near the inferior angle, which I cut through and subsequently detached without difficulty, as it was only connected with the bone in consequence of its having caused a small circular part of the bone to be absorbed at this part, to the extent of half an inch, through which the tumor had made its way; but it was readily drawn out, leaving the bone bare and rather rough. A small projecting spicula of bone was removed with the forceps.

Nearly the whole of the subscapularis muscle had been absorbed, leaving the bone bare, but quite healthy, with which the tumor was in contact. The part where the bone had been absorbed had been crossed by the latissimus dorsi, and had been subjected to more pressure from that circumstance. It was only necessary to tie two arteries, and the wound was approximated, leaving sufficient space for the escape of any discharge. A compress and bandage were applied, and the arm fixed to the side. Not a single unfavourable circumstance occurred; the greater part of the wound healed by the first intention; very little suppuration followed, and the patient quitted the hospital quite well on the 4th of November, having recovered the use of the arm and shoulder.

The above case I consider very satisfactory and instructive, in proving the value and correctness of reasoning on anatomical facts, as certainly I was principally induced to undertake this apparently formidable case from the circumstances which I have mentioned of the free action of the serratus magnus. When the whole of the tumor was removed, the fibres of that muscle could be distinctly seen, forming a muscular pouch or cyst, which had surrounded all the inferior and outer surface of the tumor. When examined, the tumor was

found to consist of a structure much resembling the intervertebral fibro-cartilage, but in some parts it appeared more vascular, and somewhat softened. Mr. Kiernan considered it to be a specimen of albuminous sarcoma.

George-Street,
Nov. 9, 1835.

IMPALEMENT ON A BROOM-SHAFT.

To the Editor of the Medical Gazette.

SIR,

THE perusal of a case, related by Mr. Ilott, in No. 4 of your periodical, induces me to forward a condensation of the notes of a similar one, which is at your disposal.—I am, sir,

Your obedient servant,
CHAS. COTTON, M.R.C.S., &c.

Lying-in Dispensary,
Nov. 14, 1835.

—— Woods, æt. about 50, by trade a broom-dasher, fell a distance of several feet upon a broom-shaft, standing in a perpendicular position, the point of which entered the scrotum between the testicles to the right side, passing up beneath the abdominal muscles to at least three inches above the umbilicus. The shaft, owing to its tough nature, was not broken, but bent, so that one extremity was extended upon the abdomen, whilst the other was firmly imbedded apparently in the sheath of the rectus beneath the muscle. Much difficulty was experienced in its extraction, and slight hæmorrhage followed.

The very exhausted state of my patient required the administration of a slight stimulus and application of warmth. In the evening symptoms of great constitutional excitement followed, together with pain of the abdomen, and in the course of the injury, which was marked by a red streak. These were combated by bleeding, a large dose of calomel and opium, salines, leeches applied over the red line caused by the accident, followed by fomentations and poultices to the wound, which afterwards suppurated kindly, and in five weeks my patient was well,—a circumstance unanticipated, when I state the man to have been given to very irregular habits, and the extent of wound, its adjacency to the peritoneum, &c. are taken into consideration. I was called to the man shortly after, he having, in a fit of inebriety, committed suicide.

MEDICAL GAZETTE.

Saturday, November 28, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

HYGIENE OF PRISONS AND WORKHOUSES—PRIVATION OF FOOD.

THE Commission appointed by Parliament for inquiring into the state of our prison discipline, will bring to light, we doubt not, some extraordinary facts. Were its labours further extended to an investigation of the discipline and regimen observed in our workhouses, we are no less satisfied that the result would prove somewhat startling to the nation. We have heard strange stories of both. There are jails in which it is the regular rule to keep all prisoners on bread and water, *even previous to trial;* and there are workhouses where the diet is so miserably insufficient, that the wretched inmates are sometimes obliged to commit crime, in order to have themselves transferred to a prison where they may have a better chance of sustenance.

If examples of this sort be found not unfrequent amongst us—and we cannot refuse credence to the instances just referred to, they having attained some considerable share of notoriety—what must be thought of the legislatorial wisdom that has directed or permitted such an order of things? The Judges of Assize are, we believe, entrusted with the regulation of prison discipline, in so far at least as relates to the dietetic treatment of prisoners, even in cases where the latter have been merely committed, and are not yet brought up for trial. But who has given power to the managers of workhouses to maintain a starving regimen among the paupers committed to their charge? A modicum of bread, and sometimes a little cheese, are

scrupulously weighed out to each pauper: the *same* quantity for each and for all; and this is to be swallowed with the aid of cold water, of which, no doubt, the poor wretches are munificently allowed to take as much as they please.

The system somewhat varies, we are told, in different parishes; but what can we expect, when even in the workhouse of St. James's, a pauper is driven, by the necessity of sheer hunger, to break windows, that he may be committed to a house of correction? Such an instance occurred very recently; and it came out in the investigation before the magistrate, that the statement of the starving wretch was not to be contradicted.

In the greater number of parish workhouses, it may be said, the system is different: the inmates get a sufficiency of wholesome but plain—very plain, food; they get soup—not wholly destitute of the flavour of meat. By such statements as these, however, the general position is only strengthened; and all we want to know is, why in any instance there should be a power of starvation vested in any individual or set of men?—a power the more dangerous, and the more likely to be cruelly abused, when we observe the unwillingness with which admissions are daily granted to those abodes of despair. If there be an interest in excluding paupers as long as possible from within the walls, why may we not suspect an interest also in getting rid of them when once admitted? The inference is inevitable.

We have had occasion frequently of late to call attention to the treatment received by *sick* paupers from the authorities who have the power, but not the will, to procure them proper medical relief. It seems that in sickness or in health the case is the same; and, strange to say, in this country, which boasts of its legal provision for the poor, we are doomed to witness a degree of tyranny exercised towards the pauper class which would disgrace the least civilized state in Europe.

There is no European state, we believe, with the exception of our own, in which the management of such matters is left to so capricious a destiny. Every country in Europe, but this, recognizes some hygienic arrangements or other, regarding its prisons and houses of correction. So far, however, as we have been able to gather, it has here been only a system of experimenting, in the different pauper and prison establishments, on how scanty an allowance of food the miserable wretches may remain alive: if they barely escape apparent death from starvation, the purpose seems to be answered; no matter even if cachectic diseases should make their appearance. The result of the experiments made on the paupers at the Millbank Penitentiary, a few years ago, is on record: a decoction of beef-bone, called soup, diluted to the extent of about 100 quarts from a single ox-head *, was administered for a time,—but the consequences were alarming. It was unluckily found to give rise to an epidemic among the prisoners; they were seized with general debility of body, with emaciation, and an eruption resembling sea-scurvy. Upon altering the plan of diet, however, the effects ceased. It might have been thought that the public investigation which led to the exposure of these facts would have put the experimentalists upon their guard; but the reverse seems to have been the case: *we shall see*, at all events, when the parliamentary commission shall have closed its labours, and given us a report.

* The Regulation literally ran as follows:—"The soup to be made with ox heads in lieu of other meat, in the proportion of one ox-head for about 100 male prisoners, and the same for about 120 female prisoners," &c.—Report, App. p. 368.

In connexion with this subject of the extent to which starvation may be carried, we cannot refrain from alluding to the valuable case of abstinence reported by Dr. Sloan, of Ayre, in the last number of this journal. It shows to those who are curious about the matter (our parish functionaries may look upon it as an experiment worthy of note) how long the oil may be withheld before the lamp of life is extinguished. We know not that we have ever met with a better example than this of the terrible effects of famine. Others there may be, perhaps, more showily narrated; but we question if any of them be more trustworthy. The celebrated case of Viterbi, of Corsica, for example, is more than apocryphal. It was drawn up, in the first instance, for general readers, and still continues to be referred to in certain books, as a complete case of determined suicide by abstinence. For above twenty days the murderer is said to have endured the tortures of hunger, and to have sunk at last through pure exhaustion. To the discomfiture, however, of those who cater for the lovers of romance, and who make so circumstantial a narrative of this wretch's last moments, it is now well ascertained that though he really abstained from food at first, he contrived to finish his career with a dose of arsenic—a fact legally authenticated by three medical men who were in attendance.

Perhaps the only other case of fatal abstinence worthy of being compared with Dr. Sloan's, is that related by M. Falret, in his book on *Suicide*. A tradesman, aged 32, in consequence of some reverses of fortune, resolved to starve himself to death. He accordingly, on the 15th September, 1818, withdrew to an unfrequented wood, where he dug his grave, and continued alive till the 3d of October following. At the latter date a chance passenger happened to light upon him, who, perceiving that he still breathed, eagerly lent him assistance; but the introduction of a little soup into the stomach proved fatal. Here was a case of abstinence proving fatal in *eighteen* days; and what renders it still more deeply interesting, is, that we have the details of the unhappy man's sufferings, kept by himself, in pencilled notes, up to within four days of his death. As we have noticed this case so far, perhaps we may as well subjoin some further particulars, briefly extracted from the journal, as given in the work alluded to. The curious reader will contrast the sufferings of the French and the Scotch victim, as well as the particular mode of enduring them displayed by each. It is to be remembered, however, that the Scotchman longed for life, while the Frenchman as ardently longed for death.

"In addition to the pains of hunger and thirst, the unhappy man had to endure those of cold and rain. On the night of the 16th he was soaked through, and during the following day and night was obliged to keep in constant motion, in order to resist the cold. His thirst forced him to lick the dew off some mushrooms which grew around him. By the 20th his hunger and thirst attained a frightful pitch: there had been no rain for the last three days. On the 21st he could not resist the temptation of running to an inn, about a league distant, where, for the last piece of coin in his possession, he bought a bottle of beer. This procured him but little relief; he determined, however, not to stay so far off from the inn in future, that he might at least in his agony taste pump-water. He tried this water on the 22d, but his stomach rejected it. Sleep was completely banished; he had not closed his eyes for the whole week preceding. His limbs now refused to bear him towards the inn: every day he expected to be his last; yet in the midst of his sufferings he did not neglect the insertion of some lines in his journal. Five days more elapsed, when, it seems, a shepherd saw him at a distance, but took no particular notice. Heavy rain came down

on the night preceding the 29th; some of it he received in his mouth; but, as he noted (in the last words contained in the journal), *water* could not now appease his tortures. What he suffered during the remaining four days can only be conjectured: it is to be hoped that during that time he was without consciousness * "

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, November 24, 1835.

HENRY EARLE, ESQ., F.R.S., PRESIDENT, IN THE CHAIR.

THERE was this evening a remarkably full attendance of members. Dr. Faraday and Mr. Clift were elected honorary Fellows. Two papers were read: the first was the *History of a remarkable Case of Varicose Aneurism*, from the pen of Mr. J. G. Perry, Surgeon to the Marylebone Infirmary. It was a case in which death ensued in six days after an operation for aneurism of the femoral artery, in consequence of the sac having communicated with the accompanying vein. We shall take another opportunity of laying fuller particulars before our readers.

Mr. ARNOTT having inquired as to the state of the posterior tibial artery, and being informed by Mr. Stanley and the author of the paper that this and the other arteries in the neighbourhood of the aneurism were apparently sound, proceeded to make some observations on the anomaly which the case presented from several others described by Breschet, Dupuytren, a writer in the Glasgow Journal, and one lately witnessed in the Middlesex Hospital—in all of which there was more or less of a venous condition about the coats of the arteries, being much thinner than natural, and having a strong tendency to collapse.

Dr. BURNE remarked on the perpetual whizzing sound noticed in Mr. Perry's case, that this ought to afford a diagnostic sign of some value in distinguishing between common and varicose aneurism; the former being attended only with the bellows sound, and that simultaneous with the contraction of the pulse.

Mr. MACILWAIN introduced some account of a case of bronchocele of a very large size at present under his care. He had not thought it expedient to tie the superior thyroid artery, as the President said *he* had done with success in a case under his care at St. Bartholomew's; but the application of a seton appeared to him (Mr. M.) most advisable.

The other paper read was *A Case of Recovery from the Insensibility of Intoxication, by the performance of Tracheotomy;* the author Mr. G. Sampson, of Salisbury. The stomach-pump had been employed in this case (above a pint of brandy and other liquors having been drunk); but still the patient became more and more comatose. It struck Mr. Sampson that this condition was owing to torpor of the brain, the blood sent to it not being sufficiently arterialized, owing to an inertness or inaction about the glottis. Tracheotomy was accordingly determined on, and performed, with speedy relief to the respiration: the patient gradually revived, and in a short time was quite well.

Mr. HOWSHIP related a case of insensibility and narcotism, from laudanum, which was ultimately restored by artificial respiration; and

The PRESIDENT narrated a somewhat similar instance, where insensibility and cessation of the pulse having been produced by too large a tobacco injection, administered previous to an intended operation for hernia, respiration was re-established by inflating the lungs, and sensibility presently restored.

A paper by Mr. Stafford, on *Wounds received in Dissection*, was deferred till the next evening meeting.—Adjourned.

The following is an abstract of Dr. Yelloly's paper, read at the meeting of the 10th instant:—

Observations on Vascular Appearances of Mucous and Serous Membranes, as indicative of Inflammation.

The author begins by recapitulating the chief points in a paper of his published in the fourth volume of the Transactions, upwards of twenty years ago. In that paper he treated of those vascular appearances in the stomach which are sometimes mistaken for inflammation. He touched also on the state of the villous coat of the intestines, and of their outer or serous covering, with reference to similar pseudo-morbid appearances. He endeavoured, in fact, to shew—

1. That appearances of vascular fulness in the lining membrane of the stomach, whether florid or dark-coloured, in distinct vessels, or in extravasations of different sizes, are not to be regarded as unequivocal marks of disease, inasmuch as they are not inconsistent with a previous

* This case was first published by Hufeland, in his journal, and introduced to French readers by M. Marc, in the *Bibliothèque Médicale* for Jan. 1820.

state of perfect health. 2. That authors were deceived who inferred from these phenomena that gastritis sometimes existed in the living body without symptoms, and who conceived it a puzzling circumstance that such unequivocal marks of violent inflammation should be found where there had been none of the usual concomitants of pain or pyrexia. 3. That Morgagni was wrong in attempting to account for the fact by supposing a paralytic affection of the parts, whereby the symptoms were masked; and Haller also, in fancying that almost every kind of fever was attended with inflammation of the bowels. 4. That erroneous conclusions as to the cause of death were frequently owing to the same mistaken observations—the effects of putrefaction, and the spontaneous changes induced by the loss of vitality, being sometimes attributed to the action of poisons. 5. That hydrophobia and some other obscure diseases were often subjected to an absurd treatment, owing to the same errors. 6. That the vascularity in question is entirely venous, and due to the tendency which even the arteries have to supply the veins at the close of life. 7. That transudation also contributes to those appearances. And 8. That the fact of inflammation having existed previous to death, cannot be inferred merely from the aspect of the vessels in the dead part: there must at least have been symptoms during life.

The same views, with very little modification, will apply to mucous and serous surfaces generally. But the state of the spinal marrow is not so commonly understood, it being seldom inspected unless for some precise object. The author thinks, that owing to this want of familiarity with the general appearances of the spinal cord, an erroneous notion has been conceived of the nature of some diseases—such as tetanus. With his former paper, the author gave a plate representing the appearances in the stomach of some criminals who had been executed: along with the present communication we have a drawing which affords an example of the state of the spinal marrow of a hanged man; the appearance is very vascular.

Some remarks are then made by the author on the importance of accurate investigation into the morbid phenomena in the body after death; and he draws a distinction between the appearances that belong to external and internal inflammation.

"The phenomena of redness, swelling, heat, and pain, sufficiently indicate the existence of inflammation in *external* parts, but in affections of internal organs we are often led to our conclusions by imperfect analogies, and by symptoms and appearances more or less doubtful. Sensations themselves are, to a certain degree, deceptive or equivocal. They may be connected with very different states of the body, and may be very dependent on the temperament of the patient by whom they are described. In internal diseases we are by this means often left to form our conclusions, as to the existence of certain states of disease connected with the extinction of life, by the appearances offered on examination after death."

The changes which occur in an *external* inflammation after death—the redness in general disappearing, and the tumor subsiding—are necessarily owing to the altered state of the blood-vessels, both arteries and veins having parted with their contents, and leaving the parts to which they belonged nearly exsanguous.

But in *internal* inflammations, especially of the abdominal viscera, various modifications take place. Being connected with the venous structures of the liver and heart, in which a considerable portion of the blood is concentred after death, an obstructing cause operates on the veins which belong to them, and this, added to their softness of texture, produces more or less of fulness of the vessels; and hence the appearance of vascularity both on the external and internal surfaces of the parts.

Previous to death, it rarely happens that this sort of accumulation takes place; so that vascular fulness in the dead body must be viewed with caution ere we take it for an indication of the state of things during life. As to the florid state of vessels, which has often been regarded as a proof of arterial congestion, it merely arises from the arterial character of the blood remaining in the veins for some time after its transmission from the arterial capillaries.

The author expresses his satisfaction at the spread of pathological knowledge, both at home and abroad, of late years; and more particularly at the adoption, by distinguished foreign authorities, of the views advocated by him in his former paper. Although Tommasini and Broussais, with their followers, may be opposed to those doctrines, yet Andral speaks decidedly of there being *various degrees of vascularity which are not to be regarded as inflammation.* Otto, in like manner, even cautions his readers against the opinions of Tommasini and Broussais, respecting redness of particular viscera as indicative of inflammation (Pathol. Anat. p. 47, English edit.) But perhaps Billard, in his Treatise on Mucous Membrane, and Rigot and Trousseaux, in the Archives Génerales, are the most energetic of the

foreign advocates who maintain these views.

In conclusion, Dr. Yelloly invites the attention of the Society to a beautiful drawing of a human stomach, accompanying his paper: it represents a pseudo-morbid appearance of a number of depressions of various sizes, which a superficial examination might ascribe to alterations, but which are found to be natural depressions followed by the mucous lining. The patient had no symptoms of stomach disease.

LECTURES ON SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

BY DR. LATHAM.

ON SYMPTOMS.

Auscultatory Symptoms—Rhonchus and Sibilus—In what sense they are Dry Sounds—Where and how Rhonchus is produced—Occasional Rhonchus—Permanent Rhonchus—Where and how Sibilus is produced—Diseases in which Sibilus occurs—Chronic Bronchial Diseases more or less inflammatory—Asthma—Peculiar forms of Acute Bronchial Inflammation.

I HAVE mentioned a few auscultatory signs, Rhonchus and Sibilus, and large and small Crepitation; and I wish to see how far these will go in explanation of diseases of the lungs, before I proceed to consider others.

They belong to the mucous lining of the air passages, a structure, above all other structures which go to form the lungs, most obnoxious to disease; for it has been said, that, to whatever other structures disease may ultimately reach, it most frequently begins in this, and in whatever others it may begin, it almost always finds its way to this in the end.

I have called Rhonchus and Sibilus *dry sounds*, because I thought it particularly important to distinguish them from others which, arising from air and fluid in the act of mingling together, are properly denominated *moist* sounds. But I must warn you against inferring, from my designation of them as *dry* sounds, that a preternatural dryness of the air passages is essential to their production; for such is not the case. Indeed, respective to the conditions out of which they arise, I would rather say, of Rhonchus and Sibilus, that they were *not moist*, than that they were *dry*. And if you like it better, there is no objection to your speaking of sounds that *are moist* and sounds that are *not moist*, instead of sounds that are *moist* and sounds that are *dry*.

Rhonchus is the most fluctuating and inconstant of all sounds that belong to the lungs. It arises out of various pathological conditions, and out of some that do not deserve to be called pathological at all. It would be an affectation to pretend to speak of them all with precision.

People in perfect health are apt to have Rhonchus mixed with the ordinary respiratory murmur: they strive to clear their throat by a forced effort of expectoration, and sometimes bring up a little phlegm and sometimes not; they only displace it. Hereupon the Rhonchus ceases, and the respiratory murmur remains alone. Again, people suffering disease of the lungs are apt to have Rhonchus mixed with all sorts of unnatural sounds. They make an effort to dislodge or reject a little phlegm, and the Rhonchus disappears, but all the other unnatural sounds remain.

In these cases, the cause of the Rhonchus is evidently secreted matter from the surface of the bronchi: but why does it not produce crepitation, and not rhonchus?—simply because it is *not fluid* enough to allow the air to penetrate it, mingle with it, and pass through it. The air, in going in and coming out, passes by it and impinges against it, and suffers a vibration from it; and this yields the sound.

This Rhonchus, which is owing to tough and glutinous phlegm clinging to the part upon which it is deposited, is sometimes propagated over the whole, or a considerable part, of the chest: but then the extent of the Rhonchus is no just measure of the quantity of the phlegm. The phlegm may still be very small in quantity, but being lodged in the trachæa, or the first divisions of the bronchi, where they are very large and free, it vibrates, like a harp-string, to the impulse of air, and diffuses the Rhonchus far and wide through the chest.

But, whether this explanation be satisfactory or not, it is perpetually happening that a Rhonchus, heard in every part of the chest, is made to cease at once by a stout voluntary effort of expectoration. Indeed I may venture to say, from my own observation, that a Rhonchus may *almost always* be thus got rid of, whether it occur alone or be one amongst other unnatural sounds. And hence I infer that the cause of Rhonchus is *almost always* a small tough moveable piece of phlegm, adhering to the trachæa or first divisions of the bronchi.

The cause is, however, sometimes immoveable and permanent, and quite of another kind. Any thing, from within or from without, that has the effect of narrowing or obstructing a bronchial tube, in ever so small a space, may occasion the same sort of sound. Changes of structure within the parts themselves—such as a thickening of the mucous membrane, or the ossification of a cartilaginous ring; or morbid growths from without, such as a bronchocele, an aneurismal sac, or a tuberculous mass in the bronchial glands, or in the lungs themselves—have so compressed or distorted the trachæa, or certain bronchi, that the air could not force its way through them without continual vibrations, and without the respiration being constantly accompanied by a hoarse unnatural sound—by Rhonchus.

It is not easy to determine the conditions which are essential to the production of Sibilus. In seeking to assign them, I am left to my own reasonable calculation of what they *possibly may be*, having no direct means of proving what they actually are. I never knew any person die whose only auscultatory symptom was a Sibilus. Yet I believe people may and do die; and I have myself seen several in imminent peril of death, from disease of the lungs, whose only auscultatory symptom referable to the lungs has been a widely-diffused Sibilus. Of such cases I will speak presently: they are of great importance, and well deserve to be pointed out.

Sibilus, like Rhonchus, may occur alone, or in combination with other auscultatory signs. But whether alone or mixed with others, it cannot, like Rhonchus, be got rid of by an effort of expectoration. Yet the cause of both may be the same in kind, and different only in situation. A secreted matter, not fluid enough to admit air to mingle with its particles and thus produce a *moist* sound, but so consistent as to present a reverberating surface, and thus produce a *dry* sound, may be equally the occasion of Rhonchus and Sibilus. But in Rhonchus this matter is within reach of a voluntary succussion of the tracheæ and bronchi to expel it; in Sibilus it is beyond it. In Rhonchus it is contained within the first divisions of the bronchi; in Sibilus, within their lesser ramifications.

Sibilus, whether alone or in its combinations, cannot, like Rhonchus, ever be regarded as an innocent symptom. It is a more unquestionable evidence of disease than Rhonchus, in whatever that disease may consist.

Sibilus is almost always mixed with small Crepitation. They are united together in the same individual, and often succeed and supersede each other, as if they were contending together for the mastery; now one and now the other being predominant.

This mixture of Sibilus and Crepitation, and the predominance first of one and then of the other, are chiefly seen where both are largely diffused throughout the lungs: and in such cases one may often remark a fluctuation of the general symptoms answerable to this fluctuation of the auscultatory signs; that these symptoms, in their aggregate, become more inflammatory when the Sibilus increases, and less inflammatory when the Sibilus declines; also that the remedy which abates their general inflammatory character commonly abates the Sibilus; also when the expectoration is freer, the Sibilus is less, and when the expectoration is scanty, the Sibilus is more.

Now these are, indeed, great practical points, if they be true; and there are always examples enough to be found in this hospital by which you may test their truth. There are plenty of patients who have been suffering cough, expectoration, and dyspnœa, long and habitually,—some from disease which belongs primarily to the lungs—some from disease which is derived to the lungs from the heart. Watch these patients well for a few weeks together; mark the auscultatory signs and their fluctuations; mark the general symptoms and their fluctuations also; mark the treatment, and its adaptation to, and influence upon, both; and then judge whether the practical points, which respect the particular auscultatory sign now in question, are really such as I have represented them.

These cases of *chronic* bronchial affection are the most favourable for teaching the character of Sibilus, and how it stands related to other auscultatory signs, and to more general symptoms, and how it is amenable to methods of treatment. Such cases tell their story (if I may say so) more leisurely: they tell it over and over again, and with many interesting variations, and thus give you time to dwell upon it and understand it.

But still of Sibilus, that is thus mixed with crepitation, now superseding it, and now superseded by it—becoming less as the expectoration is more, and more as the expectoration is less—increasing as the general symptoms are more inflammatory, and yielding to the same remedies that they yield to;—of this Sibilus I do not pretend punctually to know the local efficient cause or exact pathological condition of the parts out of which it immediately arises. I am content to believe (without any curious speculation upon

things which I cannot prove) that, upon the access of a more inflammatory action, the secreted matter in many bronchial ramifications becomes more scanty and less fluid, so that the air that it breathed cannot freely mingle with it, and thus crepitation yields to Sibilus; and that, upon the subsidence of the inflammatory action, the secreted matter becomes more copious and more fluid, so that again the air freely mingles with it, and thus Sibilus in its turn yields to Crepitation.

But Sibilus does not occur under these circumstances only. It does not merely go and come, or occasionally intervene in chronic bronchial disease, of which the auscultatory symptom that is most characteristic and abiding is of another kind. There are cases in which Sibilus is itself the most characteristic auscultatory symptom,—cases in which for a long period, and cases in which even from first to last, there is no other auscultatory symptom whatever but Sibilus.

There are cases of (what I suppose would be called) genuine asthma, that present some such symptoms as these: dyspnœa, or rather an agony and fighting for breath; livid lips, cold and livid extremities, and a dry ineffectual cough, terminated and relieved, after an uncertain interval, by a copious puriform expectoration. Here, during the agony or paroxysm (and unfortunately it often continues long enough to allow a very leisurely examination of the chest by the ear—sometimes many days, sometimes a week or two) the sole auscultatory sign is a Sibilus pervading a larger or smaller portion of the lungs, according to the severity of the case. And as the agony lessens, and the expectoration begins to appear, Crepitation is found mingling itself with Sibilus; and, when the agony has *entirely* ceased, and the Crepitation become more copious and free, Crepitation, and Crepitation alone, is then heard in the same situations, and to the same extent, that Sibilus, and Sibilus alone, was heard before.

I have witnessed instances of asthma in several individuals, and several attacks of asthma in the same individual, where the auscultatory signs have had as strict and definite a correspondence with the stages, progress, and prominent symptoms of the disease, as that which I have here described.

Now, if absolute Dryness can be ever safely predicated of the respiratory passages, and can be ever safely reckoned among the pathological ingredients of their diseases, and ever clearly notified by one express symptom, it is in spasmodic asthma, of which it is the chief pathological ingredient during its first and often most protracted stage, and is clearly notified by a widely-diffused Sibilus.

I am persuaded that the natural moisture of the respiratory passages is *then* really in defect, and that Sibilus is really an index of the fact. Sibilus may then, if ever, be truly called a Dry sound. But I am not sure that the Sibilus directly results from the mere condition of Dryness; I doubt whether simple Dryness alone would naturally produce it. In consequence of its Dryness the mucous membrane may lose its elasticity, and become to a certain degree unyielding; or it may undergo wrinklings or puckerings at various spaces, or its general tumefaction may produce a narrowing of the smaller tubes, and thus present obstacles to the passage of air, and impart to it new vibrations; and hence the Sibilus.

Nevertheless, whether Dryness of the respiratory passages, or other conditions necessarily resulting from it, give immediate occasion to the Sibilus, the Sibilus may be properly regarded as the auscultatory symptom of the former. Dryness of the mucous membrane bespeaks a well-known pathological change; the other conditions are mere matters of conjecture.

Thus far I have spoken of Sibilus occurring in two forms of bronchial disease. 1st, As it intervenes among the auscultatory symptoms of certain chronic affections, characterized by dyspnœa, expectoration, and cough, instances of which are perpetually at hand to enable you to verify the fact. 2dly, As it presents itself as the sole auscultatory symptom that attends the paroxysm or agony of an asthmatic attack, when it is so marked that its presence can never be doubted. In both these forms of disease Sibilus conveys pathological and practical information of great importance.

But does Sibilus ever occur in acute bronchial or vesicular inflammation? And does it ever *so* occur as to throw essential light upon morbid processes going on, and essential light upon modes of treatment?

Inflammation of the bronchial ramifications perhaps never exists without the natural secretion of their mucous surface being either diminished or increased, and, consequently, without the accompaniment of those sounds which indicate its defect or excess; *i. e.* without Sibilus or Crepitation.

Sibilus is apt to occur at the beginning of such inflammation; and thus it corresponds with the pathological condition out of which it arises, the mucous membrane, when it is inflamed, becoming drier than ordinary before it yields a more abundant secretion.

Sibilus, too, after it has arisen, is apt to be of short duration, seldom abiding long as the *sole* auscultatory symptom of such inflammation. And herein also it corresponds with the pathological condition from which it proceeds; for the dryness of the mucous surface generally soon gives place to moisture.

Hence it happens that Sibilus is so seldom met with in practice, except with some mixture of Crepitation. The inflammation is, in truth, not submitted to our observation until the stage of *dry* sounds is passing, or has already passed, into the stage of *moist* sounds.

Nevertheless, there are cases in which Sibilus is the sole and abiding symptom derived from auscultation, and a Dryness of the air-passages the sole and abiding morbid condition for a considerable period. They are cases distinct from asthma, cases of genuine inflammation, and so remarkable as to require your especial notice.

I have met with a frightful affection in children; but what its nature was I could never tell, until auscultation enabled me to unravel it. It commonly passes for inflammation of the lungs. But when children have got well, they have got well so soon and so entirely, that I could never believe the disease to be pneumonia, although the symptoms seemed to indicate that it could be nothing else.

Last summer I went out of town to see a little boy, seven or eight years of age, whose life was very precious to his family. He was thought to be dying of inflammation of the lungs. I found him raised up in bed, supported by his nurse, and breathing with all his might. His skin was hot; his face flushed; and his chest heaved, and his nostrils quivered frightfully. There was no croupy sound. Whatever the disease was, it was all within the chest. I percussed the chest; it sounded well in every part. I listened: the air entered freely, and reached every cell and vesicle of the lungs; but there was not the least perception of the natural respiratory murmur; a shrill Sibilus had taken place of it altogether. Wherever you applied your ear to the chest, you might fancy you heard the piping and screaming of a nestful of unfledged birds.

But what was this disease? Surely it was inflammation largely diffused over the mucous surface throughout the bronchial ramifications, but inflammation as yet only in its *first* stage; for the air, as it passed through them, did not mingle with a particle of fluid any where, and the sound it produced was a dry Sibilus only.

But *how* inflammation yet only in its *first* stage? The boy had been already ill four days. Still it might be inflammation in its *first* stage. The boy continued ill two days longer, with the same kind and the same degree of suffering; and then, under the influence of tartar emetic, the fever began gradually to subside, and the dyspnœa to abate. The Sibilus gradually gave way to the healthy respiratory murmur, and he was well again *without expectoration of any kind.* The inflammation began and ended with the *first* stage; and although it continued with great severity for a week, it never got beyond the *first* stage.

This is an instance which strikingly shows the value of auscultation in detecting at once the state of things, about which you might go on conjecturing and conjecturing for ever what it *possibly* might be, and not gain the least assurance what it *actually* is.

In adults sometimes, but not so frequently as in children, I have met with the same evidences of acute inflammation widely diffused through the bronchial ramifications, and remaining in this its first stage for days and days together. In the meantime their mucous surface has still been dry throughout a great part of both lungs, and the ear has continued for days and days together to hear no other unnatural sound but a Sibilus. Convalescence has taken place without expectoration, and the sibilus has given way, without the intervention of any *moist* sound, at once to the murmur of health.

But such inflammation, after lingering long in the first stage, will sometimes pass beyond it; and the whole mucous surface that was previously dry will pour forth an enormous secretion, and the widely diffused Sibilus will be changed into a widely diffused Crepitation. Still the lungs are unhurt beyond the lining membrane of the air-passages, and the patient will get well, if he be not suffocated by the enormous expectoration.

I am speaking of a disease which must be distinguished from asthma, according to the usual acceptation—a disease not habitual to the individual, and of which, perhaps, he has never suffered a previous attack. I am speaking of acute inflammation extending throughout the bronchial ramifications, and reaching, perhaps, the vesicular structure of the lungs, putting on a peculiar form, and affecting a peculiar course; but still of acute inflammation, as further evidenced by the remedies necessary for its relief.

During the last summer I saw a gentleman, who had been, two days previously, seized rather suddenly with feverish symptoms, and with the most dreadful dyspnœa.

His lips were blue; he was labouring for breath, and coughing with hard and ineffectual efforts to rid himself of something which seemed to tease the larynx, but no expectoration followed.

Cupping on various parts of the chest (the state of vascular action required that blood should be drawn), and tartar emetic in frequent doses, were the remedies employed; but in the same state of agony he remained for a week, propped up in bed, striving with all his might to free himself from his oppression, coughing and endeavouring to expectorate, but ineffectually.

What was going on all this time? There was anguish enough for any disease of the most formidable name; for fluid in the pericardium; for extensive hydrothorax; for induration of a whole lung; for stricture at some orifice of the heart. A few years ago the most sagacious physician could only have guessed at the real state of disease, and probably would have guessed wrong. Such severe dyspnœa, so long continued, without expectoration, would probably have determined his diagnosis to hydrothorax.

But what was the disease? Every part of the chest sounded well to percussion. The heart beat regularly, and with a natural sound, only with too great frequency.

What could it be? There reached the ear from every part of the chest to which it was applied a loud Sibilus. The disease was an inflammation largely diffused through all (perhaps) of the bronchial passages, great and small—inflammation abiding long in its first stage, and limiting itself to one structure.

But in this case the inflammation ultimately passed beyond its first stage; for ultimately there arose an immense expectoration, and so the disease reached a favourable termination.

I have said that it is the peculiar praise of auscultation, not merely to discover disease in its ultimate results, but to analyse it in its several processes as it goes on; to mark its stay and continuance in one process, and its passage and transitus from one to another. Here we have a conspicuous instance of both these circumstances discovered to us by auscultation.

It is, I suspect, among the characteristics of inflammation, that in proportion as it is more widely diffused, it should be less rapid in its progress; not necessarily less severe, as far as severity is measured by force of vascular action and by fever, but less rapid in accomplishing its whole course; dwelling longer in each particular stage before it passes to another, than the inflammation which begins at a point.

Of this we have an example in acute rheumatism, which is diffused over similar structures in many joints simultaneously. In acute rheumatism, action and suffering are carried to the utmost degree of severity. There are extreme heat, and extreme pain, and extreme vascular action, in the parts, and in the constitution at large; yet all are expended upon one stage of inflammation.

Rheumatism may exist for weeks and months together, with all its pain, and heat, and vascular action, unabated; chronic in duration, but most acute in what respects action and suffering, it should seem that any length of time was permitted to it to do all that inflammation can effect within *a certain* limit, but that it was restrained by a strong, though not an invincible, law, from transgressing *that* limit, or doing harm beyond it.

After many weeks or months the inflammation will cease, and every joint be restored to the form, and feelings, and functions of health.

Such inflammation may exist in internal parts. (I do not mean rheumatic inflammation. Do not let me perplex you with a name: I only refer to rheumatism as to something well known, for comparison's sake, or analogy.) Inflammation, I say, may exist in internal parts, which (like rheumatic inflammation of the joints) is of a diffusive character, and occupies a large extent of surface at once, travelling tardily from structure to structure, slow to disorganize, abiding long in each of its stages, and giving leisure for the application of remedies in all of them. Such an inflammation is especially incident to the lungs; and, of the pulmonary structures, especially to the mucous membrane which lines the air-passages.

I have given instances of such inflammation of the bronchi lingering in *its first* stage, its stage of mere vascularity and defective secretion; and I have dwelt upon these instances for the sake of shewing you the real value of Sibilus as a pathognomonic sign. But for it, in the instances specified, I could not have made out the nature of the disease.

Of Sibilus I will venture to observe, that sufficient regard has not yet been bestowed upon what it is *in itself*. It is usually spoken of as conducting to Crepitations, and mixed with Crepitations, or moist sounds. But there wants some illustration of it in its separate import, as standing alone. To that illustration what I have said may perhaps contribute a little.

CLINICAL LECTURE

ON THE

REMOVAL OF THE SUPERIOR MAXILLARY, AND OTHER BONES OF THE FACE,

Delivered at the Westminster Hospital,

By Mr. Guthrie.

Mary Brown, aged 46, was admitted July 7, 1835, into this hospital, from Codicote, in Hertfordshire. She had long suffered from pain in the right cheek; but it was only in the autumn of 1834 that she felt an enlargement of the upper jaw, for which she had a tooth removed without relief, and afterwards another, without any essential benefit, the swelling continuing gradually to increase, and the pain, which was occasionally pricking, as well as lancinating and exceedingly severe, to augment. The tumor was firm and springy, and left no doubt as to its nature. It is unnecessary to give a further detail, as the case has been already reported by some gentleman about the hospital; and it is also of no consequence, further than that a little difference of opinion prevented my proceeding at first to the operation, and her health afterwards prevented its being done until my return from a tour I had made for the re-establishment of my own health in September.

I had made an opening into the tumor nearly opposite the foramen infra-orbitalis, from which a probe could be introduced into a cavity of some extent; and there was an opening where the teeth had been, which appeared to lead into the same cavity, and the discharge from which being swallowed, seemed to give rise to the derangement of stomach and diarrhœa by which she was so much troubled. Her health being, however, a little improved, I thought it right to defer the operation no longer, and many of you saw it done, exactly three weeks ago.

This operation is one of the improvements of modern surgery, within the last sixteen years, and for which we are principally indebted to our neighbours the French. The removal of a part, or parts, of the superior maxillary bone, has been had recourse to since the early ages of surgery, to which we need not refer. The removal of the whole, with its attachments, and even of the neighbouring bones, was, I believe, first attempted in France, but it is not settled by whom. Velpeau, in his "Médecine Opératoire," says that M. Paillard decides in favour of M. Dupuytren; M. Pillet for M. Gensoul. It has been done by Messrs. Stevens and Rogers in America, and Messrs. Lizars, Syme, Earle, Scott, Robert, and others, since that period, and their cases are reported; but the letter of M. Gensoul, of Lyons, in 1833, is the best monograph I have seen, and to which I have been much indebted in the course of my researches on this subject. But I believe so many of the bones of the face—as the superior maxillary, the malar, the lachrymal, the palate, and the inferior turbinated—were never removed at one operation before.

The antrum of Highmore, and the superior maxillary bone, are liable to many diseases I do not intend now to notice. There are, however, three in particular which require an operation, which may be mistaken one for the other, and lead, therefore, to error. The first is the malignant disease, or diseases, of the part; whether it arises from the membrane lining the antrum in the first instance, or from the bone, is indifferent. In the present instance the disease appears to have originated in the bone, which had been changed into a fibro-cartilaginous substace, which Mr. Kiernan, who has examined it, and is paying great attention to this subject, calls an albuminous sarcoma, the cavity of the antrum being more or less entire.

The second is when the disease appears to depend principally on an augmented growth of the bony paries, which, as in one of the cases related by M. Gensoul, had attained the thickness of an inch and a half. This is probably the result of inflammation of a common, not of a specific kind, and is cured by the removal of the anterior portion, which is a simple operation.

The third is when the tumor is caused by a sort of dropsy of the antrum, from whatever cause it may arise, which leads to its distention, and the gradual thinning and enlargement of its anterior wall. This is always curable by making an opening between the gum and the cheek, through the bone, by a chisel or strong pair of large scissars, through which the contents may be evacuated; after which the part gradually resumes its natural appearance. In such a case the tumor may be very large, and assume many of the characters of malignant disease; but it may be distinguished by its comparative incapability of resistance under pressure; the thin external wall of bone yielding and giving a crepitating sensation, which is highly characteristic of the complaint. In all cases, in fine, where doubt exists, the tumor should be punctured before any other operation is commenced.

Before I proceed to describe the manner of operating in the present instance, I will endeavour to make you understand the operation, so as to enable you to do it yourselves if a case should occur to you rendering it necessary; and I will draw your attention to the attachments first of the superior maxillary bone, and afterwards to those of the other bones it became necessary to remove. It is fixed in its situation towards the nose by its nasal process; within that it is attached to the os unguis, and to the os planum of the ethmoid bone, by the edge of its orbitar plate. The line of this attachment, if the edge of a chisel is placed on the edge of the orbit, is diagonally across the eye, from below and within, upwards and outwards.

On the outside it is attached firmly to the malar bone by its malar process, orbitary edge, and plate, as far back as the spheno-maxillary sinus; but as the os malæ must in general be more or less removed, its ascending orbitar process forming the outer edge of the orbit, and its zygomatic process, should be well considered. You will immediately perceive that these two parts are readily divided, and the orbitar plate of the malar bone easily yields, if the chisel or saw be carried on towards the spheno-maxillary sinus, through the medium of the foramen lacerum orbitale inferius. To do this the chisel must be held diagonally, in the opposite direction to that on the inner side of the bone.

The palate bone is firmly attached to the superior maxillary behind, the palatine plates of each forming one-half of the bony palate. Each plate is also firmly united to its fellow on the opposite side, and forms a point of attachment, which must also be divided by the chisel. The ascending portion of the palate bone being attached anteriorly to the superior maxillary bone, and posteriorly to the pterygoid processes of the sphenoid, form the fourth and remaining point of attachment; and the palate bone must be therefore either entirely removed, or be partly broken off, in which case the greater part of the ascending portion remains *in situ*, whilst the palatine plate is removed.

The internal maxillary artery is the only one of any importance which can readily be divided; and it, or any of its branches, which require a ligature, may be easily secured.

The second branch of the fifth pair of nerves is the only great trunk likely to be divided; and this may always be done by the knife, near the posterior edge of the orbitar plate of the superior maxillary bone, before it enters the infra-orbitary canal; by which means the injury likely to result from tearing the nerve will be avoided.

The plate before you shows the distribution of the pes anserinus of the seventh pair of nerves, some branches of which must be divided; but it does not follow that much or any deformity should ensue, as depending on that source of evil. The naso palatine and the inferior palatine branches, from Meckel's ganglion, must also be injured or divided.

The woman being seated a little inclining backwards, with her head supported by an assistant, I plunged the point of the knife down to the bone, a little below the insertion of the tendon of the orbicularis palpebrarum muscle, and carried it directly downwards by the side of the nose (leaving room for the passage of a ligature between the incision and the edge of the ala of the nose) through the lip, in as straight a line as possible. A second incision was then made outwards from the first, commencing on a line with the edge of the ala of the nose, until it reached nearly to the lobe of the ear; and from whence this terminated, another was carried upwards perpendicularly, to the side, and about one inch distant from the edge, of the orbit, and above its middle. The flap thus made formed three sides of a square, the fourth, or upper side, being the edge of the orbit. By the division of the lip and the second transverse cut, the lower part of the cheek could be turned down over the lower jaw; and Mr. Hancock will show you on the dissected head on the table, at your leisure, the situation of the duct of the parotid, which was in this manner avoided.

The next step was to separate the upper part of the tumor from the malar bone and edge of the orbit, and to insulate it from its attachments on its external edge; which done, I cut off the whole of that which formed the projecting part of the tumor, opening into what had been the cavity of the antrum, and showing that the disease occupied the whole of the bone, and rendered its removal necessary.

When a bone is inflamed, whether from common, specific, or inflammation of a malignant nature, it always becomes soft, the earthy particles being removed; a circumstance which greatly facilitates all operations on inflamed bone, and which was of great advantage on the present occasion. The malar bone being involved in the disease nearly as high as its attachment to the external angular process of the frontal bone, I divided it, with the help of the chisel and mallet, a little below that point, and then the zygomatic process. Another stroke of the chisel from the spot first divided, separated the

orbitar plate of the malar bone from the orbitar plate of the sphenoid bone, and passed under the eye into the foramen lacerum orbitale inferius, thus separating the malar bone. I now isolated the tumor on the orbitar plate of the maxillary bone, raising the eye, dividing the second branch of the fifth pair of nerves before it reached the infra-orbitary canal and the inferior oblique muscle.

The next step of the operation was to clear the line of the first incision, separate the ala of the nose from its attachments, open into the nasal fossa, cut through the mystachial suture, or rather a very little to the right of it, which was readily done by the chisel, which was then carried on in the palate through the palatine plates of the superior maxillary and palate bones. The chisel being now placed diagonally from within upwards and outwards across the ascending and nasal processes of the superior maxillary bone, and across the lachrymal sac, it was easily driven through; passing between the orbitar plate of the maxillary and the os planum of the ethmoid bone. If you examine the skulls before you, the line of junction of these bones will be immediately perceived. The chisel, guided by my two fore-fingers, was now carried as far back in the orbit, under the eye, as it could conveniently go, when all the loosened parts yielded under a little steady depression outwards, and the body of the tumor came away on the division of the soft palate, some attachments on the outer side of the tuberosity of the maxillary bone, leaving two processes, one of which was attached to the tonsil, the other above to the body of the sphenoid bone. There had hitherto been little or no bleeding, and at this time one artery sprung, of some size; but which ceased of itself, after filling the mouth twice or thrice, and causing a little delay. From the situation of the internal maxillary artery, which you may see injected in the head on the table, you will perceive that it must have been a branch that was divided—probably a pterygoid, rather than any other. In order to remove the process of tumor adhering to the tonsil, I seized it with a hook, drew it inwards, and dissected round it with some curved scalpels I had had made for shaving the cartilages of the ribs; one being round, the other sharp at the point. You will see on the dissected parts the vicinity of the internal carotid artery; and you may estimate the danger there was of cutting it, by any irregular motion of the patient or of mine. This part of the operation was, however, accomplished in safety. Another portion of the tumor was attached above to the body of the sphenoid bone, and the base of it could only be separated from the bone with great care, and with equal difficulty. It was the only point about which I had any anxiety; and I begged Mr. White and Mr. Thomson to say if they could perceive any the slightest portion remaining, and Mr. Keate and Mr. Stanley were so good as to do the same. These two last steps of the operation took up by far the largest part of the time occupied in its performance; and if any return of the disease should occur, it will be, I feel assured, from the last mentioned spot, and as no more of this could have been removed, unless a portion of the body of the sphenoid bone had been taken away, and which it would not have been safe to do, I regret that circumstances prevented this operation being done some months sooner: it would, I think, have been much more easily performed, and these two last attachments might not have taken place.

I allowed half an hour to pass away (having placed the woman on a bed) before I brought the external parts together; which was done by five hare-lip pins and as many sutures made of lead, and twisted instead of being tied. The hare-lip pins I removed on the fourth day, and the others on the subsequent one; leaving the whole of the incisions united. A small spot gave way on the eighth day, near the inner angle of the eye, where the skin was very thin, and another at the outer corner of the cross incision, about the size of a pin's head, and which last has nearly closed. The skin over the upper part of the tumor, forming the under eyelid, was so thin that I had great fears for its safety; it appears, however, likely to do well ultimately, although it is inflamed and irregular in its appearance. The upper eyelid has inflamed and swelled a good deal, but the eye is sound, and she can see very well with it when the lid is raised. M. Gensoul has observed that the half of the tongue of the affected side is covered by a crust, of a yellow colour, in these cases, whilst the opposite side, or half, is clean. I was not able to verify this statement, owing to her inability to open the mouth or protrude the tongue. The pulse was 126 before the operation, and the same after it. She has suffered much less pain than before, has slept on the whole well, has taken sufficient nourishment, and is, I believe, doing quite as well as can be desired.

Some eighteen months ago, an old officer applied to me for assistance, in consequence of a malignant tumor which appeared at the inner canthus of the eye, and which could also be seen in the

nostril. This I removed, but the disease returned, and he decided on submitting to a second operation, at which Mr. Keate, Mr. Partridge, and Mr. Hancock, assisted, at the Ophthalmic Hospital. I this time removed the eyeball, in order to enable me to clear the orbit, which I found I could not do without. The disease, after a longer interval, returned, and he is now, I am informed, dying in the country, blind of the other eye, deaf, and evidently suffering from the disease having affected the brain. If I had removed the whole, or even the upper part, of the superior maxillary bone, I think I should have done a great deal better, and might perhaps have taken away every portion of diseased structure. The eye was removed under circumstances not commonly met with, being quite sound. Mr. Partridge was so good as to take charge of it immediately on its removal; and, on cutting it across, to ascertain the state of the retina, I found that part quite transparent, as might have been expected. Our surprise was great, however, at not being able to perceive the yellow spot of Soemmering; and it was only after the lapse of ten minutes that it became at all observable. In a few minutes more it was very distinct, of a deep yellow brown colour, and the retina became gradually opaque. The lens and the circle around were also at first quite transparent, but the retina becoming opaque soon rendered these parts less clear. In a case of similar disease, occurring near the same time, but confined to the malar bone and its orbitar plate, I succeeded in curing the patient by taking it away, and by scraping even the periosteum, and the disease has not returned: but the periosteum cannot be scraped from the bottom of the orbit, unless there is a considerable space made to enable you to do it.

KIRBY'S BRIDGEWATER TREATISE.

STRICTURES ON THE CRITICISM IN THE MEDICO-CHIRURGICAL REVIEW.

To the Editor of the Medical Gazette.

SIR,

THAT the medical profession has often been accused of irreligion is too well known to be insisted on; and although prepared to maintain that, at the present day at least, the reflection is undeserved by the body against which it is directed, I must deeply regret that any thing should occur which might tend to countenance it. In the last number of the Medico-Chirurgical Review is a critique on the first volume of Mr. Kirby's Bridgewater Treatise, containing expressions which ought never to have been admitted into the pages of such a journal, occupying, as its editor does, a station which may give rise to the belief that the sentiments and language contained in the publication which is conducted by him are those which are standard, or at least current, in the profession of which he is a member.

The natural historian may reasonably maintain that the reverend author of the treatise has too literally rendered the prophecy of Isaiah, with respect to the harmony which shall some day prevail amongst the animal kingdom; but as the mode of subsistence not only of the first created animals, but also of those preserved during the Deluge, must be to a great extent a matter of conjecture, unless we admit the special interposition of Providence, it would have been well if the reviewer had allowed the author's supposition of a providential restraint on their instincts (supported as he is by the Mosaic record of—"to every beast of the earth, &c. have I given every green herb for meat") to have passed unnoticed, rather than have illustrated his position of the unnatural tenor of the idea, by the supposed conversion of a monkey into a man, or at least to have commented on it in language less repulsive to the feelings, and in a style of reasoning more analogically correct. Subjoined are the passages contained in pages 402 and 403 of the Review:—

"The author does not inform us at what precise period God altered the organization of the carnivorous animals, and adapted them to the digestion of vegetable food. No; he merely gives us his *ipse dixit* that God 'restrained their instincts,' and ordered the incisor teeth of the tiger to grind corn and rice, till such time as sufficient numbers of kids, lambs, and other animals, were multiplied to supply the altered gusto of the leopard, the lion, and the hyena!! Has the reverend author never reflected on the inferences which might be drawn from such doctrines as those which he has here promulgated? If animals can be thus changed in their nature, so far that the tiger may browse on the flowery lawn with the kid, why need we shudder at the idea of the monkey taking a start of growth at some remote period, and shooting up into a man?" &c. &c.

Whether Mr. Kirby has always correctly interpreted the manifestations of divine goodness and wisdom, is a consideration foreign to my purpose; for the admission

that he has not done so will afford no excuse for flippant observation, or levity of reasoning, on such serious subjects. I pass over the style of the reviewer's remarks on the coral reefs (*vide* Medico-Chirurgical Review, page 406), and those on the intestinal worms of man (page 412), with a simple expression of regret at their ever having been written; and I will direct attention to his comments on the reverend author's views of divine authority over all things; from which the reviewer complains, that the "evil principle in nature is excluded," "and all moral responsibility is taken away from man." Far better would it have been if he had construed the author in the sense which he intended (bordering as it is on the abstruse doctrines of predestination and prescience)—viz. that all things are under the superintending care of Providence. Better, likewise, if the following passages had been omitted:—

"If the Almighty guides the sword, and gives 'his commission to all his scourges, against individuals as well as against nations,' that being of whom we have heard so much, and whom we hate so heartily, has been most slanderously abused and belied! The office of Satan is a complete sinecure, and in these reforming days will doubtless be abolished. This doctrine is far worse than that of blind chance and fatalism. According to Mr. Kirby, it was the hand of the Almighty that fired the 25 muskets against Louis Philippe's cortège, and slaughtered the innocent and guilty indiscriminately! Where can be the justice of future rewards and punishments, if the sword, the dagger, the fire-damp, and the poison, were guided by Almighty, and consequently irresistible power?"—Med.-Chir. Rev. p. 412.

Your readers will scarcely believe that Mr. Kirby expressly guarded his writings against such a construction, or that the reviewer could have read the page which follows the one on which he made such comments. I subjoin it here, and leave your readers to form their own opinion on the justice of his interpretation of the author's meaning, the analogies by which he illustrates his conception of it, and the language in which he expresses himself.

"By what is here said, I by no means assert the doctrine of *inevitable fate;* for then there would be no use in the employment of means of prevention. *Sir H. Davy's safety lump would not preserve the life of the miner, nor Dr. Franklin's conductor disarm the thunder-cloud;* and all the other means that, *non sine Deo,* have been invented to render harmless the action of the physical powers under certain circumstances; but I would merely assert that constant superintendence of the Deity over the world that he has created, and *who upholdeth all things by the word of his power,* which we call Providence, by which in general, as well as individually, his will has full accomplishment; and every substance or being, whether animate or inanimate, takes the station which he has assigned to it. As God willeth not that any should perish, so he withholdeth not from any the means that, if duly used and improved, will be sufficient for his salvation; and in all his dealings with mankind he hath this great and merciful object in view*."

Dr. Johnson must be held morally responsible for the contents of his journal; and his station warrants the conclusion, that the Review conducted by him must be frequently considered as a criterion whereby to judge of the modes of thought and expression which prevail amongst the medical profession. The desire of preventing the odium of irreligion being affixed to our body, has induced me thus to extend my remarks, and to select the Medical Gazette for their diffusion. The reviewer may refer to one or two qualifying paragraphs which he has written, but your readers may judge how far they redeem his language, and justify his interpretations.

May I add, that in another part of this same number scriptural expressions are unnecessarily made use of? If, however, this letter shall remove the impression which the reviewer's comments are calculated to create,—not to say the stigma which they are likely to inflict,—if it shall prevent the repetition of similar expressions, and induce their author to reconsider his remarks, and repair the aggrievance which they must have occasioned to the feelings of an honourable divine, its writer's purpose will be fully answered.—I remain, sir,

Your obedient servant,
A Stranger to Mr. Kirby.

Oct. 31, 1835.

AN APOLOGY FOR GERMAN DIPLOMAS.

To the Editor of the Medical Gazette.

Sir,

Trusting to your impartiality and desire to act justly towards your professional brethren, you will perhaps permit me to make a few brief remarks on the subject of your well-written leading article of last week. The fact which you mention, respecting the diplomas obtained by some practitioners from the German Universities, cannot be denied; at the same time,

* Treatise, p. 362 3.

the facilities for procuring them are not quite so great as, perhaps, you have been led to believe: but that is not the point to which I would beg to draw your attention. It is well known to the profession that there are some few practitioners in this town, having the title of Doctor, who possess no other diploma than one from Germany or from Scotland, obtained from the latter before the recent regulations took place; and I think that some of the individuals alluded to are men of as high professional and moral character as their professional brethren generally. They are practitioners of many years' standing, and the cause of their being driven to the alternative of purchasing a foreign diploma is quite obvious; inasmuch as in this metropolis, where we have medical schools equal, if not superior, to any in the world, no man has an opportunity of obtaining a degree in medicine; consequently, if circumstances prevented his sojourning at a university in his youth, the doors to his obtaining high professional honours are closed against him; for I need not say, that should an established practitioner leave his practice for two years, to study at a university, he may as well abandon it altogether. Let a metropolitan university, with the power of granting degrees, be established, or let the power be delegated to any other public body, and I feel confident that you would hear no more about foreign diplomas.

I have the honour to remain, sir,
Your obedient servant,
M. D.

London, Nov. 15, 1835.

FUNCTIONS OF THE NERVES.

To the Editor of the Medical Gazette.

SIR,

IN Mr. Noble's interesting communication of last week, the condition of the *olfactory* function, in Davis's case, is not alluded to. By his being kind enough to state this, he would at once render the case more complete, and oblige

A CONSTANT READER OF THE GAZETTE.

Nov. 24, 1835.

CARBONIC ACID GAS SOLIDIFIED.

THIS gas, which was some years ago reduced to a liquid form by Mr. Faraday, has been very recently rendered solid by a M. Thelorier, of Paris; and the fact vouched for by MM. Arago and Thenard, of the Academy of Sciences. It requires a temperature of nearly 100° below the freezing point of water, to solidify carbonic acid; but when reduced to this state, it is capable of remaining so, in the open air, for a few minutes. It does not expand violently, or explode, as the liquid does, but gradually disappears by a slow evaporation.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

November 26, 1835.

George Hopper, Scarborough.
John Hill Gibbs, Westbury, Wilts.
Charles Robinson, Weston, near Bath.
Thomas Foster Sagar, Leeds.
Augustus Prater.
William Thackwell, Dymock, Gloucestershire.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Nov.* 24, 1835.

Abscess	7	Hernia	1
Age and Debility	95	Hooping Cough	4
Apoplexy	17	Inflammation	38
Asthma	20	Bowels & Stomach	2
Cancer	1	Brain	2
Childbirth	5	Insanity	3
Consumption	116	Jaundice	4
Convulsions	57	Measles	16
Croup	11	Mortification	5
Dentition or Teething	7	Paralysis	3
Dropsy	19	Small-Pox	29
Dropsy on the Brain	17	Venereal	1
Dropsy on the Chest	4	Worms	1
Epilepsy	2	Unknown Causes	1
Fever	12		
Fever, Scarlet	9	Stillborn	10
Heart, diseased	2		

Increase of Burials, as compared with the preceding week . . . 201

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Nov. 1835.	THERMOMETER.		BAROMETER.	
Thursday . 19	from 33	to 49	29 87	to 29·95
Friday . . 20	32	51	29·95	29·90
Saturday . 21	46	55	29·85	29 84
Sunday . . 22	47	55	29 79	29·73
Monday . . 23	48	54	29·73	29·83
Tuesday . . 34	38	54	29 79	29·80
Wednesday . 5	45	55	29·78	29·72

Prevailing winds, S.W. and S.

Except the 19th, generally cloudy; with frequent showers of rain.

Rain fallen, ·275 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

With respect to the FARNHAM INQUEST, we are sorry we cannot respond to the appeal made to us. We published no report of the case; and the matter in question, we fear, would be as unintelligible, as it would certainly be uninteresting, to our readers.

The papers of Mr. Adams, Mr. Anderson, Mr. Armstrong, Mr. Bishop, Mr. Chatto, Mr. Davey, Mr. Alexander Shaw, Mr. Labatt, Dr. Clendinning, Dr. Farre, Mr. Niaarp, &c. have been received.

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SATURDAY, DECEMBER 5, 1835.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE X.

PACHYDERMATA.

WE have now arrived at the seventh order of mammals in the Cuvierian arrangement (for we obtain no remedial means from the sixth), which has been called *Pachydermata* (from παχυς, thick, and δεϱμα, skin), on account of the thickness of their hides. They are distinguished from all the preceding animals by the absence of claws or nails, and by the presence of one or more hoofs. They are totally incapable of grasping or seizing bodies by their extremities, which are only destined for the support of the body. The presence of hoofs, however, is not a character peculiar to them, being common also to that of ruminants. But the pachydermata may be readily distinguished from the latter order, by the greater simplicity of their stomachs, and the total absence of the power of ruminating or chewing the cud.

Cuvier makes three families of this order—namely,

Family the 1st, the *Pachydermata proboscidiana*, or those bearing a proboscis—as the elephant.

Family 2d, the *Pachydermata ordinaria*, or those devoid of a proboscis, but which have two, three, or four toes on each foot—as the hog.

Family 3d, the *Solipedes*, or *Solidungula*; those bearing no proboscis, and which have only one apparent toe and a single hoof to each foot, although, under the skin on each side of their metatarsus and metacarpus, there are spurs, representing two lateral toes. The horse and the ass are examples.

The two first of these families constitute the order *multungula* (or many-hoofed) of Illiger, Goldfuss, and others.

The second of these three families is the only one yielding any medicinal article, and that not an important one. I allude to the well-known hog's lard, which, being officinal, it is my duty to examine.

Sus Scrofa.

History.—The hog is an animal very anciently known. By the Levitical law, the Jews were forbidden to eat its flesh; some say on account of the filthy character of the animal, others because it was supposed to produce cutaneous and other diseases (more especially the leprosy), with which the Jews were much affected at the time they left Egypt. It is, perhaps, impossible now to ascertain to which of these two circumstances we ought to refer this prohibition, though the balance of evidence is considerably in favour of the first. The Mohammedans are also interdicted from eating it.

Etymology.—The generic name of the animal is from the Greek, συς: its specific name, *scrofa*, probably arises from *scrobs*, a ditch or slough, and refers to its dirty habits, " quod gaudeat *scrobes* facere."

Description.—The animal is so well known as to require but little description.

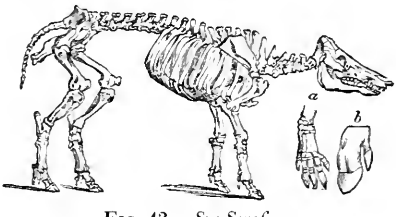

FIG. 43.—*Sus Scrofa.*

The teeth are from 42 to 44 in number—namely, 10 or 12 incisors; 4 triangular canines, long, and bent upwards and inwards; and 28 molars, the posterior ones having tuberculated crowns, while the anterior ones are more or less compressed. There are usually four (though sometimes five, fig. 43, *a*) toes, which are hoofed, the two middle ones only touching the ground. The snout is truncated, elongated, and cartilaginous; the body is covered with bristles; the colour is various.

Varieties.—There are an immense number of varieties of this animal; which, however, are most conveniently reduced to the three following:—

Variety 1.—*Sus scrofa ferus;* the *wild hog*, or *wild boar*.

Variety 2.—*Sus scrofa domestica*, the *domestic hog;* which again varies in regard to form and colour: compare, in this respect, the Chinese hog (fig. 44) with the Suffolk (fig. 45) and Herefordshire (fig. 46) breeds.

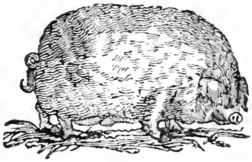

Fig. 44.

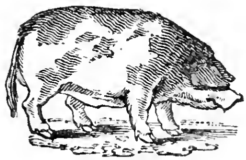

Fig. 45.

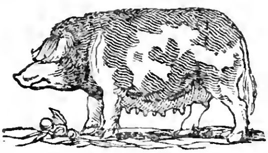

Fig. 46.

Variety 3.—*Sus scrofa pedibus monungulis*, that is, having solid or undivided hoofs; a variety noticed by Aristotle, Pliny, Linnæus, and others (fig. 43, *b*).

Geography.—It is a native of the temperate parts of Europe and Asia; of the northern parts of Africa, of America, of the Islands of the South Sea, &c. It was formerly a native of this country, and we are told that the forests on the north side of London were retreats for them.

Officinal parts.—As in other animals (man and the ox, for example), the fat about the loins has greater firmness and density than that of other parts, and is, therefore, preferred for medicinal use, and for domestic purposes, under the name of hog's lard (*adeps suillus; adeps porci*), or axunge (*axungia*) from the use anciently made of it—namely, greasing the axle of a wheel (*unguendi axem*).

Preparation.—In order to separate the lard from the membranous parts in which it is contained, the fat is melted over a slow fire, and then strained (through flannel or linen); and, in order to keep it, it is poured, in the melted state, into bladders. Sometimes salt is added, but this is highly objectionable for medical purposes. We may, however, readily remove it by melting in boiling water. When cooling, lard should be kept stirred, in order to prevent the separation of the stearine and elaine.

Properties.—At ordinary temperatures, it is a white or yellowish-white solid. Its melting point varies from 78° 5′ to 87° 5′. In the liquid form it should be perfectly clear and transparent, though when intermixed with water, it acquires a whitish or milky appearance. It should have very little or no taste and odour.

Composition.—Its ultimate constituents are carbon, hydrogen, and oxygen, with perhaps a minute portion of nitrogen.

	Chevreul.	Saussure.	Berard.
Carbon ..	79·098	78·843	60·5
Hydrogen	11·146	12·182	15·4
Oxygen ..	9·756	8·502	24·1
Nitrogen	0·0	0·473	0·0

But lard, like most other fatty bodies, is made up of three substances (called its proximate principles)—namely, one called *stearine* (from στεαρ, tallow, or suet), because it is solid at ordinary temperatures; a second termed *margarine* (from *margarita*, a pearl); and the third denominated *elaine*, or *olein* (from ελαιον, or *oleum*, oil), because it is liquid at common temperatures. Each of these requires to be noticed.

1. *Stearine.* — Here is some stearine, which I have obtained thus:—Lard, folded in blotting-paper and laid between two boards, was compressed in a vice, or press; by which means a considerable portion of the elaine was got rid of. The residue was dissolved in boiling alcohol; as the liquid cooled, the stearine deposited in a crystalline form. Or you may procure it by melting lard in sulphuric æther: when cold, compress the insoluble part (the stearine) first in a cloth, and then between folds of blotting-paper.

Stearine has the following properties: it is a white, easily pulverised solid; fusible at about 129°; soluble in boiling

alcohol, but separating by cooling; very soluble in boiling æther, but cold æther retains only $\frac{1}{225}$ of its weight in solution; saponifiable by alkalies, being converted by the assistance of water into stearic acid and glycerine (so called from γλυκυς, sweet, on account of its flavor). The following table shows the changes it undergoes in this process:—

Results of Saponification.

	Carbon.	Hydrogen.	Oxygen.		Carbon.	Hydrogen.	Oxygen.
1 Stearine	73	70	7	1 Anhydrous Stearic Acid	70	67	5
1 Water..	0	1	1	1 Glycerine	3	4	3
	73	71	8		73	71	8

All the figures refer to the number of atoms.

2. *Margarine* is solid at common temperatures; it fuses at 117° 5′ Far. Its solubility in alcohol is similar to that of stearine, but it is more soluble in cold æther; and hence it is obtained in the preparation of stearine, by allowing the ætherial solution to evaporate spontaneously after the stearine has been deposited. It saponifies by an alkali, producing glycerine and an acid. Query, is it margaric or stearic acid? Dr. Turner says it is stearic acid.

3. *Oleine*, or *elaine*, is distinguished from the stearine and margarine by liquidity at ordinary temperatures; its congealing point being, according to Chevreul, 44°. It is very soluble in cold æther and cold alcohol. Its specific gravity is 0·915. It is saponified by alkalies, probably producing glycerine and oleic acid, and perhaps also margaric acid.

	Stearine.	Oleine.	Stearic acid.	Margaric acid.	Oleic acid.
Carbon ..	78 02, or 73 atoms = 438	79·030	80·145, or 70 = 420	79·053, or 70 = 420	80·972, or 70 = 420
Hydrogen	12·2, or 70 atoms = 70	11·422	12.478, or 67 = 67	12·01, or 65 = 65	11·359, or 58 = 58
Oxygen..	9·78, or 7 atoms = 56	9·548	7·377, or 5 = 40	8·937, or 6 = 48	7·699, or 5 = 40
	100 564	100·000	100·00 527	100 533	518

The relative quantities of stearine, margarine, and oleine, in lard, have not been accurately ascertained. Braconnot obtained,

Stearine (but it was mixed with margarine)	38
Oleine	62
Lard	100

Now Dumas says that stearine constitutes about 25 per cent of lard; hence probably the relative proportions will be about the following:—

Stearine	25
Margarine	13
Oleine	62
Lard	100

Rancidity of Lard.—It is well known that when lard is exposed to the air for some time, it acquires an unpleasant odour, and is then said to be *rancid*. What is the nature and cause of this change? The first fact easily ascertained, is that rancid lard has become acid, since it reddens the tincture of litmus. This rancid condition is brought about by the agency of the air, part of the oxygen of which is absorbed, while a little carbonic acid is evolved. As stearine does not become rancid in the air, while elaine does, the rancidity of lard is referred to the latter. However, it has been found that the purer the elaine, the less easily does this process take place; so that it has been suspected there is some foreign substance in the elaine, which is the cause of the rancidity, either by undergoing decomposition or by acting on the elaine. Chevereul, whose elaborate experiments on fatty substances during ten years are so well known, analysed some rancid lard, and found in it the following substances:—

1. Unchanged stearine and elaine.
2. A volatile non-acid matter, having a rancid odour.
3. An acid analogous to caproic acid (an acid so called from *Capra*, a goat, because it is found in goat's milk).
4. Another volatile acid.

5. Oleic, margaric, and perhaps stearic acids.
6. An apple-yellow colouring matter.
7. A non-acid, non-volatile matter, soluble in water.

Physiological effects. — Like other fatty substances, the local action of lard is emollient, that is, it relaxes the tissues of parts. This effect has been by some compared to the power which all emollients possess of diminishing friction between the particles of inorganic bodies,—a comparison, however, which only holds good when applied to inorganized parts, as the nails, &c. By its sheathing properties it becomes a good demulcent. Its remote action is that of a nutrient.

The power of assimilation of fatty bodies varies considerably in different individuals, but, in most, a large quantity of fat cannot be easily digested. This is proved by the experience of persons whose digestive powers are weak, and by the experiments of Mr. Beaumont, an American surgeon, on a Canadian, who, in consequence of a gun-shot wound, had a permanent aperture in his stomach. From the experiments and observations on this man, one of the inferences drawn by Mr. Beaumont is, that "oily food is difficult of digestion, though it contains a large proportion of the nutrient principles." When digested, it appears, however, to be powerfully nutritive; and when mixed with other nutrient substances, its assimilation is promoted: and Dr. Cullen states the flesh of quadrupeds is nutritious in proportion to the quantity of oily matter it contains. Barbier says, that persons who take habitually much animal fat, are of a soft complexion, have lax muscular organs, and are subject to hernia and incontinence of urine. He assigns, also, as a reason, that some of the oleaginous particles, escaping the digestive process, get into the circulation, and thus exercise a local emollient action on the different tissues. But the whole account is, I believe, more imaginary than real. My observation does not bear out this statement, even in natives of this country. I have been acquainted with several persons who, although very fond of, and consuming large quantities of fatty substances, yet were not of a relaxed habit; and we all know that a great part of the animal food of a considerable portion of our hard-working country labourers is fat bacon; yet we see no symptoms in them of lax muscular organs. Most undoubtedly Barbier's assertion will not apply to the natives of the Arctic regions, who live in a great measure on fat and oil. It is certainly true that all persons do not possess the power of digesting much fat, but those that do I have for the most part found to be individuals of a strong constitution; and I am disposed to think, with Dr. Cullen, that when it can be digested, the more fatty the diet, the more nutritious. Sir John Ross considers that the natives of cold countries seem to require a more fatty diet than the inhabitants of tropical regions, in order to promote the production of animal heat. "All experience," says he, "has shown that the large use of oil and fat meats is the true secret of life in these frozen countries, and that the natives cannot subsist without it, becoming diseased and dying under a more meagre diet." And he goes on to say, "I have little doubt, indeed, that many of the unhappy men who have perished from wintering in these climates, and whose histories are well known, might have been saved if they had been aware of these facts, and had conformed, as is so generally prudent, to the usages and experience of the natives." I am acquainted with one instance that strikingly confirms Sir John Ross's views, namely, that more heat is generated in those who consume much fat. It is the case of a person exceedingly fond of fat, and who, although in the enjoyment of good health, has a remarkably hot skin, almost that of fever.

You may ask, in what respect does hog's-lard differ from other fatty bodies in its effects? It is a very old notion, that both the flesh and fat of the hog dispose to cutaneous diseases; but it is difficult either to prove or disprove this statement. As, however, it is undeniable that in some instances particular kinds of food do cause eruptions, thereby proving a close relation between the digestive and cutaneous system, I would advise you so far to conform to this notion, as to prohibit a diet of pork in all chronic diseases of the skin, by which you will be adopting the most prudent course.

The particles of fatty bodies are not easily absorbed, probably because they are not readily miscible with the blood, but the occasional detection of them in the secretions proves that they must have existed in that fluid.

Use.—In medicine we employ lard principally as a basis for various unguents; for example, mercurial ointment. It has been used as an emollient by friction; but this practice is rare. When employed instead of spermaceti ointment to dress blisters, I have frequently seen it excite considerable irritation.

RUMINANTIA.

We now come to the eighth order of Mammals, called *Ruminantia*, from the power which these animals possess of returning the food which they have already

swallowed into their mouth, to be remasticated at their leisure or while reposing. They are characterized by having incisor teeth (usually eight, but sometimes only six in number) in the lower jaw only; the two metatarsal or metacarpal bones are consolidated into one, called the *canon bone;* the feet are terminated by two fingers and two hoofs, which seem, by their flattened face, to have the appearance of one cloven hoof; and hence these animals have been called *cloven-footed*, or the *Bisulca*. But that which more particularly distinguishes them from all other animals, is the number and structure of their stomachs, and their faculty of ruminating. To these, therefore, I must in the first place direct your attention.

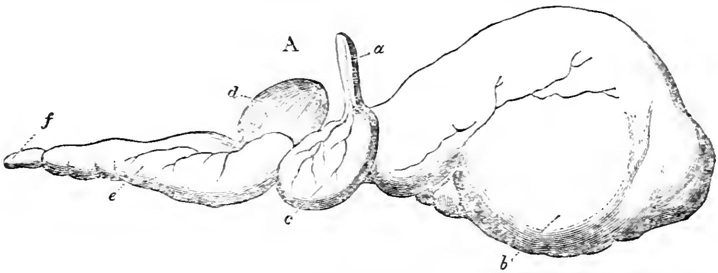

FIG. 47.

a, The gullet.
b, The paunch.
c, The honeycomb.
d, The manyplies.
e, The reed.

There are four stomachs—the paunch, the honeycomb, the manyplies, and the reed.

1. The first stomach, called *paunch, venter, rumen*, or *ingluvies*, receives the food

FIG. 48.

from the gullet after it has been roughly broken by the first mastication, and transmits it to the second stomach. It is often divided into pouches by transverse contractions, and is lined by a thick membrane, beset with numerous papillæ. (Fig. 48.) In this stomach the food undergoes little change, and it seems only to be kept here until the second is fit for its reception.

2. The second stomach, called *honeycomb, bonnet, king's hood*, or *reticulum*, receives the food from the paunch, rolls it up into balls, and projects it into the œsophagus, from whence it is transmitted into the mouth by the second mastication. The internal lining of this stomach is thrown into a number of folds, forming the sides of polygonal cells, (fig. 49) like

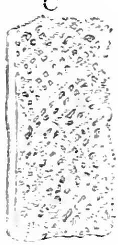

FIG. 49.

those of the honeycomb; and hence one of its names.

3. The third stomach, called the *manyplies, tripe, feck*, or *omasum*, receives the

FIG. 50.

food after it has been properly masticated. It also receives liquids direct from the œsophagus. This coat is remarkable for the number of longitudinal folds of its internal coat, and from which one of its names (manyplies) has been taken. (Fig. 50.) These folds have been compared to the leaves of a book; and hence the French call this stomach *Feuillet*.

4. The fourth stomach, called the *reed*, or *red*, or *abomasum*, receives the food from the manyplies. Here the gastric juice is secreted, and, therefore, here the process of digestion goes on. Its internal surface is irregularly folded. (Fig. 51.) An infusion of this stomach is used, under the name of rennet, to coagulate milk.

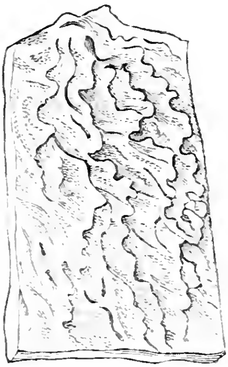

FIG. 51.

As the œsophagus is connected with three of these stomachs, you may ask how is it that the food passes into the paunch after the first mastication, but into the manyplies after the second? Some have supposed that it was effected by the voluntary act of the animal; but Flourens has explained it without reference to volition. He thinks it is the condition of the food, and the anatomical structure of the parts, which determines its passage into the first or into the third stomach.

The œsophagus opens at first almost equally into the paunch and honeycomb, and is continued afterwards, in the form of a gutter or semi-canal, into the upper part of the manyplies. Drinks and finely-divided semifluid food (as that which results from the second mastication) pass down the œsophagus and along this semi-canal into the manyplies, from thence to the reed. But when food is swallowed in large masses (as after the first mastication) it separates mechanically the edges of the semi-canal, and thus falls into the paunch.

Division of Ruminants.

We may arrange them thus:

1. *Ruminants without horns*, and having canine teeth in both jaws. This division includes two genera—*Camelus* and *Moschus*.

2. *Ruminants with horns* in the male animal. Of these we make subdivisions, thus:

(*a*). The horns caducous annually, branching: no canine teeth. Here we have the genus *Cervus*.

(*b*). The horns conical prominences, persistent, and covered with a velvety skin: the *Giraffe*.

(*c*). The horns hollow, persistent, elastic, growing by layers in osseous prominences: as the *Bos* or Ox.

For convenience, I shall commence with the ruminants with horns.

Cervus Elaphus.

History and etymology.—The genus Cervus has received its name, probably, from κερας, *horn*. It contains several species, which may be thus arranged:—

Cervus, or Deer	Horns entirely or partially flat ..	C. Alces, or Elk. C. Tarandus, or Rein Deer. C. Dama, or Fallow Deer.
	Horns rounded ..	C. Elaphus, or Red Deer. C. Canadensis. C. Virginianus. C. Axis.
	Horns small	C. Capreolus, or Roebuck. C. Pygargus. C. Muntjac. C. Porcinus. C. Mexicanus.

a, Cervus Elaphus. *b*, C. Capreolus. *c*, C. Dama.
FIG. 52.

The officinal animal, the C. Elaphus, has been known for a very long period. It is noticed by Hippocrates, Aristotle, Galen, Avicenna, and Pliny.

Description.—This animal has usually 34, though sometimes only 32 teeth; namely, 8 incisors in the lower jaw, two canines in the upper, but which are sometimes absent, and 24 molars. In the summer its colour is reddish-brown; in the winter greyish. The rump and tail are always of a pale yellow. The horns of the male animal (called the Stag) are, in fact, osseous prominences of the frontal bone, and are called *antlers*. They are long and much branched. These are the officinal parts, and I must therefore give you some account of their annual productions and shedding.

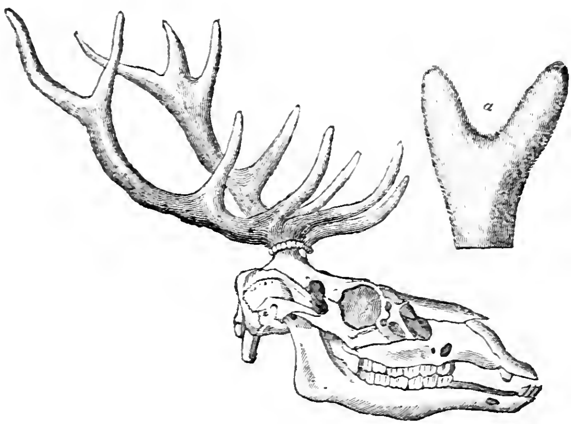

FIG. 53.

Growth and fall of the Antlers.—The stag usually begins to shed his antlers in February or March, immediately after which their reproduction begins, and by July he has completely renewed them. The first sensible phenomenon of the formation of these parts is the vascular excitement about the frontal bone. The arteries are observed to be enlarged, and to pulsate more strongly than usual; the heat is increased, and, in fact, all the symptoms of active inflammation come on. Very soon we perceive two cartilaginous tubercles, one on each side; these enlarge and elevate the skin, by which they acquire, from the distention of the latter, a velvety covering fig. 53, *a*) These tubercles are soon converted into real bone; but the deposit of ossific matter does not stop here; it continues around the base of the antlers, thus giving rise to what has usually been termed the *burr*. Now, consider for a moment the probable effect of this. These osseous prominences, the antlers,

are supplied with two sets of vessels—an external or cutaneous, which is the most efficient, and an internal. By the pressure made on the former by the burr, they are obliterated: the covering of the antlers no longer receiving a supply of blood, soon ceases to live, dries up, and falls off. The internal vessels continue to keep up the life of the bone for a few months longer, when death takes place. This occurrence may be in part owing to the imperfect nutrition, and partly, perhaps, to the exposure of the bone to the air without any envelope; but it arises principally from some unknown changes in the vital actions. The antlers being now dead, nature soon sets about their separation. To effect this, the living parts at the base are rapidly absorbed, so that the antlers, being left but very slightly adherent to the frontal bone, readily fall off by a gentle knock. A few hours only elapse before the irregularity on the surface of the os frontis is covered by a thin pellicle, and shortly afterwards the formation of a fresh pair of antlers is commenced.

Chemical properties.—*True horns*, such as those of the ox and sheep, consist principally of a substance analogous to *coagulated albumen* or *modified fibrine*; whereas the antlers of the stag (called *hartshorn*) have the same composition as bone; that is, they consist of an organized cartilage composed of *gelatine*, and, therefore, soluble in boiling water (more so than the cartilage of bones) and of *phosphate of lime*. The following are the proportions given by Merat Guillot:—

Soluble cartilage	270
Phosphate of lime	575
Carbonate of lime	10
Water (and loss)	145
	1000

The presence of the *gelatinous matter* is readily shown by boiling hartshorn shavings in water; and adding the infusion of galls. You observe there a copious precipitate (tannate of gelatine.) The *phosphate of lime* has the same composition as that found in bones, and therefore consists of—

3 Phosphoric acid	(72×3)	$= 216$
8 Lime	(28×8)	$= 224$
		440

so that it is, in fact, a subsalt. We readily obtain it by burning the antlers in a crucible, and thereby destroying all the organised or cartilaginous matter. But the *cornu ustum*, or *burnt hartshorn* of the shops, is usually bone ashes, the two being identical in composition. I have digested some hartshorn shavings in muriatic acid, and have thereby dissolved the phosphate of lime; you observe that on the addition of ammonia to the liquid, a white precipitate (of phosphate of lime) is thrown down.

Medical uses.—The decoction of hartshorn, like isinglass, is nutritive and emollient, and may be employed in intestinal or pulmonary irritation. Decoction of hartshorn is used by brewers, &c. for the purpose of fining. It is preferred to isinglass on account of its cheapness. The gelatinous matter of bones being less soluble than that of the antlers, shavings of bone will not answer so well as those of hartshorn.

The agency of the gelatine in this process will be readily comprehended. Some of the constituents of the liquor to be fined (the colouring matter, for example) coagulate the gelatine, which, in precipitating, envelopes within its parts the matters rendering the liquor turbid.

Hartshorn shavings are employed in the manufacture of antimonial powder.

Burnt hartshorn (that is, phosphate of lime) has been exhibited in those affections of the bones in which there is a deficiency of earthy matter, on the false notion that there was a want of this substance in the system; but the truth is, the deficiency of phosphate of lime in bones arises from an altered action of the vessels which ought to deposit this ossific matter, and not from a deficiency of material.

The liquor called *spirit of hartshorn* is in fact a solution of carbonate of ammonia in water, with a little empyreumatic oil. It is generally obtained by the distillation of bone, and hence it is called in commerce *bone spirit*; it will be described hereafter. The London College have superseded it by a solution of carbonate of ammonia.

I have now finished that family of ruminants having osseous and caducous horns, and proceed to notice another in which the horns are hollow, persistent, and elastic. First of the

Ovis Aries.

History.—The sheep is one of the anciently known animals, and is mentioned by Moses, Aristotle, and other old writers. The immense number of races of this animal in cultivation are well known; and it is now perhaps impossible to determine its native condition. Modern zoologists, however, refer our domesticated sheep either to the *ovis ammon*, called the *argali* of Siberia (fig. 54), or the *ovis musimon*, the *moufton* or *mufon* of Sardinia (fig. 55.)

FIG. 54.—*Ovis Ammon, or Argali.*

FIG. 55.—*Ovis Musimon.*

Description.—This animal has 32 teeth—namely, 8 incisors, and 24 molars. It is probable that in the natural state it always has horns; but a considerable number of the English breeds are hornless. When present they are hollow, thick, angular, wrinkled transversely, and curved spirally backwards and downwards, and are sometimes found in both sexes.

Varieties.—It is quite unnecessary for me to give any account of the almost innumerable races of sheep. You will find a good description, with figures of the British breeds, in Loudon's Encyclopædia of Agriculture. Here are representations of one or two varieties.

FIG. 56.—*Black-faced Sheep.*

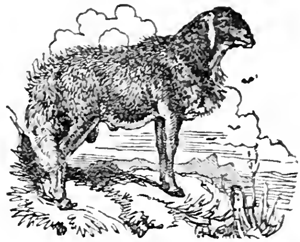

FIG. 57.—*Fat-rumped Sheep of Tartary.*

Officinal part.—The only officinal part of the animal is the fat about the kidneys and loins—the well-known mutton suet. It is prepared in the same manner as hog's lard.

Properties.—In many of its physical properties it agrees with lard. It has, however, a greater consistence, and fuses at about 98° or 100°.

Composition.—Its ultimate constituents are carbon, hydrogen, and oxygen.

	Chevreul.	Berard.
Carbon	78·996	65·0
Hydrogen	11·700	21·5
Oxygen	9·304	13·5
	100·000	100·0

It is composed of four proximate principles—namely stearine, margarine, oleine, and hircine. The three first have already been described under the head of lard.

Hircine is a fatty body, which has received its name from *hircus*, a male goat, because it has been found in the fat of this animal. Its properties, however, are very imperfectly known. It forms with oleine the liquid part of mutton suet. It is distinguished from oleine by its greater solubility in alcohol, by its goat-like odour, and by saponification yielding a volatile fatty acid, of an oily appearance, which, on account of its odour, has received the name of *hircic* acid. This acid forms with bases, salts called *hirciates.*

The relative quantities of stearine, margarine, and hircine, have not been ascertained.

The *effects* and *uses* of mutton suet are analogous to those of lard. In some cases it is preferred, on account of its greater consistence, as in the manufacture of certain plaisters and unguents. It is a constituent of the emplastrum cantharidis of the London Pharmacopœia, and of the unguentum sambuci of the Dublin.

I now proceed to notice the

Bos Taurus.

History and description.—Here is another animal very anciently known, and now so familiarized amongst us, as to require little or no description. It has no upper incisor nor canine teeth; the limbs are large and unwieldy; the forehead is longer than it is broad, and the horns are placed at the extremities of a prominent line, which divides the forehead from the occiput.

Geography.—It is found wild in Poland, the Carpathian Mountains, Lithuania, &c.

Parts used in medicine.—Although no part of the animal is officinal, yet I cannot pass it by without noticing one or two of its products useful to us in a therapeutical and dietetical point of view.

The milk is the first of these which I think it necessary to examine: it is secreted by two mammary glands which lie close together, and constitute the *udder.* Each gland consists of a number of *lobes*, made up of *yellowish* or *reddish soft granules.* These granules consist of very fine blood-vessels, nerves, and the commencement of the *milk* or *lactiferous ducts* (*ductus galactophori*, so called from γαλα, *milk*, and φέρω, *to bear*), which unite so as to form eight or ten principal ducts, which open into the large *duct* or *tube of the teat.* This tube is conical, and has a number of folds on its internal surface.

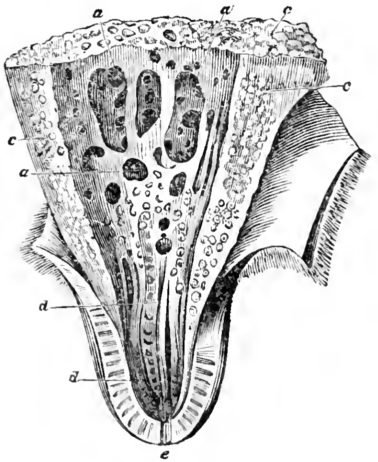

Fig. 58.

Many of the physical properties of milk are so well known, that it would be a waste of time to recapitulate them. Some of them, however, deserve notice. If you submit milk to examination by the microscope, you will observe that it consists of a number of *globular particles*, which float in a serous liquid. Raspail says they appear strongly coloured, and black on the edges, on account of their minuteness. They are not more than half the size of the globules of human blood; their diameters, therefore, will be about one-ten-thousandth of an inch. As I shall afterwards show, they are composed of a *fatty matter* (the well-known *butter*), and a coagulable substance of the nature of albumen, but which differs in some properties from the albumen of the egg, or of blood, and has been called *caseum*, or *caseous matter*, because it constitutes the basis of cheese.

These globules being specifically lighter than the liquor in which they are suspended, easily separate by standing. They therefore rise to the surface, carrying with them some caseum, and retaining some of the serum; thus forming what is called *cream.*

By agitation, as in the process called *churning*, the fatty globules unite into one mass (*butter*), leaving what is called *butter milk*, composed of caseum and serum.

The *skim milk* contains a considerable quantity of coagulable or *caseous matter;* but it is not quite clear that this is identical in its nature with that found in the cream. If an acid be added to skim milk, a white clot (called *curd*) is immediately formed by the coagulation of this caseous matter, while a thin liquid is left behind, called *whey.* To form this curd it is usual to employ the liquid called *rennet*, which is an infusion of the fourth stomach (*abomasum*) of the calf. Tiedemann and Gmelin have found in this stomach acetic and butyric acids, traces of acetate of ammonia, albumen, some other animal matter, besides acetate, phosphate, and muriate of soda and potash, and phosphate of lime. After all the curd has been formed by rennet, a fresh quantity of coagulum may be obtained by the addition of acetic acid. The substance which is thus coagulated has been by Schübler supposed to hold an intermediate place between caseum and albumen; and it has been called by the Germans *zieger*, by the French *serai.* Berzelius, however, thinks it is merely a combination of the uncoagulated caseum with acetic acid.

The *whey* which is left by the separation of the curd and serai yields on evaporation *sugar of milk*, one or more *azotized substances*, the nature of which is not well known (*osmazome ?*), *lactic acid*, and some *salts.*

The following table shews the composition of milk, and of some of the products used in domestic economy (as cream, &c.): —

- MILK
  - Cream ....
    - Butter ....
      - Fatty Matter solid at ordinary temperatures .. *Stearine.*
      - Liquid Fatty Matter....
        - By saponification yielding Butyric, Caproic, Capric, Margaric, and Oleic Acids, and Glycerine .... *Butyrine.*
        - By saponification yielding no Butyric, Caproic, nor Capric Acids .... *Oleine.*
    - Butter-milk ....
      - Caseum.
      - Serum.
  - Skim-milk
    - Matters which are coagulable
      - by Rennet .... *Caseum*, or *Curd.*
      - not by Rennet, but by Acetic Acid.................... *Zieger*, or *Serai.*
    - Serum, or Whey ....
      - .... *Sugar of Milk.*
      - Azotised Matter .... *Osmazome?*
      - .... *Lactic Acid.*
      - Salts ..
        - Soluble in Alcohol
          - *Lactates* of Potash (principally), Soda, Ammonia, Lime, and Magnesia.
          - *Chlorides* of Potassium and Sodium.
        - Soluble in Water, not in Alcohol
          - *Sulphate* of Potash.
          - *Phosphate* of Potash and Soda.
        - Insoluble in Water
          - *Phosphates* of Lime, Magnesia and Iron.

In this table I have taken no notice of the existence of a little butter in butter-milk and skim-milk.

We cannot help being struck with the remarkable analogy which exists between milk and an emulsion (as that made with almonds.) Both have a whitish appearance and a sweetish taste; both, on examination by the microscope, are found to contain an immense number of oily globules, held in suspension by an albuminous matter and sugar; and both are intended for the nourishment of young living beings.

Of the constituents of milk there are only about three which require a separate notice. I shall first examine *caseum*, or the *coagulable matter of milk*, including under this head *zieger*, or *serai*, which I cannot regard as being essentially different from caseum. Caseum must, I think, be considered as agreeing in many of its properties with liquid albumen, from which, however, it is distinguished by its non-coagulability by the agency of heat, and by the products of its spontaneous decomposition. It is, however, easily coagulated by acids, as I have already mentioned. In all probability the coagulated caseum is a compound of the acid and caseum.

In the manufacture of cheese (of which caseum is the base) rennet is commonly employed to coagulate the caseous matter; but I understand that in the manufacture of Dutch cheese, muriatic acid is employed; to which, we are told, its pungency and freedom from mites are to be referred. Various salts also coagulate caseum: I shall only shew you examples of a few of these—namely, perchloride of tin, sulphate of copper, nitrate of silver, and perchloride of quicksilver (corrosive sublimate). [The experiments were shown.] What may be the exact nature of the change in these cases I will not pretend to offer an opinion, further than that in all probability the caseum combines both with the acid and the basic constituents of the salt employed. Putting theory aside, these facts and experiments are of considerable practical importance. The salts employed lose, by their combination with, or alteration by, the caseum, a great part of their activity; so that milk (as containing caseum) becomes a valuable agent to us in the treatment of poisoning.

I have already mentioned that the products of the spontaneous decomposition of caseum distinguish it from albumen; but they connect it with the gluten of vegetables, which undergoes a series of analogous changes by exposure to the air in the moist state, constituting a remarkable kind of fermentation. Braconnot, who is the latest experimenter on this subject, says that *curd* (obtained from spontaneously coagulated skim-milk), covered with water, completely putrified in the space of a month; and that he obtained two classes of results: the one *soluble* in water, having an unpleasant odour, and containing *free acetic acid*, much *acetate of ammonia*, a little *acetate* and *muriate of pot-*

ash, *ammoniacal phosphate of soda*, a yellow acrid *oil* (the cause of the pungent taste of old cheese), resin, *caseous oxide* (called by Braconnot *aposépédine*, from απο, *from*, and σηπεδων, *putrefaction*, because it is a product of putrefaction), *two substances* analogous to osmazome, and *phosphate of lime*. The other class of results obtained by Braconnot was *insoluble* in water, and contained a *brown animal matter*, *margarate of lime* (the base of which came from the lime in the caseum), and *margaric* and *oleic acids*.

Butter is another constituent of milk deserving a short examination. Its physical characters are known to all of you. It is composed of three fatty bodies, one a solid (stearine), and two liquids—viz. oleine and *butyrine*; the characteristic distinction of the latter being, that by saponification it yields butyric, caproic, and capric acids, all of which are volatile and odoriferous, and therefore readily distinguished from stearic, margaric, or oleic acids, which are fixed. If to a solution of a soap made with potash and butter we add tartaric acid, and distil, the volatile acids pass over; the fixed ones remain behind. This distilled liquid, therefore, will contain butyric, caproic, and capric acids.

Sugar of milk is obtained by the evaporation of whey. That which is met with in commerce is made in Switzerland, from the whey left behind in the manufacture of cheese. The specimens which are before you were received from Paris. They occur in cylindrical masses, in the middle of which you observe the stick or thread which serves as a nucleus for the crystals. This kind of sugar differs from common sugar in several peculiarities, the most marked of which are the following:—It is incapable of undergoing the vinous fermentation (a vinous liquor called *koumiss* is, however, made from mare's milk by the Tartars)—is very slightly soluble in alcohol—is less soluble in water than common sugar, and by the action of nitric acid yields, amongst other products, saccholactic or mucic acid; so that this kind of sugar forms, as it were, a connecting link between common sugar and gum; and, like starch, it is converted into common sugar by boiling in very dilute sulphuric acid. Berzelius states its composition to be—

5 Carbon	30
4 Hydrogen	4
4 Oxygen	24
Atomic weight,	58

If this statement be correct, its composition differs from common sugar.

Lactic acid is a substance highly interesting in a chemical point of view, but is of secondary importance in reference to pharmacology. Gmelin contends that the so called lactic acid is merely a compound of acetic acid and animal matter; but the opinion seems disproved by the elaborate experiments of Berzelius. The *nanceic acid* of Braconnot, or the *zumic acid* of Thomson, is merely a mixture of lactic and acetic acids.

Physiological effects of milk.—I need hardly tell you that milk is a powerfully nutritive substance, since we all know it is the principal or only aliment of mammals during the first periods of their existence. The essentially nutritive parts are the butter, the caseum, and the sugar. But we must not pass over the phosphate of lime and lactic acid. The bones of young animals are imperfectly formed; they consist principally of cartilage, and contain a less quantity of phosphate of lime than is found in the bones of an adult. It is evident, therefore, that at this period of life phosphate of lime, or its constituents, is an essential substance for the perfect development of these beings. Nature, ever provident, and careful not to give wants without accompanying them with the means of satisfaction, has placed phosphate of lime in the milk. But it will be immediately said, how is it held in solution? In all probability by lactic acid; for this substance has been found to dissolve it readily out of the body, and therefore we see one use, at least, which this peculiar acid has in the milk.

The quantity of nutritive matter in milk varies, not only with the species of animal from which the milk has been obtained, but even in the same species—nay, in the same individual under different circumstances.

The following table has been drawn up from the best analyses of milk which have been published:—

100 Parts of the Milk of the	Cream.	Butter, or Fatty Matter.	Caseum.	Sugar of Milk.	Total Butter, Caseum, and Sugar of Milk.
Ewe (Stiprian Luiscius, and Bondt)	11·5	5·8	15·3	4·2	25·3
Goat (ditto)	7 5	4·56	9·12	4·38	18·06
—— (Payen)	(not stated)	4·08	4·52 including insoluble salts.	5·86 including soluble salts.	14·46
Cow (Stiprian Luiscius, and Bondt)	4·6	2 68	8 95	3·60	15·23
—— (Berzelius)	(not stated)	4·5	6·10	3·5	14·10
Woman (Payen) 1st case	(not stated)	5·16	0·180	7·62	12·96
Woman (Payen) 2d case	(not stated)	5·2	0·250	7·930	13 38
Woman (Payen) 3d case	(not stated)	5·18	0 240	7 86	13·28
Ass (Stiprian Luiscius, and Bondt)	2·9	..	2·3	4·5	6·8
Mare (ditto)	0·8	..	1·62	8·75 only 3 7 according to Young.	10·37

By reference to the fifth column of this table, you will see what very unequal quantities of nutritive matter these different kinds of milk contain. However, the discrepancies among authors is so great on this subject, that I have no great faith in the correctness of the above table, though I have drawn it up from the best data we possess.

The milk of the same species is also liable to considerable variation, depending on numerous causes; for example, the quality of the food modifies the quality of the milk. Dr. Young found that a bitch fed on vegetable aliment yielded an acescent and spontaneously coagulable milk; but when animal food was employed, the milk was alkaline, and did not spontaneously coagulate.

Dr. Cullen says, "I allege it to be a matter of experience, that supposing the quantity of liquid to be the same, nurses living entirely, or for the greater part, upon vegetable aliment, afford a greater quantity of milk, and of a more proper quality, than nurses living upon much animal food. This I venture to assert, from the observations of fifty years."

The influence which many medicines taken by the mother have over the suckling infant, is a circumstance known to every nurse, though Cullen denies it. We can modify the *colour* of the milk by mixing saffron or madder with the food; the *odour* may be affected by various cruciferous and alliaceous plants; the *taste* may be altered by the use of bitters, as wormwood; and lastly, the *medicinal effect* may also be influenced. Children may be salivated by suckling nurses under the influence of mercury, or purged by the exhibition of drastics to the nurse. Mental emotions also affect the quality of the milk. I have frequently seen the bowels of the child disordered in consequence of some sudden mental emotion on the part of the mother. It is also not improbable that diseased conditions of the parent may render the milk unhealthy. Labillardiere states that the milk of a cow, affected with a kind of tuberculous phthisis (*pommelière*) contained seven times more phosphate of lime than usual. Now this is a circumstance of the greatest moment, not only in reference to the frequency of this affection in cows, and, therefore, to the possible morbific character of such milk, but it is of considerable importance in reference to the milk of the human subject. I think, with this statement before us, it is highly improper to allow a female with any trace or suspicion of tuberculous disease to suckle. Not that a few grains, more or less, of phosphate of lime in the milk can probably do any injury to the child, but the fact once established, that the milk may be thus altered by disease, leads to the suspicion that some other substances not yet recognised by their physical or chemical characters, may be in the milk of diseased nurses, and which may have an injurious influence on the child; and the suspicion

does not confine itself to those affected with tuberculous diseases: other hereditary or constitutional affections may also be attended with altered conditions of the milk. This suspicion is strengthened by the common observation that the milk of nurses will not equally suit children. A child quite healthy, and in good condition, will sometimes, without any evident disease, fall off, and get into what is commonly called a bad condition, apparently from a change of the nurse. I am aware that we cannot always refer this to any positively hurtful matter in the milk. The quantity of nutritive matter in the same quantity of milk of two nurses, may be very different: according to Payen, milk with too much nutritive matter in it may disagree with the child. Another point worthy of attention, is the quantity of milk yielded in a given time. Payen says it varies in different women as much as from 1 to 10½. Climate, age, and period after delivery, are among the circumstances influencing the quantity of milk secreted.

Medicinal uses of milk.—We take advantage of the nutritive qualities of milk either as an article of diet, or as a ready means of introducing a large quantity of nourishment into the system in a short period of time. For example, after violent hæmorrhages, as those from the uterus, milk is one of the best nutrients we can employ. In these cases the most nutritive milk (as that of the goat, ewe, or cow) will be the best. A milk diet has been strongly recommended in consumptive cases, and sometimes with benefit. It has also been recommended in gout. Probably we ought to regard it as a negative means—that is, it is nutritive without being exciting. In these cases, woman's or ass's milk is the best.

We employ milk also as a demulcent. Thus it is used in the form of lotion or cataplasm, mixed with crumb of bread: it quickly undergoes decomposition, and then acts as an acrid substance. Internally, milk is an exceedingly useful demulcent in irritation, or chronic disease of the digestive organs. In poisoning by all acrid poisons it is useful, and in some cases acts as an antidote—as in poisoning by corrosive sublimate, perchloride of tin, sulphate of copper, &c.

The whey of milk is a mild refrigerant; but when taken warm with a stimulant (as wine), is a powerful diaphoretic.

Gelatine is another product from the ox which has been used in medicine. It is obtained by boiling bones in water. In Paris a nutritious soup is prepared, for hospitals and other pauper habitations, from bones; which are first digested in hydrochloric acid, to dissolve out the phosphate of lime, and then boiled in water. Soup has even been prepared from the fossil bones of the great mastodon, by a Prefet of one of the departments of France. Seguin employed gelatine as a tonic in agues. He thought that the active principle of cinchona bark was gelatine, since it precipitated the infusion of galls like the latter substance.

An extract of *ox bile* was formerly employed in medicine as a tonic, in doses of a few grains in the form of pills. This extract consists (according to Berzelius) of

Biliary matter (considered by Thenard and Gmelin as a compound of several proximate principles).
Mucus of the gall-bladder.
Alimentary extract.
Muriate and lactate of soda.
Soda.
Phosphate soda, and lime.

REPORT OF THE DISEASES OF MARYLEBONE,

AS OBSERVED IN THE PRACTICE OF THE ST. MARYLEBONE INFIRMARY,

From July 24, 1835, to November 13, following.

By John Clendinning, A.M. & M.D. one of the Physicians, &c.

	In-Patients.		Out-Patients.	
	Males.	Females	Males.	Females
Abortus	0	0	0	7
Abcessus	5	3	2	2
Amaurosis	1	0	0	1
Ambustio	1	1	0	0
Amenorrhea	0	0	0	2
Anasarca	0	0	3	4

	In-Patients.		Out-Patients.	
	Males.	Females	Males.	Females
Aneurisma	1	1	0	0
Anthrax	0	0	0	1
Apoplexia	5	6	2	3
Asthma	2	1	0	3
Carcinoma scroti	0	0	1	0
Cardialgia	0	0	1	0
Cephalalgia	0	0	0	3
Cholera	8	8	0	2
Colica	2	0	0	1
Concussio cerebri	1	1	1	0
Contusio	4	6	2	5
Cut throat	1	0	0	0
Debilitas	6	2	2	2
Delirium tremens	2	1	1	0
Diarrhœa	4	4	11	17
Dysenteria	0	0	0	1
Dysmenorrhea	0	0	0	1
Dyspepsia	0	0	3	8
Epilepsia	5	1	0	0
Erysipelas	5	5	3	3
Fractura	5	2	1	1
Febre F. Simplex	42	38	32	31
Gastro-Enterite	2	2	4	21
Typhus	2	3	6	8
Rubeola	0	0	14	17
Scarlatina	5	0	2	3
Variola	0	0	10	10
Gonorrhœa	3	0	0	0
Hematemesis	0	0	1	0
Hemoptysis	2	0	0	0
Hæmorrhagia	0	0	0	3
Hernia	1	0	2	0
Herpes	3	3	0	0
Hydrocele	2	0	0	0
Hydrops	4	6	1	0
Hysteria	0	0	0	5
Inflammationes				
Inf. Mammæ, Genu, Cruris, &c.	0	3	3	6
Bronchitis	7	19	10	34
Cynanche, Tonsil. Parotid. &c.	0	0	4	6
———Laryngea	1	0	1	1
Enteritis	1	3	1	3
Gastritis	1	2	0	0
Hepatitis	0	0	1	1
Hernia humoralis	2	0	3	0
Hydrocephalus	0	0	3	5
Ophthalmia	23	2	0	8
Otitis	1	0	0	2
Peritonitis	0	1	0	3
Phrenitis	2	2	1	3
Pleuritis	2	10	4	5
Pneumonia	9	3	19	9
Impetigo, Lepra, and Psoriasis	1	0	3	2
Icterus	0	1	1	0
Leucorrhœa	0	0	0	1
Lumbago	3	0	4	3
Mania et Melancholia	7	13	3	1
Marasmus	0	2	0	3
Menorrhagia	0	0	0	4

	In-patients.		Out-patients.	
	Male.	Female.	Male.	Female.
Morbus Chronicus Cerebri	0	2	0	0
——— Cordis	5	8	7	1
——— Coxæ	0	0	2	1
——— Spinæ Dorsi	2	0	0	0
——— Mesenterii	0	2	0	0
——— Uteri	0	2	0	0
——— Ventriculi	2	4	0	0
Mors Subita	0	0	1	0
Necrosis	0	1	0	1
Nymphomania	0	0	0	1
Obesitas	0	0	0	1
Obstipatio	1	0	1	3
Œdema	0	0	0	1
Paralysis	5	0	1	0
Pernio	11	2	0	0
Pertussis	1	2	0	0
Phthisis	20	15	19	20
Pleurodynia	0	0	0	1
Podagra	0	0	2	0
Poisoning	0	1	0	0
Porrigo	22	10	2	0
Prolapsus Ani	0	0	0	1
Psoas Abscess	1	0	0	0
Psora	21	22	3	1
Ptyalismus	0	0	0	2
Purpura Hæmorrhagica	2	0	0	0
Retentio Urinæ	2	0	1	0
Rheumatismus	11	4	26	23
Sciatica	0	0	0	1
Senectus	1	1	0	5
Sequelæ Partûs	0	0	0	43
Strictura Urethræ	2	0	0	0
Sphacelus	1	4	0	1
Syphilis	3	8	4	4
Tympanites	0	1	0	0
Ulcus	11	11	5	4
Urticaria	1	0	1	1
Varicella	1	0	0	1
Vertigo	0	0	1	0
Total	308	255	244	382

TABLE *of Admissions, Discharges, and Deaths, from July 24th, to November 13th,* 1835.

	In Patients.	Out Patients.
Under treatment on the 24th July, 1835	169	50
Admitted to 13th November	572	724
	741	774
Discharges from 24th July to 13th November	430	602
Deaths	91	48
Out-Patients admitted into Infirmary	—	80
Under treatment on 13th Nov.	220	44
	741	774

Observations on the Table of Cases.

From July 24th to September 9th, the decline of seasons appears to have had little effect on the health of the poor of the district of Marylebone. The admissions into the Infirmary during that period varied little. From July 24th to 31st, thirty-four persons were admitted as in-door patients, and fifty-one as out-door. At the middle of the period, viz. Sept. 25th, the admissions were—in-patients, thirty-six; out-patients, thirty-nine; but within a few weeks after, an increase became sensible. At the close—namely, Nov. 13th, the admissions of the preceding week were—in-patients, forty-nine, out-patients, fifty-two.

With respect to the cases of ophthalmia, psora, and porrigo, an observation formerly made may here, without impropriety, be repeated. From the columns of in-patients, compared with those of out-patients, there appears a singular preponderance for an hospital of such cases which are as well or better suited to the resources of a dispensary. The reason is, that all cases occurring amongst the hundreds of children in the Workhouse Schools, have instant admission into the Infirmary.

TABLE *of Deaths in the Wards of St. Marylebone Infirmary and Workhouse, from July 24th to November 13th.*

	Male.	Female.
Ambustio	0	2
Aneurisma	1	1
Apoplexia	2	0
Carcinoma	2	0
Cholera	1	1
Convulsions	0	1
Diarrhœa	0	4
Dysenteria	0	1
Epilepsia	0	1
Erysipelas	1	0
Enteritis	1	2
Febris simplex	1	0
—— typhus	2	4
Fractura femoris	1	0
Hydrops	0	1
Icterus	1	0
Morb. chronicus cerebri	0	4
———— cordis	4	8
———— hepatis	1	0
———— mesenterii	0	1
Morb. chronicus renum	1	0
———— ventriculi	1	1
Marasmus	1	1
Phrenitis	1	2
Phthisis	15	15
Pneumonia	4	2
Purpura hemorrhagica	1	0
Senectus	0	3
Sphacelus	3	5
——— pulmonum	0	1
Ulcera intestinorum	0	3
—— poisoning	1	1
Total	45	65
Add 2, diseases unknown at closing of account	2	0
Grand total for Infirmary and Workhouse	112	

Observations on the Table of Deaths.

From even a cursory glance at the above table of deaths, it will be observed that the mortality is considerable when compared with that of some other hospitals. One principal reason of the mortality will appear from the following table, containing the ages of the dead, given in periods of ten years:—

	Males.	Females.
To age of 10 inclusive	5	10
.. 20 ..	1	2
.. 30 ..	4	8
.. 40 ..	6	10
.. 50 ..	7	8
.. 60 ..	6	2
.. 70 ..	6	10
.. 80 ..	6	11
.. 90 ..	2	5
.. 100 ..	0	1
	43	67 = 110

In the British Medical Almanack, the editor of which seems much versed in statistical inquiry in general, also to have taken considerable trouble in collecting information respecting London hospitals, the mortalities of eight

principal hospitals are stated [B. M. A. 1836, p. 122] to vary from 7·6 per cent. (Bartholomew's) to 10·6 (St. George's) on the totals of admitted in-patients. From private information I can depend on, I am inclined to think that statement substantially or in the main correct. Yet those hospitals are authorized to refuse, and often do refuse, admission to hopeless cases, especially phthisis; to all infants, &c.

From the preceding table, it appears that more than one-third had passed 60 years of age, and considerably more than one-eighth were young children—several, in fact, infants. Another reason, also, is prominent in the table—namely, the large proportion of diseases of the organic kind, admitting of palliation only, and ultimately fatal, under whatsoever treatment or circumstances. Phthisis, and disease of the heart alone, give 42 of the 112.

I have instituted some inquiries respecting the mortalities of other parochial charities, and from such answers as I have already been favoured with, I consider the St. Marylebone mortality rather below than above the average of such institutions; as most assuredly, from its airy site and superior internal arrangements and resources, I should have expected. In an adjoining parish, to give an example, which has a large workhouse and well ordered sick wards, and good professional attendance, I learn, from a statement in my possession, that, from July 24, 1835, to November 13th, the mortality was to the admissions, including those under cure July 24th, as one to five; one-half nearly, or about one-sixth (to use a round number), greater than the St. Marylebone mortality.

The mortality table includes three deaths from old age; but of the 112 deaths it records, 21 occurred in the Workhouse, in which, independently of the sick-house, the old name for the Infirmary, there are wards for lying-in women, for idiots and epileptics, and for infirm and bed-ridden persons; altogether fourteen wards, which are under the charge of the resident assistant surgeon-apothecaries, superintended in the accouchement department by my distinguished friend Dr. Robert Lee.

Cholera.—The cases of cholera noted in the table of deaths, occurred all in August and September, and were the only cases fatal in a total of 16.

Owing to my absence in Dublin during the meeting of the British Association, and elsewhere on business afterwards, the whole of the cases, with one exception, were under the care of my able colleague and friend, Dr. Sims, so that I cannot speak of them from personal observation: they were of all ages, and, as the table shows, of both sexes—five were under 20 years of age, seven were above 60. In more than half, the characteristic symptoms—viz. rice-water evacuations, vox cholerica, ghastly expression, cramps, blueness, coldness, and attenuated and nearly insensible pulse—were present on their introduction into the Infirmary. In the others, the coldness and lividity, cramps, &c. were less intense, and the pulse could always be easily counted; but in a general aspect, they agreed with the former, and were no doubt of the same family. The practice was in nearly all the same, viz. calomel and opium at short intervals, with effervescing draught, for the first day and night, followed on the second day by mild aperients. The hot air bath also was used, nothwithstanding strong aversion to the heat, and to the fixed position it required in several instances, and with apparently very good effect. The most striking advantages arising from it were suppression of the cramps, and a return of warmth to the chilled extremities. The use of calomel and opium was generally followed by bile in the stools on the second day of treatment, and by immediate or speedy relief, to a greater or less extent, of gastric irritation.

One body was examined, and presented the usual choleric appearances, viz. parenchymatous congestion in every cavity, pasty coating of the mucous membrane of the small intestines, gruel, rice-water, &c. in the cavities of the latter, and black coloration of the tissue of the former; the heart charged with black blood.

In one case, I understand, in which there were symptoms of cerebral oppression, bleeding to 8 oz. was used, and with much advantage.

Aneurism of the Aorta.—The lamented death of Sir David Barry, occasioned by rupture of an aneurism of the aorta, has given a transitory additional interest to cases of that class. There have been several instances of the disease very lately in the Infirmary, the remains of some of which have been inspected. The most nearly resembling that of Sir D. B. was that of

a female, of 65, who died suddenly on the 10th ult. She was of short stature and plethoric habit, and was an inmate of the Workhouse until just before her death. It was not suspected that any disease of that kind had existed. Shortly before her admission, she had had some uneasy sensations, for which she had recourse to smoking, but was speedily seized with faintness and syncope, and soon died. Next day her remains were examined, when the heart was found floating in blood, of which probably half a pint at least was poured out from the sac of the pericardium after a free incision; there was aneurism of the aorta ascendens, from the cavity of which the blood had escaped through an elongated irregularly-shaped opening in the wall of the artery into the cavity of the pericardium.

Aneurism and Hypertrophy. — Two days after, we inspected the remains of a patient of mine, a man of 48, long ill of hypertrophy of the left side of the heart. In the summer of 1833, he had undertaken a pedestrian contest with another man to Brighton, for a wager. In something more than eleven hours, he stated that he had completed his journey less by two miles, when he felt himself unwell, shortly became faint, sat down on the road-side, and of course lost his wager; ever since he has been suffering from heart-beat and other symptoms referable to organic lesion, and on the 13th of October was admitted under my care. Nothing availed to give him permanent relief, and in a month he sank. During his stay in the Infirmary, the bellows sound was always very conspicuous in front, and sometimes behind and at the side; the first, or ventricular sound of the heart, was well marked, but the second, or short sound, was missing, obscured, apparently, by the *bruit de soufflet.* The inference was, in addition to hypertrophy of the left ventricle, defective action of the systemic semilunar valves. We found, accordingly, in addition to hypertrophy of the systemic ventricle, whose walls were thick and massive, and cavity probably double the normal size, considerable deficiency of the valves of the aorta: one was puckered, and a second was attenuated and curved outwards, so that the free edge must have extended beyond the line of traction, and yielded to some extent to every impulse from either side. But the principal source of defective valvular action seemed to be the expansion of the cavities each side of the valves: for, in consequence of the aneurisms of the heart and aorta, the communicating opening was itself expanded, the valves removed from each other, and free influx secured to a large portion of the aortic contents. The aorta was, through the greater part of its length, expanded, and every where more or less charged with atheromatous deposit. In the abdomen, at the root of the cœliac axis, was found a small aneurismal sac, which, had the patient not been cut off by one of the syncopes to which such subjects are liable, must ere long have destroyed him. In such a state of permanent patency to a greater extent of the outlet of the left ventricle, it is obvious that the signs of defective valvular action must be well marked, whether we suppose, with perhaps a majority of pathologists of this country, that the bellows sound depends simply on reflux, or that it is caused by divergency or eddies in the refluent current, as maintained by Dr. Corrigan, in an able address to the medical section, at the late meeting of the British Scientific Association.

Poisoning with Sulphuric Acid. — The case marked in the table "Poisoning," was that of a woman of 28, who, from some disappointment in love, swallowed $1\frac{1}{2}$ oz. of sulphuric acid, and was immediately seized with the symptoms of irritant poison. Magnesia was administered speedily and freely, by a neighbouring practitioner, and a stomach-pump was used, I imagine to effect the introduction of the antidote; and next morning, fifteen hours after the rash act she was admitted into the Infirmary (September 5th). The symptoms observed at her admission were the usual signs of the most acute gastritic irritation. There was no hæmatemesis; the bowels were confined; the pulse quick and feeble. Energetic measures were immediately taken, under the direction of my colleague, Dr. Sims, who kindly acted for me during my absence from town. From the 6th to the 16th, blood-letting by lancet and leeches was repeatedly employed, and the gastritic symptoms were so far subdued that, with the exception of a blister applied to the præcordia, to be dressed with savine, no important antiphlogistic means were considered necessary until the 20th of October, when recourse was again had to leeching. On the 16th I saw her, for the first time, and found

her greatly relieved, but thought it advisable to add prussic acid to her gum mixture. Under these she, in two or three days, so far improved, that, what with the mitigation of the symptoms on the one hand, and the wasting of the patient, and loss of power in the circulation, and calorific function, on the other, I was induced to venture on a little solid food and port wine. But in a day or two the gastritic symptoms (leeching, prussic acid, &c. notwithstanding) got ahead again, and never receded. On the 21st I had occasion to visit a distant part of Kent, and she died on the day of my return. On the 25th her remains were inspected. The principal morbid appearances were in the gullet and stomach; the tongue was shortened, and, as it were, humped, but without sloughs; from the tongue down to two inches from the rima glottidis, all was entire and pale, but a very little farther down there was sensible puckering of the mucous membrane, with opacity of the cuticle; about three inches below the rima were observed the first traces of ulceration, beginning at several different points in the circumference of the gullet, and extending downwards and laterally, so that somewhat lower down traces merely of the mucous and cellular linings were observable, in the form of elevated specks, in colour and hardness like cartilage; lower down still, viz. five to six inches from the rima, the muscular coat seemed quite bare. In the stomach there was ulceration and loss of substance at both the cardiac and pyloric extremities, with thickening and contraction of the apertures; the pylorus was thick and firm, and too narrow to give passage to an average-sized little finger; and in its immediate vicinity was an excavated spot, probably two lines in depth, and, as I conceived, the effect of the poisoning. The stomach was all over much injected and swollen, but the mucous lining had, with the exceptions already mentioned, suffered in texture little, save a spot at the great end, where there was softening and erosion. The patient had vomited almost immediately after swallowing the acid, and antacids had been administered with very little delay. This case, a notice of which was read to the Harveian Society (at a meeting at which I had the pleasure of seeing Sir David Barry in apparently the best health, though but a day or two before his loss to the profession), excited some discussion, which turned a good deal on a point to which I have not yet particularly adverted—viz. the use of purgatives in the after-treatment of this as a case of irritant poisoning. In this instance, as in the majority of such, there was obstinate constipation: enemata produced no satisfactory effect, and active purgatives were administered by the mouth. Croton oil was administered in two-minim doses six or eight times in the course of the first five weeks of treatment, and with due effect on the bowels, and without apparent countervailing disadvantage. Now the propriety of including purgatives in the remedial plan was disputed by several speakers, and amongst the number by the distinguished Editor of the Medico-Chirurgical Review, who favoured the society on that occasion by taking part in its proceedings.

The argument against the use of purgatives by the mouth was, as it appeared to me, conceived much in the speculative spirit of the French school, while the defence was conducted on the empirical principles of British practice. It was maintained that the introduction of acrid substances, such as croton oil, in cases of corrosive or irritant poisoning, is an application of irritants to parts already much injured, and highly irritable, &c. and not only unnecessary in point of fact, but dangerous in tendency; and that the natural efforts, aided by enemata, should be sufficient for all the defecation required. "Autant les purgatifs sont en général utiles dans ces maladies (engorgemens du foie), autant ils sont dangereux dans presque toutes les inflammations intestinales aigues et chroniques, particulièrement dans les gastro-enterites et les gastro-enterites, et les entero-colites ordinaires."—*Guersent, Dict. de Médecine, art. Purgatifs*; and the only other authority at hand, viz. Barbier, who writes the analogous article in the *Dict. des Sciences Méd.*, disapproves of purgatives in inflammatory fever (synocha, I presume), in inflammations of parenchymatous parts (as pneumonia, hepatitis), of serous membranes, &c. But it is not necessary to seek for authorities for the fact that the great physicians of France are generally indisposed to the use of purgatives, and but, as I should say, imperfectly acquainted with their value.

I have myself seen, I conceive, enough of the practice of the Hôtel

Dieu and La Charité, in the services of Laennec, Chomel, and Cayol, at the latter, and of Recamier at the former, to be able to affirm it of my own knowledge. Now none will deny that the opinions and practices of such men are to be viewed with deep respect; nor should I venture to refuse to the opinions of the clever but rash exclusive, the founder of the *soi-disant* physiological medicine, considerable weight; yet I cannot admit the high claims put forth by Gallic authorities for the *potion gommeuse* and *diete absolue*, as compared with the British substitutes of light or low diet, evacuation more or less free by purgatives and diaphoretics, and counter-irritation.

The French method, so far as mucilage and famine are concerned, is, in my opinion, at the utmost to leave nature to her own resources, to suffer the injured organs to right themselves if they can; while the British plan is, if I may so express myself, to endeavour to help them to do so.

1. Pungent lotions, gargles, ointments, &c., are in general use as applications to inflamed parts, and with success, on both sides of the channel. The principle of practice on which the objection to purgatives seems to be founded, and which is, I believe, briefly expressed by *contraria contrariis medentur*, does not, when rightly interpreted, mean that to parts in a state of irritation, or inflammation, no substance should be applied which is capable of irritating or inflaming healthy parts. The antagonism referred to by the ancient is not of a simply physiological character, and its laws and conditions are not best investigated chemically or physiologically, but by clinical trial and experience; and the aphorism may be translated or paraphrased for this occasion—"Irritation and inflammation should be treated with such agents only (irritants, excitants, stimulants, &c. included), as are known to subdue the species of irritation under treatment, and not at all with such as appear from experiment to aggravate it;" so that the admission of the truth of the aphorism to its fullest extent by no means implies the exclusion of purgatives from the treatment of cases of irritant poisoning.

2. British experience proves that purgatives are, in one state or other of combination, valuable remedies in many diseases in which gastric and intestinal irritation is in one degree or another undoubtedly present; *ex. gr.* every form almost of fever, enteritis, peritonitis, dysentery, recent diarrhœas, cholera, colics of every kind, &c. &c.

In addition to the security against the gastric and intestinal distention and irritation, so often arising from foul and fermenting excretions, it may be urged in favour of the British practice, that purgatives, when judiciously administered, not only neutralize the irritation they themselves have excited by the copious exhalations they occasion, but that their operation extends further, and that they diminish and remove preexisting irritations of the alimentary tube. How, in fact, should saline, oily, and other active purgative substances, simple or combined, prove useful, as they unquestionably do in intestinal irritations of most kinds, if it were not that exhalation, when judiciously excited, counterbalances the irritation of the stimulant applied to the exhaling tissues, while it at the same time roots up the morbid actions constituting the original disease?

On the whole I am disposed to think that purgatives by the mouth should have place in the armoury of the physician as one of many remedies for the after-treatment of cases of irritant poisoning; while I readily admit that, in several combinations of circumstances, it may be advisable to prefer the method of stimulating the bowels per anum.

J. C.

16, Whrp le-Street,
Nov. 23, 1835.

STRUCTURE AND USES OF THE INTERVERTEBRAL SUBSTANCE.

To the Editor of the Medical Gazette.

Sir,

You will oblige me by giving the following remarks a place in your widely-circulated journal, should you deem them of sufficient importance.

I am, sir,
Your obedient servant,
Hamilton Labatt, A.B.
Demonstrator of Anatomy, Medical School, Marlborough-street.

Dublin, 1, Rutland Square,
Nov. 16, 1835.

Between the bodies of those irregular bones which constitute the spine, and form the connecting chain between the superior and inferior divisions of the human frame, we observe a remarkable fibro-cartilaginous substance interposed, which not only serves to protect the brain and spinal marrow from the effects of jars and concussions, but also, by enduing the whole column with a marked degree of springy flexibility, enables it to react on every motion of the body. So great, indeed, is the perfection and power of this great central spring, that we find, when the trunk has been thrown suddenly in the lateral direction, and the muscles commence their action to recover and restore it to a straight and steady position, a distinct vibratory or oscillatory motion is carried on in the lumbar region, before the contractile organs succeed in mastering and reducing the whole to a state of quiescence.

In examining the nature of the intervertebral substance, we find it composed of two distinct portions, differing both in their consistence and internal arrangement. The first we shall describe occupies the centre, and has been ingeniously compared to a central pivot for facilitating the motions of the vertebræ on each other. It is of a whitish colour, presents the appearance of a homogeneous and almost gelatinous mass, and possesses a considerable degree of elasticity, as is proved by its starting up and rendering the whole surface convex when a knife has been passed transversely between two vertebræ, and the pressure of the superimposed bones removed. I am aware that this portion of the intervertebral material is considered by some anatomists as quite inelastic; for the reason assigned, however, I must dissent from such an opinion.

Surrounding the central part, and confining it within its proper limits, we find the second or external portion. This is evidently of a fibrous structure, rather of a darker shade than that just described, and possessing a much greater degree of firmness and strength.

With respect to the fibres of this external division, which is likewise elastic, they are concentrically arranged, and assume a crucial direction with regard to each other. This is best displayed by taking a portion of the lumbar region of the column, and dissecting off the anterior common ligament; when, by slicing away a part of the intervertebral substance in front, we bring into view the substance alluded to.

The general opinion as to the above crucial arrangement, is that it serves to give a greater degree of elasticity and spring to the spine. This end I allow it answers. On considering the subject further, however, and on examining this peculiar provision in connexion with the motions which the column enjoys, I was led to a second important use; not as yet, I believe, proposed by any writer, and which I would therefore suggest as worthy of investigation. I mean, a power possessed by the crucial fibres of the external portion of the intervertebral substance, of limiting twisting motion; and, by their elasticity, of restoring the whole to its original position when the twisting effort is over.

With the view of following up this inquiry, I separated two lumbar vertebræ from the spinal column, preserving their articulations, &c. I then sawed off the posterior circles, in order to avoid, as much as possible, any interference with the free and independent motions of the bodies on each other; and having dissected away the anterior common ligament, I brought into view (in the manner already directed) the crucial fibres of the intervertebral material. By twisting the bodies on each other, I was now enabled to observe very distinctly the two orders of fibres, alternately relaxed and stretched. On the parts being left free, there was a spring back to their regular position. I may here remark, that so evident were the crucial fibres made by the above motions, that I would recommend the student who may wish to see them to adopt the course pursued by me.

Having so far established my position, the next point to be considered was, whether the crucial arrangement is as evident in those regions of the spine which do not admit of twisting motion, as in that which enjoys it in a very appreciable degree. To elucidate this, I applied my attention to the cervical division, where we know, from the peculiar mode in which the bodies of the vertebræ are locked into each other, no twisting can be effected. Here I found the crucial fibres, comparatively speaking, very indistinct indeed, and the substance becoming more homogeneous. Again, in the dorsal region, although

they were much more distinct than in the cervical, still they were not so well marked as in the lumbar, where twisting is principally enjoyed.

Between the atlas and vertebra dentata, we do not find any provision analogous to the intervertebral substance. The rotatory motion here is limited by check ligaments, which pass from the summit of the odontoid process of the dentata to be attached to the inner edges of the occipital condyles.

Before I dismiss the consideration of this important structure, I shall offer a remark on an opinion which is advanced in an article on Animal Mechanics, published in one of the numbers of the Library of Useful Knowledge. It is there asserted, that "when a weight is on the body, this gristly material is squeezed out from between the bones." From such an opinion I must certainly dissent; it bespeaks a degree of yielding and laxity which would by no means be compatible with the necessary safety and solidity of the whole; and moreover, the substance alluded to is confined by dense unyielding ligamentous bands, stretching along the bodies of the vertebræ, that I cannot conceive how such an occurrence could take place.

In conclusion, I would observe, that if the view which I have now put forth as to the use of the crucial fibres of the intervertebral substance be correct, we can readily see how we possess, in such a material, a provision which, whether we twist or bend the body, will always tend to restore it to the straight and upright position.

CASE OF INFANTILE AFFECTION OF THE SENSORIUM, SUCCESSFULLY TREATED.

To the Editor of the Medical Gazette.

Sir,

I trust you will consider the following brief outline of a case, which I have had recently under my care, of sufficient interest to receive a place in your valuable journal.—I am, sir,

Your obedient servant,

John Armstrong.

Gravesend, Nov. 10, 1835.

L. B., aged 2 years, of delicate make and complexion, with rather a large head, and possessing considerable vivacity and sensibility, was seized, whilst under the care of a relative at Chatham, with a convulsive fit, on the 7th of last August, from which she soon recovered, and no medical assistance was deemed necessary. On the 10th she was much frightened by a horse coming close to her; next day she had a much more severe fit, for which, finding she did not recover her senses, medical aid was obtained. The medical gentleman recommended leeches and blisters, to which the persons in charge would not consent. Some mild aperients were then prescribed, without benefit. On the 14th, finding no improvement, the child was taken home, and I was requested to visit her. The face is pale; she lies on her back; the skin cool; the eye-lids half cover the eyes, which seem to roll about in vacancy; the pupils act sluggishly, and a candle may be brought very close to the eye without any apparent consciousness; the pupils are rather dilated; there is equal insensibility to sounds, and pinching seems to give no pain; no voice nor sound has been uttered since the fit; there seems a total suspension of all the senses—not even the sense of taste seems to exist, as all substances, whether bitter or sweet, are swallowed indiscriminately; the pulse is feeble, and there is very little increased activity in the vessels of the head. From the known constitutional delicacy of the child, and the want of power in the circulation, I omitted bleeding, and incised the gums freely. The head was shaved, and covered with evaporating lotion. Blisters were applied behind the ears, and two grains of the hyd. sub. mur. were directed every four hours.

August 15th.—The bowels have not been relieved; blisters have acted slightly, without any apparent benefit. Powders of scammony and calomel were directed. In the evening she was seen by Mr. Park, of whose skill and experience I gladly availed myself. He concurred with me in the propriety of avoiding blood-letting, and recommended blistering the occiput, and keeping it discharging with the ungt. sabinæ, and to continue the calomel and scammony.

17th.—Bowels have acted freely; fæces exceedingly dark and offensive;

sensorium the same; pupils more staring; pulse feeble and intermitting. When the child is raised for any purpose, the head rolls as if all power of support was lost; total insensibility to light, sounds, or pinching; swallows any fluids pretty well. It was observed that there are periods of distinct easy sleep, which may be distinguished from the waking moments, if a state of insensibility to surrounding objects can be so called.

I shall not trespass on your columns by detailing all the particulars of this case; suffice it to say that an extensive discharge was kept up from the back and sides of the head; the calomel was steadily persisted in without producing any thing like ptyalism, until the 28th, when I ordered the following:—

> R Sol. Hyd. Oxymur. ℥iij.; Sp. Æth. Nit. ʒj.; Tr. Digit. gtt. xij.; Syrup., ʒij.; Aq. Cin. ℥iss. M. st. coch. j. med. 4tis horis.

This evidently increased the secretion of urine, but there was no apparent benefit; on the contrary, the powers of life seemed failing. I therefore ordered good beef tea, broths, and arrow root, with wine, keeping the bowels free with calomel and castor oil.

Sept. 3d.—Much the same, except that the left hand is observed frequently raised to the head; exceedingly low and weak, with a pulse scarcely perceptible.

> Quinine in small doses, and a larger allowance of wine, was ordered.

5th.—The motion of the hand seems somewhat like that of a person labouring under chorea; pulse more developed.

> Coch. Ferri was ordered in addition to the quinine.

From this period the child began to mend, and it was truly astonishing how rapidly she improved.

It was exceedingly interesting to observe the various stages of improvement. The sense of taste seemed first in order, as was evinced by her grimaces after the quinine. Sight next; the hand passed across the eyes caused winking. She began also to follow objects with the eyes for a short distance; and a faint smile was occasionally observed to alter the fearful and long-continued composure of the countenance. I now ventured, in answer to the anxious inquiries of the parents, to assure them that voice would also soon return, and that the intellectual faculties would be restored. She could, however, run about the room, play, smile, &c., but no voice was uttered to relieve our anxiety.

27th.—Sounds have been attempted, which have gradually become more determinate; and the vacuity of countenance has greatly diminished.

Oct. 20th.—rapidly improving. The only complaint is that the child seems disposed "to run about too much, and stuffs every thing into the mouth," probably owing to dental irritation; but speech, taste, feeling, hearing, &c. are restored.

The above case was to me and the friends one of the most intense interest, and the pleasing issue of no small gratification. It has appeared to me to have some points very similar to a case reported in the Gazette a short time back, from Professor Badham, though obviously attributable to a very different cause. I feel persuaded that depletion in my case would have been attended with bad consequences, as I have no doubt it was the cause in his; yet I must say, I think the severity with which he has visited his brother's error (and who is not liable to err?) is by no means commendable.

REPORT OF FRACTURES

TREATED IN THE LONDON HOSPITAL DURING OCTOBER.

To the Editor of the Medical Gazette.

Sir,

I inclose you the report of fractures admitted into the London Hospital during the month of October.

I have selected one case for especial notice. It occurs under the head of fractured fibula; this was complicated with a compound dislocation of the tibia inwards.

I am, sir,
Your obedient servant,
John Adams.

New Broad-Street,
Nov. 1835.

		Right.	Left.	Unknown.
Cranium	2	0	0	0
Spine	1	0	0	0
Sternum	1	0	0	0
Ribs	7	3	3	1
Scapula	1	0	1	0
Clavicle	6	1	5	0
Humerus	10	4	6	0
Fore-arm (both bones)	1	0	1	0
Ulna	4	1	3	0
Radius	2	2	0	0
Hand	4	4	0	0
Femur	4	2	2	0
Leg (both bones)	2	1	1	0
Tibia	2	0	2	0
Fibula	5	4	1	0
Toe	1	1	0	0
	53	23	25	1

Oct. 12th.—Joseph Dark, æt. 45, a groom in a gentleman's family, whilst carrying a load up a ladder, his foot slipping, was precipitated, and fell upon the inner side of the right foot. It was twisted up, and the tibia passing off the upper surface of the astragalus, the inner malleolus protruded through the skin for the distance of at least an inch and a half. The fibula was fractured at about the distance of three inches above the outer malleolus. No very serious injury of the soft parts adjacent was visible. The minutest fragment of bone was felt in the wound, but this eluded my attempts to extract it. Shreds of the internal lateral ligament were adherent to the lower end of the tibia. The foot was in the position common to such accidents. By flexing the knee-joint, and forcibly drawing down the foot, it was speedily and easily reduced. The leg was laid upon the outside, with a splint with a foot-piece beneath it; and the wound was accurately brought together. Cold lotion was applied, and at night forty drops of laudanum were given.

13th.—The leg is painful, and there is a slight oozing of bloody discharge from the wound. Some febrile excitement is visible.

Ordered saline mixture, with ten drops of laudanum every six hours.

From this period an erysipelatous inflammation commenced in the leg, and extended up the thigh; this was attended with considerable fever, and a dusky red appearance of the skin about the knee. He lost a small quantity of blood, and the same medicinal treatment was pursued, with attention to the condition of the bowels. He appeared mending, but suddenly sank on the 19th.

Sectio cadaveris.—Suppuration within and around the joint. The tendons in its vicinity were all entire. Evident marks of diffuse cellular inflammation were seen along the leg, the cellular substance being infiltrated with pus. The whole limb exhibited signs of diffuse inflammation. A slight slough was observed on the outside of the leg, and the skin in the vicinity of the nates appeared to be approaching to a gangrenous condition.

The abdomen contained a quantity of bloody serum; and in the vicinity of the liver the intestines were covered with a semifluid substance like coffee-grounds. The intestines were distended with flatus, the peritoneal covering exhibiting marks of preternatural injection. The brain much more vascular than usual; its veins distended. The arachnoid opaque; considerable quantity of fluid beneath this membrane, and especially at the basis.

I scarcely think it fair to bring this case forward to militate against the position laid down by Sir Astley Cooper, of the propriety of attempting to save the limb under circumstances like the present, but I am bound to say that the results of the generality of cases that I

have witnessed in hospital practice, have induced me to form a very guarded opinion as to the ultimate success of such an attempt.

In the present instance, so far as the joint itself was concerned, nothing of a very untoward nature had arisen; but the patient seemed to have sunk from the effects of diffuse cellular inflammation, occurring in a constitution not of the most vigorous description.

ON THE

NERVOUS INFLUENCE AND ANIMAL HEAT;

IN REPLY TO DR. WILSON PHILIP'S LAST LETTER.

To the Editor of the Medical Gazette.

SIR,

DR. WILSON PHILIP is mistaken in supposing that by referring to the writings of others I meant to reply conclusively against his position, or to "determine the point by a majority of voices." The opinions of others were quoted to show that, from the facts hitherto known, opposing conclusions were drawn by different physiologists, and that the "legitimate inference" of Dr. Philip is denied to be such by other writers. These writers have stated the grounds of their dissent; in their works, and in those of Dr. Philip, the reader may study the facts and arguments on either side. There, in my opinion, discussion has already done its utmost; and as the "legitimate inferences" of the several writers are remarkable for their diversity, I may be well excused from unprofitably occupying the pages of the Medical Gazette with the details of an unsettled controversy, which are already before the public.

Before I quit this subject, I must, however, correct the interpretation and answer which Dr. Philip gives to a statement which I had quoted from Dr. Alison. The passage is this:—"The province of the nervous system in producing the phenomena of life, which was long neglected in certain schools of medicine, has been erroneously conceived in others; by Stahl, by Cullen, and Whytt; and to a certain degree by Bichat, by Legallois, and even by Dr. Wilson Philip; and was more accurately understood by Haller than" (not, as Dr. Philip adds, "in the present day," but) "*by any of these writers**."

That Dr. Alison does not here deny, what I have stated, that the researches of Dr. Philip and other recent inquirers have greatly increased the precision and extent of our knowledge, may be inferred from his frequent reference to the *facts* obtained in those researches; and it is equally clear from the context of his remarks, that it is in regard to *general conclusions* respecting the functions of the nervous system that Dr. Alison asserts the superior accuracy of Haller.

As the legitimacy of Dr. Philip's inference, adverted to in the first part of his letter, thus appears to be, to say the most of it, a matter of opinion opposed to opinion, so in his following observations we find his statement of a fact opposed to a fact described by a later observer. Dr. Philip states, on the authority of his own experiments and those of Legallois, that the gradual destruction or simple removal of a part, or even of the whole of the brain and spinal marrow, produces no effect whatever on the circulation. Dr. Marshall Hall concludes from his experiments on frogs and fishes, that the simple removal, or cautious destruction (without shock), of the brain and spinal marrow, is uniformly followed by a gradual failure in the capillary circulation, this failure beginning in parts most distant from the heart†. Between these conflicting statements I do not pretend to decide, for neither of them materially affect my view of the causes of animal temperature; but I may remark that in the experiments of Dr. Philip, to which he refers, it does not appear that the capillary circulation was examined so long as in those of Dr. Hall.

Dr. Philip next shortly adverts to the experiments which I had cited from Chossat, and remarks, that it could easily have been foretold that bruising the semilunar ganglion, or tying the aorta, would greatly impair the temperature, "because the one deprives the

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* Dr. Alison's Outlines of Physiology, &c. 2d edition, 1833. Preface.

† Essay on the Circulation, &c. 1831. Page 118 and seq. In my last communication these experiments were by mistake said to associate injuries of the spinal marrow only, with failure of the capillary circulation. The removal of both brain and spinal marrow was necessary; and then, within half an hour, the circulation in a frog's web ceased entirely, although the heart still continued to contract.

blood of a great part of the nervous influence, by the action of which on this fluid animal temperature is maintained, and the other prevents the due supply of the blood itself." Dr. Philip forgets that the effect of these operations, as described by Chossat, was not such a slow dimiuution of heat as that which results from destroying even large portions of the spinal marrow, but as rapid a cooling as that caused by the removal of the brain itself. Bruising the semilunar ganglion produced a more speedy cooling than the division of the spinal marrow at the upper dorsal vertebræ; and tying the aorta below the diaphragm made the animal cool as fast as when the brain and medulla oblongata are removed; and this cooling was equal in the œsophagus, which was still supplied with blood and nervous influence, and in the rectum, which, according to Dr. Philip's views, ought to have been exclusively or chiefly affected.

Now bruising the semilunar ganglion cannot destroy the functions of the brain and spinal marrow, which Dr. Philip says are "the only active parts of the nervous system;" nor, in his view, can it act by intercepting the influence, for he tells us that "the maintenance of animal temperature depending on functions which take place in every part of the system, and these being, like the other assimilating processes, functions of the central parts of the nervous system, can only be generally influenced by causes which influence the functions of these parts, *not by causes which prevent their influence from reaching particular parts of the body**."

So also, according to these views, tying the abdominal aorta ought to affect the temperature only as much as dividing the spinal marrow at a corresponding point, the part below which it can only incapacitate; whereas Chossat found its effect to be as great as that of the destruction of the brain and medulla oblongata, and greater than the effect of destroying the whole spinal marrow. These experiments, then, are signally opposed to Dr. Philip's views, whilst they strikingly illustrate the connexion of animal heat with secreting organs which receive their blood from the artery which was tied, and are remarkably influenced by the ganglionic system which was injured.

The fact which I adduced in my last communication, that heat is evolved in oxygenated blood out of the body, is ascribed by Dr. Philip to *nervous influence remaining in the blood*. Now this notion, and that of the influence remaining in nerves separated from the cerebro-spinal axis, must be judged by the view which Dr. Philip holds of the nature of this influence. He considers it to be voltaic electricity.

Now I may venture to affirm that there is no known property of voltaic electricity which could enable it to act in a nerve or in blood after its source of supply is cut off. The chemical properties of voltaic electricity exist only in its currents; and unless electricity be excited anew in the separated nerve or blood, there can be no currents in them. Electricity is not a ponderable agent, that can be stored up in a nerve, or retained in an active state in a cup of blood. We can, it is true, accumulate it in bodies by means of insulation and of induction; but there are no such means in the nerves or in the blood, in which an equal diffusion of electricity, and, therefore, a cessation of its activity, must ensue the moment that the current ceases. It is, I repeat, by unequal distribution, and by the currents which ensue to restore its equality, *and by these alone*, that voltaic electricity can heighten, control, or modify chemical action; and unless there be a source of such currents, there can be no disturbing electrical force. For the truth of these positions, I need only refer to the latest and best authority, in the masterly series of researches of Professor Faraday, contained in the Philosophical Transactions.

If, then, the nervous influence is voltaic electricity, it cannot be concerned in producing the heat evolved in blood out of the body. I have shewn that this evolution of heat may be fairly referred to the chemical changes that are known to accompany it. That *electricity is* NOT *the cause* of the heat in question, is thus proved by a reference to the known laws of electricity. That *chemical action* is *the cause*, is supported by what we know of the laws which regulate chemical action.

But in the next paragraph which I

* Med. Gaz. Nov. 7, p. 173.

have to notice, Dr. Philip assumes a position which, if it have any strength, is quite at variance with his preceding arguments, and totally destroys their force. The passage is too long to be repeated, but I would beg the reader to turn to it. (See the last paragraph of page 272, in Med. Gaz. Nov. 21.) Dr. Philip there argues thus: The chemical changes of inanimate nature depend on electric action; the same changes take place in the living body: to suppose the principle of action in these identical cases to be different, is absurd; *therefore* the chemistry of the living animal must depend on electric action, *which is the nervous influence.*

Your readers are aware that the electrical theory of chemical action very generally prevails among the chemists of the present day; and no fact has tended to support it more than the beautiful discovery of Professor Faraday, that there is an exact relation between the forces of electricity and those of chemical attraction. But this theory obviously supposes the electricity of chemical agents to be within themselves; and although chemical change is necessarily attended with a change in the relation of this electricity, it is still according to chemical laws, and independent of external agency.

In appealing to the electro-chemical doctrine, therefore, Dr. Philip adduces the electricity of inanimate chemistry, and thus supersedes the necessity of the electric power which he attributes to the nerves. To take his own example: carbon and oxygen (he intimates) unite in the blood on the same principle as in the laboratory of the chemist—that is, on the principle of electric attraction; but in the laboratory of the chemist this attraction depends on the intrinsic electricity of the uniting matters, which, being sufficient in this case, are sufficient, without the supposed nervous electricity, in the other.

To attribute to electricity all the phenomena in which its presence may be detected, is about as logical as to attribute to gravitation all the phenomena which ponderable bodies exhibit. An explanation in physiology, as in physical science in general, is the reference of a fact to known laws; and until voltaic electricity be proved to possess more properties than those by which it is at present known,—until some new laws be traced in its operation,—the view of Dr. Philip, which identifies voltaic electricity with the nervous influence, can be regarded only as an ingenious hypothesis. It is, perhaps, estimating it beyond its value, to compare it with the relation of magnetism to electricity in the time of Œrsted's first discovery; but the comparison will illustrate its position, and may stimulate the minds of future inquirers.

Œrsted found that an electric current affected a magnetic needle in a definite manner; but until, by further study, the laws of the relation of electricity to magnetism were made known, and until electricity was made to exhibit all the properties of magnetism, and magnetism to evolve electricity in its distinctive characters, no philosopher admitted that their identity was proved.

So Dr. Philip and others have shown that electricity affects certain vital functions in a definite manner, and that the living nerves in a single point resemble conductors of electricity. But until it be shown that these relations are not secondary,—until the distinctive properties of electricity be detected in the influence conveyed by the nerves, and the nervous function be not only imitated, but effectually performed by electricity, —the identity of the powers cannot be considered as established.

In these last remarks I may have somewhat exceeded the limits which I had prescribed to myself, and entered on a subject not essentially connected with my views on animal heat; but the observations in Dr. Philip's last letter have unavoidably led me to these observations, and I trust that they are sufficient to substantiate the opinion which I gave on this subject in my first letter.

I am, sir,
Your obedient servant,
C. J. B. Williams.

Half-moon-Street,
Nov. 30, 1835.

TREATMENT OF NÆVUS.

To the Editor of the Medical Gazette.

Sir,

Having read the report, in last week's Gazette, of three cases of nævus, treated by M. Lallemand, I beg to give you a

case treated by me, with complete success, upon the plan laid down some time ago, in your journal, by Dr. Marshall Hall.

Miss W——, 3 years of age, had, from her birth, a nævus occupying the apex and septum of the nose, which produced a very unpleasant appearance: its surface projected, and had the resemblance of a ripe raspberry.

On the 14th of May last, I punctured the nævus in every part with needles, and repeated the operation on the 28th.

At the end of a fortnight I observed a manifest diminution of the vascularity.

On the 24th of June I again punctured it as before. From this time it has been gradually diminishing in vascularity and elevation of surface, and at the present time there is not the least appearance of nævus remaining, the part resembling, with regard to vascularity, any other part of the face.

By inserting this brief account in your next number, you will oblige,

Sir, yours truly,
RICHARD WALLACE,
Surgeon.

Hackney Road, Nov. 30, 1835.

P.S.—It will be remarked that the seat of this nævus was such as to render the knife, the ligature, or the caustics, altogether inapplicable; the scar induced would have been a greater deformity than raspberry-like nævus itself.

The progress of the cure was extremely slow. Not having seen the case recently, I told Dr. Marshall Hall, yesterday, that I believed a point still remained of the original hue; but having visited my little patient this morning, I find that since my last visit that spot also has gradually and entirely lost its vascularity. It is important to remember this slow progress of the cure, in order to prevent undue expectations, and consequent disappointments. It is now more than six months since the first acupuncturations were performed.

I must admit that I know not what other plan of cure *could* have been adopted in this instance. This mode of cure must therefore be deemed of great value; and not the less, because it is attended with scarcely any pain, hæmorrhage, or other inconvenience.

ON THE SMALL SIZE OF THE PELVIS IN RICKETS.

To the Editor of the Medical Gazette.

SIR,

THROUGH the kindness of Mr. Sweatman, I have had the opportunity of making the accompanying drawing of a pelvis affected by rickets (fig. 2), with the view of contrasting it with the natural pelvis. It was taken, within these few days, from a female, æt. 28, who died from peritoneal inflammation, after having been delivered by the crotchet. She had the common appearances of being deformed by rickets; her shin-bones were curved forwards; yet the distortion was not very great. She had gone her full time, and had been three days in labour before she was seen by Mr. Sweatman. The head of the child had not descended into the true pelvis, and nearly an hour was occupied in completing the delivery, although the child was rather below the average size of a full-grown fœtus. Her death occurred nine days after delivery; and on dissection, besides the peritoneal inflammation, the inner surface of the uterus and that of the bladder were found to have suffered severely from the bruising of the child's head, so as to be in a state approaching to gangrene.

My object in presenting these drawings to the notice of your readers, is to point out a circumstance which I do not find touched upon by writers on midwifery in treating of rickets, and which yet appears of great importance: I mean, the smallness of the pelvis that results from the imperfect development of the bones which compose it. These authors, as well as the best writers on the pathology of the bones, confine their observations on the condition of the pelvis to the distortion which is consequent on the softness of the bones, and the pressure of the superincumbent weight; and attribute all the dangers met with in such cases, in parturition, to that cause alone.

Now in the pelvis (fig. 2) which is here represented, it is apparent that there is very little distortion of the bones. The promontory of the sacrum, it is true, has been thrust forwards to a slight degree, so as to destroy the pro-

Fig. 1.

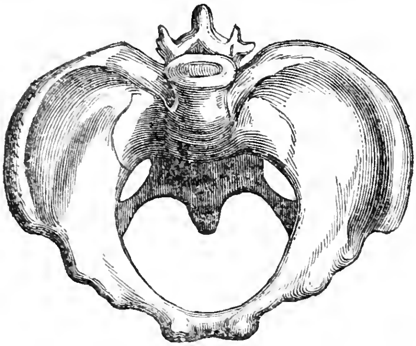
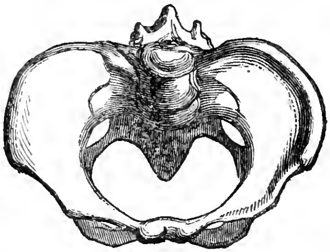


Fig. 2.



per oval form of the inlet; but this has only taken place to a trifling extent. Yet, when we contrast this pelvis with the pelvis of the natural size, placed beside it (fig. 1), we cannot but be struck with the remarkable smallness of the brim of the former, and how impossible it must have been for a full-sized fœtus to have passed through such a pelvis in a natural manner: and it is not only the antero-posterior diameter that is diminutive — which we might expect, from the sacrum projecting forwards; but the oblique and transverse diameters, which it might be supposed would be enlarged by the change in the form of the bones, are likewise diminished. In short, looking to the remarkable difference in the size of the two pelves, we cannot doubt that, in this case, it was the small size of the bones composing the pelvis, and not the deformity occasioned by the softening of their textures, that was the cause of the death both of the parent and child.

With the view of showing that the present case is not merely a singular instance of a small-sized pelvis occurring in a rickety person, but that this smallness of the pelvis is a common character in those who have been affected with this disease, I may be allowed to refer to a paper* which you did me the favour to insert in a former number of your journal, on the same subject. In that paper I gave a table containing the measurements of numerous rickety skeletons and pelves, obtained from different museums, and contrasted them with corresponding measurements of the natural bones. By such a comparison, it was shown that the disease in question, at the same time that it gave rise to softening of the textures of the bones and distortion, had the effect of arresting the growth of the whole skeleton. And another interesting circumstance was made evident. It was proved that in this stoppage of the growth, certain parts of the skeleton suffered to a greater extent than others. The several divisions of the skeleton having each an appropriate rate of growth, some undergoing a more remarkable change in their dimensions, during the period of growth, than the rest, it was found that those bones which naturally acquire the greatest increase of size were the most visibly affected by the development being arrested.

It will be acknowledged that there is no part of the skeleton that, in natural circumstances, exhibits a more extraordinary difference in its size, at birth and at adolescence, than the pelvis; especially in the female. Accordingly it was proved, from the measurements referred to, which included twenty-nine specimens of rickety pelvis, that this circle of bones suffered more extensively in its dimensions, from the effects of the disease, than any of the other parts of the skeleton. And the estimate which I made, by taking the average of the measurements, and comparing this with the natural measurements, marks how important it is to take this subject into consideration, for I was brought to the conclusion, that the diseased pelvis was defective in its growth, within a small fraction, to the extent of a quarter of the natural size of the pelvis.

This calculation corresponds with

* See the last volume of the Medical Gazette, p. 45. See also a paper in the Medico-Chirurgical Transactions, vol. xvii.

the difference of size in the rickety pelvis and the natural one which are here represented in contrast: as may be seen by consulting the table below *. The average of the measurements of the diseased pelvis, taken in six different directions, is $5\frac{1}{2}$ inches, while that of the natural pelvis is 7 inches: and $1\frac{1}{2}$, which marks the difference between them, is between a fourth and a fifth of the number representing the natural dimensions.

I am unwilling to encroach further upon the valuable space of your journal, or I might have touched upon the peculiarities to be remarked in the relative proportions of the skeleton affected with rickets, and contrasted with these the effects produced by an acceleration of the growth, in persons who are overgrown, instead of being thus stunted in their growth; and such questions might have illustrated what has been stated concerning the pelvis. It is also apparent how important it is to determine the period of life when rickets prevails; and to be able to distinguish between the distortion which it gives rise to, and the common lateral distortion of the spine; for the latter kind of distortion occurs before the growth of the pelvis is completed, and with greatest frequency in females, and has, moreover, been attributed by numerous writers to the influence of rickets. I may be allowed to state that these points are discussed in my former communication on this subject, in the last volume of the Gazette, and in the paper in the Medico-Chirurgical Transactions.

Every one knows that in a natural delivery the child's head undergoes a series of changes in its position upon its own axis; and that these are caused by the peculiar form of the head, and the points and surfaces of the pelvis being in succession opposed to its descent. Distortion of the pelvis alone will, therefore, derange the natural stages of labour, delay the descent of the head, and consequently exhaust the mother. In this way, a distorted pelvis, even if the diameters were very considerable, would call for the assistance of the accoucheur. But in the rickety pelvis there is an additional cause of protracted labour, depending on the diameters and whole cavity of the pelvis being deficient in size, from imperfect development; and that which I have drawn proves, from the fatal effects that it gave rise to, what a formidable source of danger there is in this smallness of the bones, even although the deformity be so slight as scarcely to be perceptible.

I have the honour to be, sir,
Your obedient servant,
ALEXANDER SHAW.

Middlesex Hospital School,
Nov. 23, 1835.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Médecine légale, théorique et pratique. Par ALPH. DEVERGIE, D.M.P., Professeur Agrégé, &c.; *avec le Texte et l'Interprétation des Lois relatives à la Médecine legale, revus et annotés* par M. DEHAUSSY DE ROBECOURT. Tome premier. 8vo. pp. 724. Baillière.

A GRAND fault fallen into by most authors who have hitherto produced works on legal medicine, has been, to dogmatize immoderately; to lay down as positive what can only be determined with probable exactness. Forgetting but too frequently the serious—we might say the awful, responsibility attaching to medico-legal research, when applied

*	Rickety Pelvis.	Natural Pelvis.
	Inches.	Inches.
From spine to spine of ilium	9	$11\frac{1}{2}$
Greatest height of os innominatum	$6\frac{1}{4}$	8
From anterior superior spinous process of ilium to posterior superior spinous process	$5\frac{1}{2}$	$7\frac{1}{4}$
Transverse diameter	5	$5\frac{1}{2}$
Antero-posterior diameter	$2\frac{3}{4}$	$4\frac{1}{2}$
Oblique diameter	$4\frac{1}{4}$	$5\frac{1}{4}$
	6)33	6)42
	$5\frac{1}{4}$	7

in aid of the administration of justice, they assert, where they should only indicate with reserve. Of this class was the late Professor Foderé, the great object of whose life seems to have been to sift out the truths which he thought were to be found among the chaffy masses of *facts* (so called) and opinions gathered into the garners of medical science. His *Traité de Médecine légale* was unquestionably at the time it was written, and for many years after, considered to be a masterly performance: estimated in point of learning and comprehensiveness of plan, it is still a highly valuable work: but it has the besetting sin of attempting to *fix* every disputed question—to leave nothing unsettled.

Almost in the other extreme we find M. Orfila: he saw, perhaps, the too easy confidence displayed by his elder contemporary, and determined to avoid that sort of feeling altogether. In consequence, he may be observed, in his anxiety to set a proper value on each isolated fact, neglecting to state what he conceives to be the combined value of the whole; and thus, by directing all his attention to the errors which medical men in their medico-legal capacity may possibly commit, by laying before them chiefly a chart of the bearings which they are *not* to follow, he leaves the mind in a sad state of uncertainty—unfavourable in a marked degree to the promotion of any description of knowledge.

The author of the work before us, M. Devergie, has rightly chosen to take the middle course. There are facts, he says, in legal medicine, as well as in practical medicine, and resting upon as sure a foundation. The diagnosis of a disease is never founded on a single symptom, but on a sufficient group of morbid manifestations: such also is the legitimate mode of arriving at results in forensic medicine.

In his introduction, M. Devergie pursues the analogy still further between the two kindred arts of medicine, properly so called, and forensic medicine; and in his practical way wishes to know why, when the importance of the study is unhesitatingly admitted by all who direct their attention to the subject—why there are not at least equal facilities afforded for the cultivation of each? "The student," he says, "who aims at being a mere practitioner, has the course clear before him: he repairs to the hospitals, where he sees with his own eyes, obtains experience through personal observation, and avails himself also of the experience of his masters: and thus he at length proceeds, equipped for practice on his own account. But this is not the case with the student of forensic medicine; all the instruction he receives in this department, as at present conducted in France, is by theory alone. He learns nothing practically: toxicology perhaps he may—that is to say, he witnesses a series of precipitations and chemical changes which pass rapidly in review before him; but when or where is he permitted to make a single actual experiment in chemistry, or to examine a person found drowned, or hanged, or asphyxiated? We assert that there is nothing so grievously wanted in the French schools as a medico-legal arrangement similar to that for teaching clinical medicine—where the student may obtain that *practical* knowledge without which he is unfitted for the due fulfilment of his mission."

How aptly does all this apply to the state of things among ourselves! How much more deplorably off, however, we are, even in respect to the theoretical opportunities alluded to by M. Devergie, we need not describe: we shall have some time to wait, we imagine, before we arrive even at the imperfect condition of medico-legal instruction complained of by our French friends.

But let us direct our attention to the volume. We are struck, in the first place, by the order of subjects adopted by M. Devergie. He affects, we perceive, in his Preface, to think order, in a work on forensic medicine, of no great consequence—of none, in fact; but we must here beg leave to differ with him: principles are to be taught in this study as well as in others belonging to the extensive range of medical science; and if age and sex, for example, constitute elements in determining identity, why should not the peculiarities of sex and age be treated of previously to entering on the consideration of identity? The peculiarities of sex, too, we presume, should precede the exposition of the doctrines respecting violation and pregnancy; and so with numerous other instances which might be adduced. But M. Devergie himself, it seems to us, *has* followed a certain natural order, at least after his first two chapters.

These relate to reports and formalities connected with French law, which are disposed of in the opening chapter; the second, which, by the way, is enormously long, occupying more than a third of the volume, contains all that relates to the dead—changes undergone in the dead body, the art of opening bodies, and the methods adopted in juridical exhumations. After this, rather abruptly, the scene changes to violation; but marriage follows; then pregnancy, parturition, and births. Infanticide comes next; then abortion, viability, and paternity, close in the rear; so that, perhaps, so far there is no objection to be made to M. Devergie's order.

In a work of this kind it would be unreasonable to expect an author to be wholly, or for the greater part, original. M. Devergie does not aim at it; he judiciously selects, for the most part, from every available French source. We have rarely met with any reference to an English authority; this, however, we can scarcely quarrel with; but that the many valuable German writers on the subject are not freely consulted is a fault—one, however, with which M. Devergie is not singularly chargeable; his countrymen generally seem to entertain a sort of national jealousy of their neighbours beyond the Rhine, if, indeed, their want of acquaintance with the German language be not at the heart of the mystery.

We should be glad to offer a specimen of the author's mode of treating his subjects, could we find any thing to extract of a moderately reasonable length. But probably we have said enough to give the reader a general idea of the production: we may return to it again when the second and concluding volume makes its appearance. That the second *will be* the concluding volume we almost have our doubts, when we recollect all the subjects that remain to be treated in it: unsoundness of mind, feigned and concealed diseases, wounds, and, above all, medico-legal toxicology. We shall not complain, however, if, at whatever length, these subjects be only discussed with the same ability displayed in the present volume.

Those who are curious about French law, as relates to all those questions on which medical evidence is given or required in courts of justice, will find it here fully set forth and clearly expounded.

Genera Plantarum Floræ Germanicæ iconibus et descriptionibus illustrata. Auctore TH. F. L. NEES AB ESENBECK. Fasc. I. ad VII. Bailliere.

THIS elaborate and elegant production we look upon as quite a boon for the botanist, it is published at so extremely moderate a price—each fasciculus containing twenty beautifully-executed plates, and forty pages of letter-press! The author's name is a sufficient guarantee for the accuracy and fidelity of the representations; and we are sure no votary of botanical science will be without the work. Among the minute and splendid dissections here figured, we may particularly notice those of nine genera of coniferæ, admirably illustrating Mr. Brown's view of the structure of the female flower. The anatomy of orchideæ (a most difficult though very interesting family) is delineated in seventeen plates, and comprehends dissections of no less than seventeen genera. Besides these two, the seven fasciculi now published contain other families, namely, betulaceæ, cupuliferæ, plantanaceæ, salicinæ, typhaceæ, acroideæ, aroideæ, junceæ, juncagineæ, veratreæ, sarmentaceæ, miricaceæ, juglandeæ, ulmaceæ, artocarpeæ, urticeæ, euphorbiaceæ, empetraceæ, eleagneæ, sontalaceæ, junceæ, siliaceæ, irideæ, amaryllideæ, colchicaceæ, lemnaceæ, fluviales, hydrocharideæ, alimaceæ, butomeæ, thymelaceæ, laureæ, chenopodiaceæ, and amarontbaceæ.

We strongly recommend the work on the ground of its exceeding merits: it is, moreover, adapted for the patronage and purchase of every class of botanical students — not only the tyro, but the adept and proficient.

MEDICAL GAZETTE.

Saturday, December 5, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

THE METROPOLITAN UNIVERSITY.

WE are glad to announce that every thing is now arranged for the establish-

ment of the long-desired central University in the metropolis. It will of course be styled the University of London; an appellation which, though usurped and grossly misapplied for some years back, through the interested and selfish eagerness of the managers of a proprietary school, will soon be restored to its right use.

Of the details of the charter we have before us an authorized statement, as well as a communication from the Chancellor of the Exchequer, explaining some minor particulars, regarding which it might be satisfactory to the public to have fuller information. Scruple about laying before our readers as much of these documents as we please, there can be none; for although the right hon. Chancellor only consented to have his letter *privately* circulated, one of the ministerial papers (the *Globe*) was the first to disregard the restriction, and it has since made its way into other public journals.

The following are the principal points worthy of notice in the official paper alluded to:—

"The school hitherto calling itself the University of London, is to receive a charter of incorporation, with the style and title of 'London University *College*.' Similar charters are to be granted to other institutions of the same kind.

"Another charter will be granted to persons eminent in literature and science, to act as a Board of Examiners, and to perform all the functions of the Examiners of the Senate House of Cambridge;—this body to be termed the 'University of London.'

"Pupils from University and King's College, and from any other bodies, whether corporated or uncorporated, hereafter to be named by the Crown, are to be admitted to examination for degrees on certificates of having gone through a course of study at those establishments, and having obtained a proficiency for a degree; the said pupils to be examined and classed according to their relative merits.

"The same conditions to be applied to degrees in Medicine.

"The degrees to be granted by *the* University are those of A.B., A.M., B.L., D.L., B.M., and D.M.

"The Crown to have the power of appointing new and additional Examiners from time to time.

"The Secretary of State to have the chief control in the formation of all bye-laws and regulations for the government of the University.

"The King to be visitor.

"Fees to be charged on degrees, for the remuneration of the Examiners; the amount to be regulated by the Treasury."

From Mr. Spring Rice's letter of November 22, we select the following passages:—

"It should always be kept in mind, that what is sought on the present occasion is an equality in all respects with the ancient Universities, freed from those exclusions and religious distinctions which abridge the usefulness of Oxford and Cambridge.

"These points (relative to the *subjects* of examination) cannot be included in the charter, as in that case great difficulties might arise in their future modification from time to time, as circumstances may require. But they will necessarily form part of those laws and regulations which would be framed by the University, with the approval and sanction of the Secretary of State, and be thus made subject to the parliamentary responsibility of that officer of the crown. The principle of the absence of all religious tests and distinctions must, however, be set forth distinctly in the charter, so as to preclude any possible doubt or alteration hereafter.

"It cannot but be expedient that Parliament should hereafter, when legislative authority is required, and the intentions of the donors do not preclude such a step, extend to these metropolitan degrees all the privileges and advantages (not of an ecclesiastical character) which are connected with degrees at Oxford and Cambridge. This, of course, applies to civil rights and professions, and not to private endowments.

"It is not proposed, at least at present, to connect with degrees any rewards beyond the classification for

honours of the pupils examined; but a provision will be made by charter for receiving endowments, the subjects for which such endowments are made being first approved of by the University.

"It will be most desirable, according to my views, that wherever collegiate or other institutions are found to exist, *whether in London or elsewhere,* that if such establishments afford to the public adequate security for good education, they should be included within the system created previously for the metropolis."

Such are the chief particulars relative to the new foundation which we think most deserving of notice: others of a minor nature we are obliged, in our limited space, to omit: they relate merely to the number (as yet unlimited) of the Examiners,—the possibility of assistant-Examiners, "for the techinal purposes of examination," being occasionally called in,—and the propriety of adopting a written form of examination, as far as it may be practicable. We have no hesitation in saying that on the whole we consider the arrangements to be at once conceived in a spirit of strict impartiality, and likely to afford a high degree of satisfaction to the public.

All the existing respectable establishments—the general schools and schools of medicine, both in London and the provinces—as it seems to us, will be materially benefitted by the new arrangement. One alone must feel peculiarly situated—must feel sorer than it pretends. The Gower-Street school, in fact, in two or three respects, can scarcely be accounted a gainer by the change. In the first place, it is obliged to resign, however reluctantly, what Mr. Tooke calls "the vanity of its title:" the self-imposed and tenaciously maintained appellation of University of London is henceforth to be appropriated to the legitimate body which it is to designate. So far the school in question loses the opportunity of flourish and puffery, which is carried on to so very considerable an extent—with what success, remains to be seen. Then, again, by the settlement of the question—of providing a Metropolitan University, to accommodate the Dissenters, and persons of every religious persuasion—the struggle being now over—the public can no longer be importuned by the ever-obtruded claims of a joint-stock proprietary, keeping up an eternal jangle of discord, and contending for the possession of rights to which they were as much entitled as any of the islanders in the South seas. In addition to this, the said joint-stock establishment—having henceforward, *perforce,* to settle itself down in the same rank with other similar metropolitan institutions—will have to rest its pretensions to the favour of the public solely on its merits and its good behaviour. Upon all these several grounds we think the friends of order may congratulate themselves on the change that has taken place.

It is rather amusing to observe the tone and manner in which the new order of things has been received in Gower-Street. First, in the course of the summer, or rather just in the advertising season, out comes Mr. Tooke with his blustering appeal, in high dudgeon, upon hearing from the Attorney-General that the *proper* charters were ready. The ludicrous production of the learned solicitor we had occasion to examine some time since, and to appreciate at its due value. Next forth comes the *Senate* (save the mark) with an address to the *Council,* smoothing down all difficulties, reconciling, or attempting to reconcile, inconsistencies, and, in short, proving to the *satisfaction* of the proprietors (for whom the address was evidently principally intended), that the projected metropolitan Univer-

sity was just the thing wanted, and that it would have been a most foolish measure to grant to such a school as *theirs* the exclusive power of granting degrees in London.

Finally, a special meeting of the proprietors (on Wednesday last) takes place, when, after much speech-making, —Lord Brougham in the chair—a resolution is adopted to this effect:—" That his Majesty's Ministers, *having discovered a plan* for conferring academical degrees more comprehensive and efficient than that originally contemplated, by extending to all qualified colleges for education equal facilities for obtaining degrees, this meeting, &c. recommends the Council to *accept the collegiate charter* offered." Thus, with a *good* grace (and same time knowingly enough), have the parties determined to make a virtue of necessity; they have struck the bargain, intending, no doubt, to make the best of it; and so, of course, ends the history of that once famous establishment, " The *Gower-street* University of London."

The above-mentioned " Address of the Senate to the Council," we looked through with much curiosity—a feeling which its perusal scarcely served to allay. The authors of it must be allowed much credit for the skilful manner of its composition; it has been successful, too, it seems, in making converts. Lord Brougham himself said that *he* was converted by it; but for our part, its principal effect upon us was to augment rather than to allay surprise. A flood of new light, we found, had burst upon the Senate; the mist was taken from their eyes; they now saw what they could never see before; the whole system, as now laid down by government, they find to be the best that could possibly be devised; a central body conferring degrees under the crown is a thousand times preferable to having the show of such a power entrusted to their own exclusive possession; the degrees themselves will be far more valuable, and the Professors will be all the better for having an independent examining body to deal with their pupils. In fine, the burthen of their song is congratulatory; they now discern the advantage of being subordinate to a body under the control of government; and seeing all this, they cannot but wish the Council and the proprietors joy that they did not succeed in that plan of exclusiveness and monopoly which they so long and so ardently yearned for.

The sum of all this would be very amusing and satisfactory, were it not somewhat marred with a strange attempt to prove that there is no inconsistency whatever in the present proceeding; that neither they, the Senate, nor even the body of proprietors themselves, in taking what is now offered them, compromise any principle formerly advocated: the amount of which is, that they hold the same doctrines now that ever they did. How curious, then, that they have never had the faculty of speaking out their mind intelligibly before! But it is too absurd to waste a word of argument with such wayward opponents,—opponents, besides, they can hardly be considered now, when they have just adopted all the opinions we have been endeavouring to impress upon them for some years past. One only question we will put to them ere we lay aside our pen: if they always thought as they do now, what, in the name of wonder, could have seduced them into the expenditure of hundreds and hundreds of pounds—which of late they could ill spare—to fee lawyers and functionaries usually employed in the procuring of charters? What was the burthen of all Dr. Lushington's arguments last year

before the Privy Council, when the self-styled University sought to make good its claims to certain privileges, in the teeth of all the combined colleges and schools of England?

LIBERAL ENCOURAGEMENT OF MEDICAL REFORM.

It would appear from sundry announcements, bearing the signature "John Epps, M.D., Hon. Sec.,", that there exists in London, or elsewhere, a medical reform association—so called.

We express ourselves thus doubtingly about the matter, because no one of whom we have inquired every heard of the society, or knows any thing about it. However, about a year ago, an advertisement was inserted in the *liberal* journals, offering 100l. for the best essay on medical reform; competitors being directed to forward their papers by a particular day, after which the "Committee" were to enter upon their duty of awarding the premium to the successful essayist. The time accordingly arrived, and all—that is, all who had sent papers—were curious to know the result. Time, however, passed on, and no announcement of the proceedings of the Committee was made. At last (about a fortnight ago) there was given to the world, through the same respectable channel, a notice from "John Epps, M.D., Hon. Sec.," informing all whom it might concern, that the Committee had decided that none of the essays were good enough to deserve the reward. The hundred pounds is therefore to remain—where it was! The names of the committee of humbug who have thus juggled the competitors have not been published, but the Treasurer is said to be no other than Joseph Hume, M.P.; in which case the prize-money (if such there really be), is obviously in very safe keeping.

KING'S COLLEGE.

CHAIR OF MATERIA MEDICA.

About three weeks ago we stated that Dr. Paris had been duly appointed to the chair of Materia Medica in King's College, but that Dr. Webster, who was lecturing for his friend, Dr. Bisset Hawkins, was for the present to continue his duties. On the same day that this information was given to our readers, the Lancet very wisely announced that Dr. Paris had declined the chair, and intimated that Dr. Webster was to be the new Professor. But having become aware of his mistake, by what appeared in the Gazette, he proceeds last week to correct it accordingly; instead, however, of limiting himself to the simple truth, he instinctively *embellishes*, and adds as facts certain inferences of his own, deduced from our account of the matter. Thus, speaking of King's College, he says, "Dr. Webster has been used very ill at this institution. We are authorized [*qu.* by whom?] to state that he has received sudden notice to quit the chair," &c. Now we *are authorized by* Dr. Webster *to state*, not only that such announcement was made without his authority, but that there is not one syllable of truth in it. He has received no "notice to quit," suddenly or otherwise, but is to lecture till the end of January, so as to complete the first course of Lectures, just as originally proposed. He did not offer himself as a candidate, in opposition to Dr. Paris, and has nothing whatever, in the shape of being "ill used," with which to charge King's College. So much for the Editor of the Lancet being "*authorized!*"

DEATH FROM QUACK MEDICINE.

MORISON'S PILLS.

Another exposure has been made of the dangerous consequences of taking those pills. Were it not for the occasional inquests which are held on the bodies of per-

sons who are tempted to use them, the public generally would know little or nothing of their deleterious effects. Probably many a death from the same cause occurs in private families—even under the eye of the regular practitioner, called in when too late—but the matter is hushed up from a dread of publicity. There is, however, no other way of ridding the world of quackery than by exposing its criminal ignorance and wickedness. We would suggest that it is the duty of practitioners to publish every case of the kind that falls in their way, whether publicly investigated or not. The names of the victims need not be divulged, yet the cases may be perfectly authenticated.

On Friday evening, last week, an inquest was held at the Sovereign, Munster-street, Regent's-park, before Mr. Stirling, on view of the body of Mrs. Sarah Porter, aged 35, of Clarence-gardens, Regent's-park.

The first witness was Dr. J. Johnson, who deposed that he attended the *post-mortem* examination of the body, and was of opinion that death was occasioned by inflammation of the intestines, brought on by some irritant substance taken internally.

Mr. Beale, surgeon, of Long-acre, deposed that he had attended the deceased several times. She was a very delicate woman. On the evening of the 20th inst. witness was requested to attend her. On his arrival he found that she was in a state of delirium, and incapable of articulation. On inquiry, witness ascertained that the deceased had taken several doses of "Morison's pills." She continued to get worse, being violently affected; on the 22d she became somewhat sensible, complaining of violent pains in the bowels; on the 23d all the symptoms became aggravated, and she died on the evening of the same day. Witness concurs in the opinion of Dr. Johnson, that the death of the deceased was caused by inflammation of the intestines, brought on by the administration of some violent medicine.

Sarah Paine, servant to the deceased, proved having purchased three separate boxes of "Morison's pills," at No. 43, Munster-street, which were taken by her mistress in doses of 14 and 16 at a time.

Other relatives of the deceased were examined, who stated that the deceased was advised to take the pills by a Mrs. Robinson, a neighbour; but she (deceased) becoming worse, they advised her not to take more.

The jury severely animadverted on the sale of the pills in question, and, after some discussion, returned a verdict, "That the deceased died from inflammation of the intestines, caused by taking Morison's pills."—*Times* (Nov. 28.)

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, November 24, 1835.

HENRY EARLE, ESQ., F.R.S., PRESIDENT, IN THE CHAIR.

WE take this opportunity of laying before our readers an abstract of Mr. Perry's paper, to which we were only able to allude in our last report.

History of a remarkable Case of Varicose Aneurism, with Observations, by J. G. Perry, Esq. Surgeon to the Foundling Hospital, and to the St. Marylebone Infirmary.

The author begins by referring to M. Breschet's researches on the subject (Mem. de l'Ac. de Med. iii.) and to Dr. W. Hunter and others, who seem to think that a communication by an artery and adjacent vein can only take place in consequence of a penetrating wound. Veins are not readily affected by the ulcerative process even of all the parts around them; and it would naturally seem unlikely that an opening could be made in their coats by the ordinary causes of absorption. Aneurisms have absorbed all the structures in their vicinity excepting their accompanying veins; but the case here related will show that the general law (if it be one) is not without some exception.

Jon. Allum, æt. 47, had been a dragoon early in life, but left the army in 1819, discharged for ill health. The only accident he could recollect having happened him was his having once been knocked down along with his horse by the fall of a heavy piece of timber. His left knee was hurt, and ever afterwards was weak and sometimes painful. In 1831 he first perceived a small swelling below the knee, but it gave him no pain, and he disregarded it. It increased in size slowly, but latterly was at times so painful as to oblige him to discontinue his labour, which was that of drawing a truck. The swelling had existed for two years, when his wife perceived a palpitation in the middle of the left thigh, resembling one of the heart to which he was also subject. It was, however, disregarded, but went on increasing simultaneously with the swelling below the knee.

On the 6th February, 1834, he applied

at the Infirmary for relief, and was admitted. An aneurism was then found to exist at the upper and inner side of the calf of the left leg, apparently occupying the lower end of the popliteal, or commencement of the posterior tibial artery. The pulsation in the thigh, along the course of the femoral artery and vein, could be distinctly *seen* as the patient lay in bed: it was about two inches below the crural arch: it was attended with a *purring* or *whizzing*, distinct from the beat of the artery, for the peculiar purr continued without intermission. Various opinions were given of the phenomenon by different observers: ligature was thought inexpedient, as the vessel was considered to be unsound even within the abdomen.

From some observations which he made, the author fancied he detected a communication between the artery and vein. The recumbent posture was enjoined, and a bandage from the foot to the groin, for the purpose of pressure, was tried, but discontinued in consequence of the uneasiness it occasioned. A sort of truss was found most likely to do good.

After nine months' residence in the hospital, he was permitted to go home, at his own urgent request. He was there visited, and the disease made no remarkable progress until September last (1835), when he applied for re-admission. In three days the tumor in the ham had increased to three or four times its former size, with an appearance of sloughing on its most prominent part. Operation was the only chance, and it was accordingly performed in the usual way. Upon opening the sheath of the vessels, the artery was found nearly as large as the abdominal aorta, and its coats so thin (and fragile also) as rather to resemble a vein than an artery. The ligature was accordingly very cautiously applied, with just sufficient tightness to stop the pulsation in the sac and lower part of the artery.

On the 5th day after the operation hæmorrhage suddenly took place from the wound, inducing syncope. This was temporarily remedied; but the patient sunk exhausted on the 6th day.

Dissection showed the left ventricle of the heart hypertrophied. The abdominal aorta was rather thin in its coats. The external iliac arteries (the left particularly) were singularly tortuous, and reflected on themselves in their passage to the crural arch. Immediately below the origin of the profunda the femoral artery appeared to have been affected with considerable aneurism; there was an opening observed in the sac from which the fatal hæmorrhage came.

At the point where a communication between the artery and vein had been suspected, an aneurismal sac was found, firmly ossified within, and which, by pressure, had formed an opening in the adjacent vein, into which the aneurism had burst. Below the aperture the vein was obliterated at a single point, but below that was again pervious. The aneurism at the ham was also examined, and a preparation made of all the diseased parts.

The author concludes with some remarks on the extreme infrequency of sloughing of the veins from pressure, or surrounding ulceration. He also observes, that the case just detailed confirms Breschet's remark, that the thinning of the arterial coats, and the dilated state of the vessel above the opening of communication, are consequences of the long duration of the varicose aneurism.

LECTURES

ON THE

DISEASES OF THE NERVOUS SYSTEM.

By M. Andral.

*** The portion of M. Andral's Lectures which treats of Diseases of the Nervous System, is in course of publication in Paris, with the approbation of the learned Professor. Should they prove of a nature to interest our readers, we shall give them in some of our ensuing numbers, stripped however of all redundant phraseology: meantime we subjoin the Introductory discourse, which contains some good general remarks upon the subject.

General Observations—Sources of Difficulty in the study — Insufficiency of our means of Investigation — Imperfect assistance derived from Post-mortem Examinations — Idiosyncrasy and particular Conditions of the Nervous System — Effects of Imagination — Elucidations afforded by Comparative Anatomy—Plan of the Course.

The study of diseases of the nervous system is rendered difficult, owing to the multitude of facts which it is necessary to collect being scattered through various books and periodical publications. We have not as yet any work which has embraced all the diseases of this system considered as a whole; and the difficulty of the study is still further increased by the diversity of the opinions which have been deduced by each observer from the facts which he has witnessed, and between which it is necessary for us to make our choice. In devoting ourselves to this laborious study we are led to the conclusion, that the science is, in truth, still in a state altogether initiatory: it as yet falls

far short of that which relates to the diseases of the other systems.

I shall enter upon some details which will make you better appreciate the truth of this remark. In every disease we have necessarily to study the causes, the symptoms, the anatomical lesions (of which the symptoms are often the effect), and, lastly, the treatment. But when we turn our attention to the affections of the nervous system, we find various circumstances which make the study of their causes, their symptoms, their anatomical characters, and their treatment, present difficulties which are not met with in the diseases of other parts. Let us pass in review those different orders of difficulties, and first let us take the causes.

Here, as in all other diseases, the causes are to be found, 1st, in the external world; 2ndly, in the reciprocal action of the organs upon each other; 3rdly, in the nature of the functions of the diseased organ, and in their modes of performance.

The nervous system is not influenced in a very remarkable manner by external agents; there is no comparison, for instance, in this respect between it and the systems of respiration, digestion, &c.

The second order of causes exhibits a very powerful influence on the production of diseases of the nervous system. Every organ which is deranged in its mode of action—reacts upon the nerves. There can be no disease in which they are not of some account, sometimes in a secondary manner, but often to such extent as to deserve to be placed in the first rank. It is thus that certain primary conditions of the liver, the lungs, or the intestines, in their progress become accompanied by nervous affections, which give them a specific character.

With regard to the third order of causes, every organ derives a source of disease from the nature and mode of action of its functions; but to the nervous system belong the intellectual and moral phenomena which constitute a cause of numerous diseases proper to this system; and as these phenomena of intellect and morals are not accomplished in the same manner in different individuals, but are modified by the diversities of position in society, by different ages, by professions, by religious opinions, &c. it follows that we have corresponding differences in the aspect of the same disease. For example, in private and hospital practice we observe that individuals have the nervous system affected in a different manner. And as the ideas of each cycle have their particular impress, (religion, for example, which was carried to excess in the middle ages, has given place by degrees to ideas of a different kind,) we ought not to be astonished that certain maladies which were developed under the influence of fanatical excitement, are no longer to be met with in our day, such as the epidemics of St. Vitus's dance, religious ecstacy, &c.; whilst, on the other hand, there are some now prevalent which were not formerly observed.

Next, of the symptoms; and here we encounter serious difficulties, which, for the most part, do not present themselves in the diseases of other organs:—1st, There are some which depend on the insufficiency of our means of investigation. Our senses cannot be made use of here in the same manner as in the diseases of the thorax or abdomen, for example, the diagnosis of which has attained so great a degree of precision. For the most part we only recognize diseases of the nervous system by induction; the difficulty is therefore immense, as compared to the others. 2nd. Other difficulties originate in the effect of premature theories which have embarrassed the study of these maladies. The theory which would attribute all the affections of this system to an increase or diminution of excitability, is inadmissible. There is something more than this, for assuredly numerous and varied disorders may be the result of a simple perversion or aberration of action. 3rd. Different lesions may give rise to similar symptoms: thus the same phenomena will often be manifested in two individuals, the one attacked with congestion, the other with anemia of the brain; so that under such circumstances it is less by the symptoms themselves than by the previous history that a diagnosis can be established. On the other hand there are lesions identical in their nature which may produce different symptoms, and this may depend on difference of situation, the gradual or rapid formation of the lesion, its extent, &c.: thus the derangement varies according as the cerebellum or brain is affected.

The brain ought to be considered as a complex organ formed by an aggregation of parts having different functions; but the localizations which have been attempted to be made are premature. We do not know the precise seats of the different faculties: these are questions for the future: and even were this diversity of organs to be admitted, it must ever be kept in mind that there is a connecting bond which establishes an unity in the vital principle. As to the rest, all these difficulties may be appreciated and subjected to certain laws; but there is one which is unknown, and which must not be left out in the account,—for it is a matter of fact: I speak of *idiosyncrasy*, in virtue of which the same action sometimes produces in two individuals phenomena so very different. Now

this idiosyncrasy plays an important part, particularly in the diseases of the nervous system.

Nor is this all. The sympathetic reactions become the source of prodigious difficulty, in the study to which I now allude. Sometimes we do not know whether the symptoms which we observe have their starting point in the nervous system, or some other organ; as in the cerebral fever of infants, &c. The symptoms of the diseases of the nervous centres present remarkable differences according to the period of life; so that two accurate observers, one placed in an hospital for adults, and another in an hospital for children, would give totally different descriptions of disease, and yet their facts might be perfectly accurate,—only they would have observed individuals of different ages. Here, then, are difficulties of the greatest magnitude, and which are not met with in the diseases of other parts.

But to continue. When we have determined the nature and seat of a disease, we have not yet done all; we must next inquire into the anatomical lesions. Undoubtedly pathological anatomy has within the last few years discovered some alterations of the nervous centres which were not known to us before. Thus *softening*, scarcely admitted by Morgagni, has been the object of valuable researches by Lallemand, Rostan, and others.

The cases in which some appreciable change is discovered by the anatomist after death are the most common with respect to other organs, while it is just the reverse with the nervous system: the cases wherein lesions are discovered are by much the most rare. This fact would appear a paradox if we only directed attention to the three or four diseases of the nervous system observed in hospitals; but it is by hundreds that we count nervous affections, and to cite only the principal of the neuroses implicating the powers of motion, sensibility, intellect, &c. where, let me ask, are situated the lesions in them? either we find none, or those we do find have no relation to the phenomena previously observed. Are we therefore to say that none exist? We *see* nothing certainly, but it is probable that there is organic lesion nevertheless, because there is lesion of action. However, we are not yet at the end of our anatomical discoveries: let us wait in hope. It has been said of late years that disturbances of the intellect are connected with alterations of the grey cortical substance. The fact has been disputed, but may, perhaps, at a future period be definitively ascertained. There are other researches, however, besides those which belong to the knife. Thus certain disturbances may be connected with changes in chemical composition resulting from various circumstances. Accordingly, of late it has been found that from infancy to old age, there is a progressive diminution of the watery elements, and increase of the albuminous. Phosphorus is at its maximum quantity in adults, &c.;—but these, again, are questions for future investigation, for the facts are not yet perfectly established.

We have thus seen the difficulties connected with the study of the causes, the symptoms, and the anatomical characters: there are others which are furnished by the treatment. In all the other organs, when we observe signs of irritation or excitement, we infer, as a necessary consequence, that there is a phlogosis, requiring the use of antiphlogistics, but with respect to the nervous system it is in many cases quite otherwise. Thus symptoms of excitement may exist; you bleed, and the symptoms become aggravated in proportion as the individual is reduced: now these morbid conditions which depletions increase, may exist along with other diseases which would require the abstraction of blood, but which is here contra-indicated; for the state of the brain after the bleeding would be more formidable than an inflammation of the lungs, of the intestines, or even of the peritoneum. And by a remarkable coincidence, this state, which is so intolerant of bleeding, is sometimes produced by the loss of too large quantities of blood: under the influence of preparations of iron and antispasmodic remedies it disappears, and harmony is restored by those means in cases where depletions would certainly augment the disturbance.

There yet are other means from which we can derive prodigious assistance; namely, those which act on the imagination. Certain diseases are produced in those subjected to the influence of strong passion, or religious enthusiasm;—and such maladies may be cured by means analogous to those by which they have been produced. It may be remarked, too, that it is not simply nervous affections, but various other diseases, which have a tendency to become organic, which admit of being thus removed. Thus it is the effects produced by a lively faith in the *baquet* of Mesmer, and the various trickeries of magnetism, as well as the infinitessimal fractions of homœopathy, all of which have a great effect on the imagination, are among the sources whence great assistance has been derived.

It has been said that in proportion as the sciences have advanced they have always become associated at a certain elevation; and, accordingly, when we study the maladies of the nervous system, the possibility of having light thrown upon the subject by other branches of knowledge becomes apparent to us. Dissections con-

nected with this investigation require to be of the finest and most minute description: for instance, there are certain diseases of the cerebellum which produce blindness in infants; and the point to be investigated in such cases becomes—what is the relation between this part and the optic nerves? There is a connexion, in truth, with the tubercula quadrigemina by the processus cerebelli ad testes, from whence the second pair of nerves derives some of its roots. In certain cases the power of motion has been seen to continue in the lower extremities notwithstanding that the spinal marrow was almost entirely interrupted, a small shred of the slenderest kind being the only medium of communication between the upper and lower portions; or even there may have been nothing but a little fluid interposed, and then it has been a question how it came to pass that the functions were not entirely suspended? Comparative anatomy has stepped in to our assistance, and has shown us that in those cases where the spinal cord is no longer an organ of sensation, it has but a very small volume. It has further shewn that in certain fishes, the nerves, though separated from the cord by a liquid, are nevertheless fitted for transmitting the power of motion. To these beautiful results experimental physiology has added its facts, and has rendered immense service to pathology. It is thus that certain kinds of paralysis of the face, where sensation remains entire, and *vice versâ*, have been so well explained since the discoveries of Sir Charles Bell; and that M. Magendie in particular has placed it beyond a doubt that lesions of the posterior portions of the spinal marrow give rise to disordered states of sensation, and those of the anterior portions to disturbance of the powers of motion. There are cases in which an individual has been known to become blind, and deaf, and to lose the sense of smell, without any alteration in the corresponding organs—the eye, the ear, and nostrils, or in the optic, acoustic, or olfactory nerves; and such facts were placed among the number of those on which pathological anatomy had thrown no light. However, it has been very recently proved that in man the fifth pair is accessory to these very functions—sight, smell, taste; while in certain species of animals, comparative anatomy has demonstrated this same pair to be the principal nerve of those senses. Guided by this knowledge we have been led to inquire whether in certain cases of deprivation of sight, hearing, &c. there were not some lesion of the fifth pair, and sometimes such lesion has been found there, and there alone.

But pathology returns what it borrows: if it receives the light of the sciences above mentioned, it becomes in its turn a fertile source on which the anatomist and physiologist may draw for aid in elucidating many points. It is therefore not without reason that it has been said, that the sciences, in proportion as they advance, tend at a certain elevation to the improvement of each other.

The order I shall follow on the present occasion is the same I usually adopt: we shall consider—

1st, The diseases of the nervous centres;
2nd, Those of the cords;
3rd, Those of the great sympathetic.

The other subdivisions will also be the same, and will comprise—

Lesions of circulation.
—— of certain secretions (cerebral œdema.)
—— of nutrition (*a.* augmented; *b.* diminished; *c.* perverted.)
—— of inervation; that is to say, of functions (intelligence, sensation, movement.)
—— of inervation of the organic functions (digestion, generation, secretion.)
—— of the forces which direct and regulate the laws of organization, and which constitute the vital powers (the sthenic, asthenic, and ataxic conditions.)

In this course we shall have to study facts, some of which are demonstrated, and some which, though not demonstrated, are yet probable: we shall find others which are reputed false and inaccurate, but which we shall not reject without investigation; for certain facts which have been reputed false at first, have after a time been established, while the favourite theories which had led to their rejection, have themselves been overturned.

We shall also consider certain theories, because theories connect facts together, and on that account become interesting, however unstable and short-lived in themselves. We shall be careful in rejecting hypotheses too, because what was hypothetical yesterday has sometimes become matter of demonstration to-day.

There are some questions which I shall content myself with merely stating: choosing the middle course between that exaggerated scepticism which annihilates every thing, and a too easy credulity which inclines to believe every thing: the kind of doubt which I recommend is not scepticism, but is one essential to science, and to those who cultivate it.

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LECTURES

ON

SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

BY DR. LATHAM.

ON SYMPTOMS.

Auscultatory Symptoms—Crepitations, or Moist Sounds, that attend the Act of Breathing—Large and Small Crepitations—The Distinction obvious and useful in its main Characteristics; uncertain and useless in its lesser degrees—Crepitations the most frequent of all Auscultatory Signs—What they can, and what they cannot, teach—How they need other and more general Symptoms to interpret their meaning—Acute Inflammation of the larger Bronchi; of the smaller—That Inflammation of the Bronchi which accompanies Diseases of the Heart—That which simulates Phthisis—That which is called Peripneumonia Notha—How much in each can be inferred from the kind and extent of the Crepitations—The Crepitation characteristic of Pneumonia.

THE sounds produced by the meeting and mingling of air with fluid in the bronchial tubes during the act of respiration, I have called Crepitations; and of Crepitations I have made one distinction only, large and small.

Between the largest of the large and the smallest of the small there are many intermediate degrees; and some of these may perhaps seem to deserve a name. But, for my own part, I always have had a sort of nervous horror of multiplying names, especially where things are essentially the same, and differ only in being greater or less; therefore I cannot bring myself to invent several names to designate different degrees in the present instance.

But though the extremes are far apart, yet in the midway large and small Crepitation will so nearly meet, that there must often be a doubt which is which; and what one man calls large, another will call small. Language, however, will afford no remedy where the difficulty is in the thing itself. You can only accurately distinguish things when they are some way apart, and not when they lie close upon the confines of each other. You can distinguish between light and darkness, but you cannot put a mark upon the boundary between day and night.

Large Crepitation arises from air meeting and mingling with fluid in the larger bronchi; small Crepitation from the same conditions in the smaller bronchi and the vesicles of the lungs. This is an important distinction, and I desire to make much of it;—important, however, in its main and prominent characteristics, but useless if it be refined into many degrees.

Without wishing to enter upon a criticism of nomenclature, I would further remark that, in giving names to auscultatory signs, we should take especial care that the names themselves do not imply any thing that is erroneous; and that they do not go beyond the truth, in pretending to designate that which they *certainly cannot* designate.

What I call "large Crepitation" is called by most French writers "râle muqueux," and by most English writers "mucous rattle." Call it râle, or rattle, or Crepitation, or what you will; but pray do not add "mucous" to it by way of specific difference; for this term must always seem to imply that the sound is produced by air passing through *mucus;* whereas it is produced equally by air passing through mucus, or pus, or blood, or any fluid whatever. Besides, it is beyond the truth to say that the *quality* of the fluid through which the air passes *can* be distinguished by the quality of the sound that results. The sound will indicate the *situation* and quantity of the fluid, and no more.

Therefore, by whatever name you choose to designate the moist sounds arising from the bronchial passages during respiration, be it Crepitation, râle, or rattle, you cannot distinguish it by any other epithets of more precise meaning than "large or small," without implying more than you intend, and that, too, something erroneous, or something beyond the truth.

Crepitation, or the sound which shows that the moisture of the bronchial passages is in excess, is the commonest of all auscultatory signs. Go round this hospital, and, out of the five hundred and more patients which it contains, you will probably find Crepitation in above fifty. And in all these it arises immediately from one and the same condition—viz. from excess of fluid in the bronchial tubes or vesicular structure.

But the fifty patients cannot, in any just sense, be said to have the same disease, because a single pathological condition is the same in all. Neither, on that account, will all be found to demand the same treatment.

I speak of disease and treatment in the largest and most comprehensive sense.

Now the fifty patients, who have all Crepitation of their lungs, are not suffering alike in *other respects.* And *these other* respects in which they differ, include the

great characteristics of their diseases, and the special indications of their treatment.

At present I am speaking only of *direct symptoms*, immediately referable to the part affected. When I come hereafter to speak of *indirect* symptoms, or those which, originating in one part, declare themselves through the medium of another; and of general or constitutional symptoms, the signs of the pulse, and of febrile and nervous affections in their various kinds and degrees; then I will endeavour to show that many diseases, apparently local, have a much larger range throughout the body, and that the treatment which is to compass their cure must be alike comprehensive in its influence.

Learn, however, all that is capable of being known concerning the *mere* part. Let the patient tell you how it feels amiss, and ascertain for yourselves how it acts amiss; and if it be a part within reach of the sight, the hearing, or the touch, make out what changes of structure it has undergone. Still its sensations, its functions, and its structure, will only half inform you what are the essentials of the disease it suffers.

Remember, there is a pathology of diseases *beyond* the part, as well as a pathology of diseases *within* the part; and that the things beyond it are really and practically the great interpreters of the things within it.

Remember, there is a great vascular system and a great nervous system, and that these, according to the manner in which they are actuated, assign a character to the local disease, and determine its treatment accordingly. Concerning the disease, they tell us what is its force of action, and what its rate of progress; and, concerning its treatment, they teach us to choose the remedies, and so to regulate their impression as to counteract this force of action, and to keep pace with this rate of progress.

Bearing these considerations in mind, you will be able to comprehend what I mean by saying, that the many patients in this hospital who have Crepitation as a common auscultatory sign, and redundant fluid in the bronchi or vesicles as a common pathological condition, may nevertheless have different diseases. And, still bearing them in mind, you will now be able to follow me as I run over a few prominent distinctions.

Among the many who have this auscultatory sign, in some it has endured for weeks or for months, and in some it has sprung up since yesterday. In some it is accompanied by much fever and great vascular action, and life itself seems already in peril, although it has existed but for a few days.

In some it is accompanied by less fever and less vascular action, and there is yet no peril of life, although it has existed for many weeks.

This crepitation is present in Rheums and Catarrhs, and Chronic Coughs, which cleave to old people, from the end of autumn to the beginning of spring, with little or no fever.

And it is seldom absent in Chronic Diseases of the Heart; and here it is found sometimes with and sometimes without fever, or any signs of inflammation; as if the bronchial surface had the power of *simply* increasing its secretions for the relief of a burdened and baffled circulation through the lungs.

Crepitation also is a frequent accompaniment of pulmonary hæmoptysis, with or without fever.

Now, while the great constitutional symptoms are our paramount guides to the knowledge and treatment of the disease as a whole, it is to *this Crepitation* that we are to look, in each particular case, for the knowledge of what the disease is in the lungs; its exact seat, its extent, and the stage of its progress.

The case is one, perhaps, in which there is much fever and great vascular action; while cough and dyspnœa, and some expectoration, denote the lungs to be the part upon which the inflammation has specially fallen.

We auscult, and discover Crepitation: and the Crepitation has one main and prominent characteristic; that it is large. It is *large*, and *large* exclusively; while through it, wherever it is heard, the respiratory murmur is also heard in every part of the lungs.

Such auscultatory signs denote the mingling of air with the matter of morbid secretion in the larger bronchial tubes, and in them exclusively; the lesser tubes and the vesicular structure of the lungs, the seat of the respiratory murmur, being entirely free.

But the much fever, and the great vascular action, declare this condition of the bronchi to be the work of the severest inflammation, and of inflammation that is still going on. Yet, while auscultation continues to show that such is exclusively the seat and limit of the inflammation, severe as it is, we are warranted in expecting a favourable result; provided always that we pursue a just treatment. For even the acutest inflammation of the larger bronchi is unapt to extend itself to other textures, or involve the structure of the lungs beyond those bronchi themselves.

But the case is one, perhaps, in which

there are the same constitutional symptoms bespeaking the disease to be inflammation, and the same local symptoms, fixing that inflammation upon the lungs. But withal, the Crepitation heard during breathing is *small*, and *small* exclusively; and this small Crepitation, wherever it is found, has entirely obliterated the respiratory murmur.

Such auscultatory signs denote the mingling of air with the matter of morbid secretion in the lesser bronchial tubes, or the vesicles of the lungs.

In the former case, the disease was acute inflammation, and it is no more than acute inflammation in this. We have the same means of subduing inflammation in this which we had in that, and the same plain indications to direct our treatment; and the disease, *in its own nature*, is as amenable to remedies *here* as it was there. Yet are we *not* warranted in forming the same expectation of a favourable result. Because inflammation of the lesser bronchi, unlike that of the larger, is ever ready to pass beyond them to other textures, and to involve the whole structure of the lungs.

Where you have small Crepitation one day, you may find that it has entirely ceased the next; and ceased, not to be replaced by the respiratory murmur, but by absolute dulness and total obliteration of sound.

And what is implied by this rapid change from small Crepitation to total obliteration of sound? Even this; that the bronchial ramifications and vesicular structure are so pressed upon, from within and from without, by the effused products of inflammation, by serum, or lymph, or pus, or blood, or a mixture of all, that air cannot enter, and the lung has become solid at that part, and may possibly have undergone irreparable disorganization.

Let it not, then, be esteemed a small thing, that, upon rational grounds, and by tokens which we can justify and explain, we are able to arrive at this diagnosis; that, in the severest inflammation of such an organ as the lungs, we can mark the cases which are within the probability of a perfect cure, and those which are beset with the most perilous hazards.

To distinguish between inflammation of the larger bronchi and inflammation of the smaller, or of the vesicular structure, is still important in all its degrees, and whether it be chronic or acute; and auscultation will always enable us to do so.

Chronic inflammation of the larger bronchi, after months and years, is still reluctant to extend itself to other structures; whereas, chronic inflammation of the lesser bronchi is alway ready to spread beyond its original seat.

The most frequent instances that I meet with, of inflammation, slight in degree and chronic in duration, affecting the lesser bronchial tubes and producing effusion into them, are those in which it accompanies chronic disease or disorganization of the heart. It is evidenced by small Crepitation proceeding from a considerable space at the lower part of one or both lungs, and often continuing to be heard for months and months together. Thus, even for months and months, the lung may remain quite pervious, but crepitating. Yet there is no security, in the meantime, that what is pervious and crepitating to-day, may not be absolutely impervious and dull to-morrow. In point of fact, I have known, by the test of auscultation, nearly a whole lung become condensed and solid in the course of a single night, when there has been nothing to give warning of such a catastrophe.

Indeed, in the dissection of those who die of diseased hearts, we are accustomed to find the lungs generally loaded with serous and sanguineous effusions, while some portions of them are solid and impervious, and sink in water.

But chronic inflammation of the larger bronchi, after months or years, is still (I have said) reluctant to extend itself to other structures. After months, or years, auscultation still finds it in its original seat; the air bubbling through a thickish, copious, puriform fluid, and producing the truest form of large Crepitation.

There is a form of Chronic Bronchitis, in which all the conceivable signs of Phthisis are present except the auscultatory: emaciation, hectic fever, cough, and a copious thick yellow globular expectoration. Yet the chest sounds well every where upon percussion, and the auscultatory sounds are *purely bronchial*, and nothing more, and proceeding from the bronchi in their first divisions, and not beyond them; large, not small Crepitation; but large Crepitation widely diffused, and permitting the respiratory murmur to be heard every where through it.

Here the *larger* bronchi alone are inflamed, and filled with the matter of morbid secretion, while the lesser and the vesicular structure are free. There is, moreover, no cavity.

It would be beyond the power of the most sagacious physician upon earth, *without the help of Auscultation*, to distinguish this case from a case of phthisis; but by *such help* we not only determine this disease, which is so like phthisis, to be no phthisis at all, but we pronounce it curable—that is, curable in its nature, although from circumstances not always cured.

Cases of this kind—cases of chronic bronchitis, in which all the conceivable

signs of phthisis are present except those which indicate vomicæ—are not common. I see a few of them in the course of the year.

They are more frequent, to my experience, in the hospital, than in private practice. The disease usually begins in a catarrh, which, from neglect or unavoidable exposure, is aggravated into a similitude of phthisis. Under this similitude it may endure even for a year or two, and becomes difficult of cure in proportion to its continuance.

But, where there is Crepitation, it need not necessarily be of one kind only. In the same patient you may hear at one part of the lungs a Crepitation like the tracheal rattle of the dying; at another, like the bursting of large bubbles on the surface of water; at another, like the crackling of salt thrown upon hot embers.

There is a disease which was first called by Sydenham *Peripneumonia Notha*, and which is still known by that name. It is, in fact, a diffused inflammation of the bronchial tubes, chiefly incident to old people. One of its peculiarities consists in the enormous secretion that is poured forth from the mucous surface; and another which I have remarked is, that the inflammatory symptoms often still remain, nay, often continue to increase, after the secretion is freely established. This latter peculiarity (if auscultation informs me aright) is derived from the fact, that inflammation does not arise at once and simultaneously upon the whole surface which it is destined to pervade, but travels over it progressively; so that various portions of the same continuous surface are in different stages of inflammation at the same time. How possible this is, every one knows, who has watched erysipelas travelling over the whole body.

Cases of Peripneumonia Notha I recommend to you as special studies for auscultation: here you will often find, in the same patient, every modification of those sounds which are produced by the matter of morbid secretion mingling with air in the bronchial ramifications of every size, from the largest to the least.

The common opinion is, that old people die of Peripneumonia Notha simply because they have not power to bring up the large accumulation of phlegm; implying, that the whole disease is limited to the first divisions of the bronchi, and that if they were freed from obstruction, there is nothing beyond them, nothing in the condition of the lungs elsewhere, capable of producing death. Doubtless, it is quite possible that accumulated secretion of any kind in this situation may be the whole and sole cause of death: it may suffocate by its very quantity. But this I know that since I have had auscultation to help my inquiries, I have never seen any one perish of Peripneumonia Notha, in whom there was not elsewhere within the lungs enough to claim a large share in producing his death.

But I find myself speaking of Auscultation too much in detail for the purpose I have in view. In these lectures I would rather wish to give you such a notion of the general bearings of this and that subject, as will help you to follow it up for yourselves in the wards of the hospital. *Here* I am not so much striving to teach, as I am encouraging you to learn.

There is a Crepitation which consists of the *smallest* conceivable cracking—a noise like the crackling of salt thrown upon burning coals. This is regarded as the characteristic Crepitation of Pneumonia, because wherever it exists, it is always for a short period only; and it is quickly followed either by the return of the natural respiratory murmur, or by the absence of all sound whatever. In the one case the passage of air through the vesicular structure of the lung has again become free, and a resolution of the disease has taken place; in the other case the passage of air is altogether precluded, and the disease has passed into its next stage, and has condensed the lung.

There is a doubt in *what* manner this particular sound is produced, and *where*. Some conceive it to proceed from the structure of the lung exterior to the bronchi or vesicles, and to result from the tearing of parts asunder that have been united by effused lymph. It may be so; but the fact would be extremely difficult to prove. To my ear the sound is the same in kind with those which I have described by the generic term Crepitation. Of these it is the smallest in degree, and probably proceeds from the same continuous surface, from the extreme vesicles and air-cells of the lungs; and is probably formed in the same manner, by the mingling of air with the morbid secretion which it finds there. This little crackling sound (so well known to those whose experience has taught them to appreciate auscultation in its practical use)—this little crackling sound, reaching the ear from a limited and circumscribed space within the chest, marks the commencement of pneumonia. It is a *direct symptom*, having immediate reference to the structure of the part. And if we consider what the part is, and what the disease—the part the lungs, and the disease inflammation—we cannot too highly value this single symptom (simple and mean as it may seem), which gives the earliest and surest

intimation that such a disease has begun as tends to disorganization and the inevitable loss of life, unless quickly arrested by its counteracting remedy.

Further, auscultation, having detected inflammation of the lungs at the point where it begins, still follows it as it spreads; and it follows not merely its progress, but its processes.

The Crepitus commencing at a small space, and gradually reaching farther and farther, gives notice of inflammation gradually passing from lobule to lobule, and effusing lymph into their vesicles as it spreads.

Then the crepitus becoming fainter and fainter, but not replaced by the respiratory murmur; and the spaces in which it was first heard becoming dull, and larger and larger spaces becoming dull in succession, until not a sound of respiration, either healthy or unhealthy, proceeds from (perhaps) half the chest; these striking phenomena give notice how the vesicular structure of a whole lung is progressively obliterated by the continually effused lymph and morbid products of the still unarrested inflammation.

Thus far the intimations of the ear keep pace with the progress and processes of an acute attack of pneumonia. Auscultation pronounces the permeable lung converted into a solid mass, and admitting no air, save what may just enter and immediately return from a bronchus or two which still remain pervious. Beyond this point auscultation cannot go. But the disease may go further.

This is a painful period of suspense in every case of pneumonia, when a whole lung, or a large part of it, has ceased to admit air, and the patient still survives.

The disease may have gone further than auscultation can follow it. Auscultation only discovers that the lung does not admit air; that it has become solid from having been permeable. But its texture may be softened; its cohesion destroyed; and it may be reduced to a state of pulp and rottenness, which is irreparable.

If its texture be *not* thus disorganized, it is yet capable of reparation; and then, the inflammation having ceased, Auscultation beautifully takes up its part again, and gives the first notice of reparation, as it gave the first notice of disease. Crepitation again begins to be heard where there was no sound. Soon it becomes more extensive, and some vesicular breathing is mixed with it; and the respiratory murmur and the Crepitation seem (as if) contending with each other for the mastery, until the respiratory murmur is predominant, and then all is well.

And what is going on all the while within the structure of the lungs? Even this. The lymph within and around the pulmonary vesicles is gradually absorbed, and the air gradually finds admission within them. At first it is impeded by the extravasated fluid it meets with in its passage; but as the permeable texture of the lungs gets disentangled and set free, it glides through them unobstructed and alone, and with the genuine murmur of health.

REMARKS ON VACCINATION.

To the Editor of the Medical Gazette.

Sir,

Should you deem the following remarks worthy of a place in your valuable journal, I shall feel obliged by your giving them insertion.

The opinion generally prevalent that vaccination may be performed with impunity at all ages, has I believe been productive of unpleasant results in many instances, where from the too great confidence in the innocuous nature of vaccination, the causes of those results have greatly perplexed the practitioner.

My observation was first attracted to this subject from the loss sustained by three of my most intimate patients, in the death of their children, all of whom were vaccinated under six months, and who were subsequently attacked with pulmonary disease within ten days from the disappearance of the eruption, which not the most active antiphlogistic treatment was able to subdue. Another case having recently occurred in which pulmonary inflammation supervened upon vaccination without any apparent cause, has again brought the subject before my notice, and I now forward you a few particulars of the cure.

On the 19th of October, 1835, Miss C. aged six months, was vaccinated, being at the time in good health. The vaccination progressed regularly in two pustules on the left arm. On the right no pustules appeared. Up to the 11th of this month the child seemed to be going on favourably, when, on that day, she manifested uneasiness about the præcordial region, attended with considerable disturbance of the system. Her bowels were open, and her motions not deficient in bile. Free incision was made on the gums, though no appearance of dental irritation presented itself, she having cut the lower incisors with ease prior to the attack. Expectorants, combined with bloodletting and blistering, were resorted to without benefit, and the child gradually grew worse, notwithstanding this treatment was persisted in, by the concurrence of two eminent practitioners, until a short time of her death.

The post-mortem examination discovered large patches of inflammation in the upper and posterior portion of the left lung, with hepatization of considerable extent in the middle lobe on the right side. At each incision of the scalpel matter oozed out in such quantities as astonished others equally with myself, and which sufficiently proved the active nature of the inflammatory symptoms. The abdominal viscera were healthy. The liver large, but free from disease. The head was not examined.

The inspection of two children, who died under similar circumstances, some time ago, gave analogous results.

Three conclusions may be drawn from the aforesaid case, emanating from the following questions:—

First—How far the introduction of a virus, generally supposed innocuous, may be productive of such irritation in the system, as to produce in an organ previously predisposed, so much functional derangement as to occasion death?

Secondly—Is the vaccine lymph capable of arousing the dormant energies of a diseased organ into action, when introduced into the system by inoculation?

And, *Thirdly*—Does not the age of the child materially modify the deleterious effects of the virus in proportion as the child advances in life?

Some of your readers will probably favour me with their conclusions on the above questions. My opinion is, that vaccination may produce disease; and that that disease may attack an important organ, destroy its function, and consequently life; and that the strength and age of the patient in a great measure prevent [illegible] surmount the attack.

It is not with a view to discourage vaccination that I make these suggestions, for of the benefit to be derived from it I am too well aware, but rather to elicit further information respecting its indiscriminate employment in reference to its safe adoption.

I remain, yours respectfully,

AN INQUIRER.

[The name of the writer has been communicated to us: we must remind all correspondents that though *opinions* may be adduced anonymously, all statements of facts ought to have the authority of the writer's name.—ED. GAZ.]

PORTRAIT OF TIEDEMANN.

WE beg to recommend to the special notice of our readers the admirable likeness of Professor Tiedemann, just published by M. Schloss. The expression is faithfully preserved, and the whole performance (for we have also seen the original sketch from which the lithograph has been taken) is highly creditable to the artist.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN NOVEMBER, 1835.

William Thomas Callon, St. Helen's, Lancashire.
Charles J. Croft, Arthur Street, City.
James S. Chapman, Appleby.
Henry Marsh, A.
George Rowles, Dublin.
George Fearnley, Yorkshire.
Joseph Dickinson, Cumberland.
Edward Richardson, Preston.
Edmund Jones, Knastor, Hereford.
John R. Webb, Coltishall, Norfolk.
James B. Eames, Castle Martyr, Cork.
Thomas Firth, Liverpool.
John T. Tallent.
Alexander Bridge, London.
Thomas Chalmer, Liverpool.
John L. Burnard, Crewkerne.
Thomas Lavery, Manchester.
Thomas Burlton, Leominster.
George A. Place, Wimborne.
Joseph Dunn, Manchester.
Andrew Skene, A.
Henry Glasspoole.
William Gosse, Poole.
Arthur W. Dumvile, Manchester.
William P. Monk, Guernsey.
William H. Smith, Kennington.
Henry C. A. Clarke, Ramsgate.
George Lawdell, Brighton.
William W. Morgan, Pentraback, Glamorgansh.
James F. Fitzgerald, Dublin.
William Lord, Farringdon, Berks.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

December 3, 1835.

Thomas Ashmall, Lichfield.
Edward Pellew Davies, Newington, Kent.
Frederick Cripps, Wisbeach, Lancashire.
John Gaskell, St. Helen's, Lancashire.
George Ross, Hull.
Christopher Miles Spencer, Norwich.
Joseph Ward, Birmingham.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Dec.* 1, 1835.

Abscess	4	Heart, diseased	2
Age and Debility	52	Hooping Cough	6
Apoplexy	6	Inflammation	34
Asthma	10	Bowels & Stomach	1
Cancer	3	Brain	2
Childbirth	6	Lungs and Pleura	7
Consumption	51	Insanity	4
Convulsions	38	Liver, diseased	3
Croup	3	Measles	6
Dentition or Teething	3	Mortification	3
Dropsy	11	Paralysis	5
Dropsy on the Brain	4	Rheumatism	1
Dropsy on the Chest	2	Small-pox	18
Epilepsy	1	Sore Throat and Quinsey	1
Erysipelas	3	Spasms	3
Fever	10	Thrush	2
Fever, Scarlet	21	Tumor	1
Fever, Typhus	3	Unknown Causes	5
Fistula	1		
Gout	2		
Hæmorrhage	1	Stillborn	22

Decrease of Burials this week, 172.

NOTICE.

We have no room for the paper of the Apothecary of the North London Hospital, in defence of Dr. Elliotson's metaphysics. This is a sort of vicarious championship which we are not at all disposed to permit.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 12, 1835.

LECTURES
ON
MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XI.

THE MUSK ANIMAL.

IN my last lecture, I mentioned that the order of Ruminants was divided into those that had horns, and those without them. Having examined all the officinal horned Ruminants, I proceed now to notice the only hornless one, namely, the

Moschus Moschiferus, or Musk Animal.

FIG. 59.

History.—This animal was first described by the Arabian authors; Aristotle, Pliny, Ælian, and Oppian, make no mention of it. We are indebted to the Greek physician, Ætius, for its introduction into medicine.

Etymology.—Some derive the word μοσχος—παρὰ τοῦ ἐκ τοῦ μέσου χεῖσθαι (*because it flows from the middle of the belly*); others from μαω, *I seek*, this substance being sought after; some from μοσχος, *a calf*, on account of the supposed resemblance between the two animals. None of these derivations, however, are satisfactory; and it is far more probable that the word is of Arabic origin.

Description.—Observe, in the first place, that the animal is destitute of horns, and, therefore, is not, as some have termed it, a deer. There are 32 teeth, namely, 8 incisors in the lower jaw; 2 canines in the upper jaw, and 24 molars. The most important character, in regard to the teeth, is the projection (in the male) of the canines (fig. 60, next page) outside the mouth, in the form of tusks; in the female, however, they are not met with. The *stylocerus moschatus* (fig. 61, next page) forms the connecting link between the deer and the musks; having the horns of the one, and the canine teeth of the other. The tail of the musk is very short; the feet with hoofs; the hair coarse, varying in colour, but on the body it is for the most part dark brown. In front of the prepuce of the male is found the well-known musk bag.

Cuvier says, no other species of Moschus possesses a musk sac; but the correctness of this remark is more than doubtful. Eschscholtz has described as a distinct species, an animal under the name of *Moschus Altaïcus*, distinguished from the *M. Moschiferus* by a white stripe on the fore part of the neck. Brandt, however, regards it merely as a variety, under the name of *M. Moschiferus Altaïcus*. Two other species have been said to yield musk, namely, the *M. Napu* (the *M. Javanicus* of Pallas), and the *M. Jasanicus* or *Kanchil*. Mr. Martin, surgeon, of Islington, tells me he some years ago, when at Java, dissected one of the Javanese musks, and found a musk sac in it. He has the skin of the animal now in his possession, stuffed.

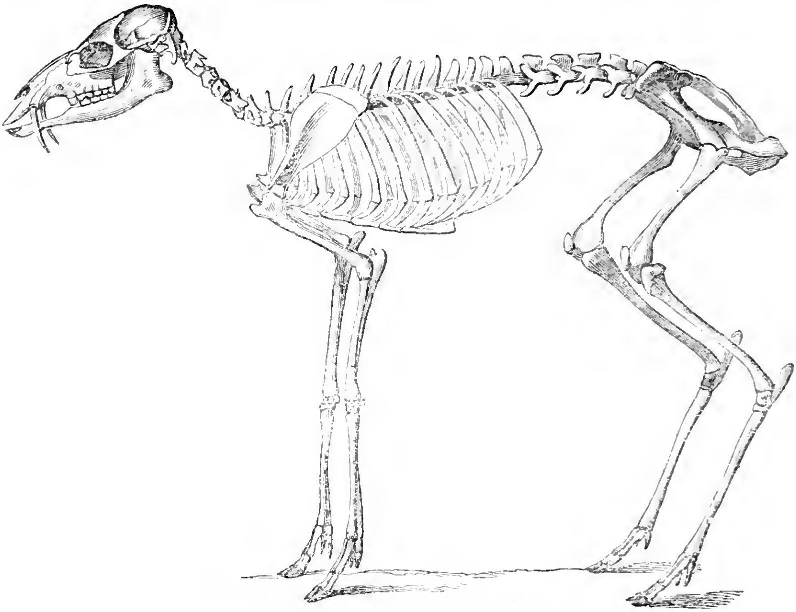


FIG. 60.—*Skeleton of the Moschus Moschiferus.*

FIG. 61.—*Stylocerus Moschatus.*

Anatomy of the musk sac.—When we examine the belly of the animal, we find in front of the preputial orifice (fig. 62, *i*, fig. 63, *d*), and behind the navel, a small aperture (fig. 62 and 63, *h*), leading into the cavity of the *musk sac.* This aperture (which I shall call the external musk aperture) is about half an inch from the umbilicus, and usually about a line or a line and a half from the preputial orifice. In this preparation, [shewing it] however, the distance from the latter is greater. The preputial orifice is somewhat more prominent, and has a number of longish hairs projecting from it, in the form of

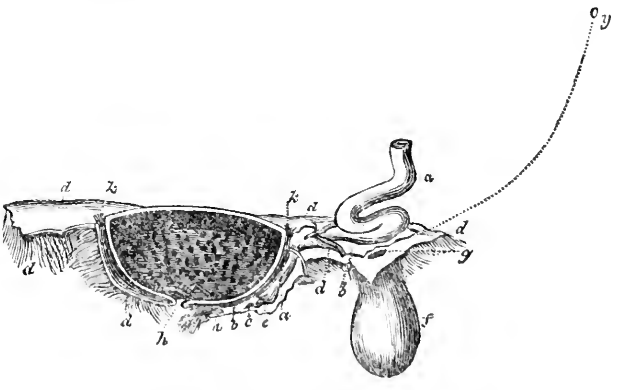


FIG. 62.—*Section of the Musk Sac in situ.*

a, The penis.
c, Urethra.
d d d, The hide.
e, Glans penis.
f, Scrotum.
g, Spot where the spermatic cord is cut off.
h, Aperture of the musk sac.
i, Preputial orifice.
k k, Muscular coat of the sac.
y, Position of the anus.

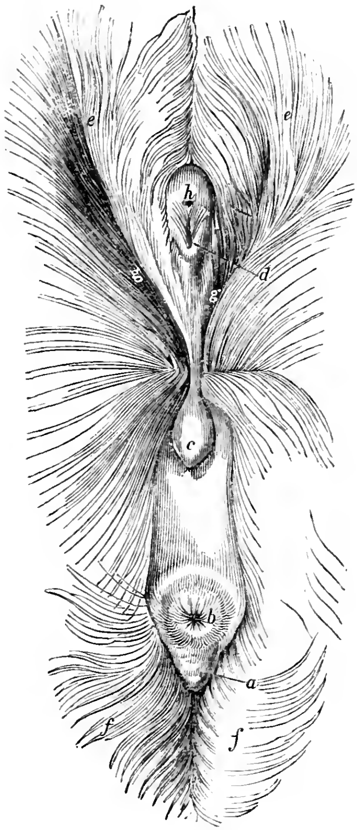


FIG. 63.

b, Anus.
c, Scrotum.
d, Preputial opening.
h, Orifice of the musk sac.

a brush or hair pencil; whereas the external musk aperture is placed in a depression, and is smooth.

The musk sac is of an oval form, rather broader at the anterior than at the posterior part. It is flat and smooth above,

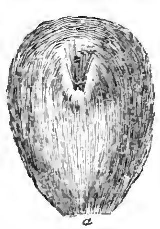


FIG. 64.

a, Penis cut off.

where it is in contact with the abdominal muscles, but convex below (supposing the animal standing.) Its breadth is from one inch and a quarter to one inch and three quarters; its length is from two to two and a half inches; its depth varies, being greatest anteriorly, where it is about half or three quarters of an inch.

I have already alluded to the external aperture in this sac. It is placed in the median line, but nearer the anterior than the posterior extremity of the sac. It is of a semilunar form, and its use is to allow of the evacuation of the musk. It leads into a small passage, which we may call the *musk canal*, the length of which is not more than a line and a half, and often not so much, the diameter being about a line. The internal orifice of this canal has a number of fine hairs placed around it, which easily coming off, are frequently found mixed with the musk of commerce.

I proceed, in the next place, to examine the structure of the musk sac. This organ is made of certain laminæ or coats, so disposed that some regard the musk sac as being formed by a fold of the skin of the animal, somewhat modified in its structure. Beginning with the most external, and proceeding inwards, they are as follows:

1. *The outer or hairy coat or skin.*—This is a continuation of the hide, and covers the convex portion of the sac — that is, it envelopes all that portion of it which is not turned towards the abdominal muscles. It is covered by a number of stiff but smooth hairs, which are so disposed as to form a kind of whorl or circular figure around the external musk orifice.

2. *Muscular coat.*—Beneath the skin we find a quantity of muscular fibre, which forms what we may term the muscular coat of the sac, consisting of two strata of fibres, between which, at the posterior part, is placed the penis. These fibres surround the sac in a circular form, as seen in fig. 65. Pallas states that they arise from the groin: anteriorly they unite with the panniculus carnosus. The same writer calls them the compressors and retractors of the follicle, and of the prepuce when the genital organ is thrust out. I have made out these fibres by soaking some of the most recent musk sacs of commerce for several days in water.

Pallas also describes and figures two muscular bands, which arise from the angle of the crura penis, and terminate at the glans. He calls them retractors of the penis.

On the inner surface of the muscular layers around the musk orifice, is a number of small oblong or roundish glands,

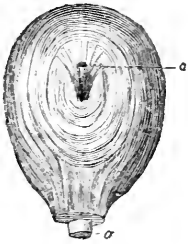


Fig. 65.

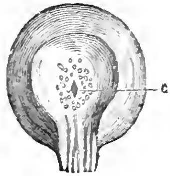


Fig. 66.

which Pallas has compared to the meibomian glands of the palpebræ. They are well shewn in this specimen. In fig. 66 is a drawing of them.

3. *Fibrous (?) coat: the most external of the proper coats of the sac.*—Beneath the muscular fibres is a lamina, which has (in the sacs of commerce) some resemblance in its firmness to the fibrous membranes; but its most remarkable character is the number of cells or depressions observed on its inner surface, and which are surrounded by ramifying folds. It is within these folds that the small vessels of the sac ramify; so that probably one use of them is that of a protection and covering to the blood-vessels. This coat is easily made out (as in the specimens before you) by soaking the musk sacs of commerce in water. It is continuous (through the musk orifice) with the corium.

4. *The second proper coat of the sac.*—Lining the fibrous (?) coat of the musk sac is a membrane, of a much softer and more delicate texture, and which generally has a shining mother-of-pearl-like appearance. It dips down into the depressions or cells of the fibrous (?) coat, and covers also the ramifying folds; so that on its internal surface it presents the same appearance of depressions and folds as the one just mentioned. It has been compared with the *rete mucosum* of the external skin.

5. *The epidermoid (?) coat: the third proper coat of the musk sac* has been compared to an epidermoid layer, though I candidly confess I do not see much analogy between them. It consists of two layers—an *outer* one, having usually a silvery white appearance—the *inner* one has a yellowish or reddish brown colour. These layers line the depressions or cells, already mentioned, of the foregoing coats, and cover the intermediate folds.

There is yet to be noticed a *glandular apparatus*, which secretes the musk. In each of the pits observed on the internal coat of the sac, there are found two or more irregular-shaped bodies, of a yellowish or reddish-brown colour. These bodies consist of a central brownish mass (supposed to be glandular), covered by a fine membrane.

Organs of generation.—Hitherto I have spoken of the genital organs in an incidental manner only; but it becomes necessary to say a few words on these parts, in consequence of their relation to the musk sac.

1. *Male genital organs.*—The *testicles* are oval, of the size of a walnut; the *vasa deferentia* are thin; the *vesiculæ seminales* small and oval; *Cowper's glands* are considerable; the *penis* is composed of one round *corpus cavernosum*, which terminates in a thin obtuse *glans*. The *urethra* is scarcely large enough to admit a straw,

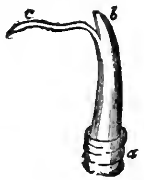


Fig. 67.—*Penis of the Moschus Moschiferus.*

a, Prepuce.
b, Glans penis.
c, Urethra.

and projects beyond the extremity of the glans (fig. 67) The *prepuce* is long, and at its mouth is furnished with a brush of hairs, as already noticed. In its usual state the penis lies rolled up within the belly. Its structure, and the form of the urethra and prepuce, may be readily made out by examining the musk bags of commerce. The preparations before you are from this source.

2. *Female genital organs.* — We are indebted to Pallas for an interesting account of them, both in the impregnated and unimpregnated state. The only remark I shall extract is, that no vestige of the musk sac can be observed. This statement does not agree with that of Valmont de Bomare, who erroneously says the

female is provided with a sac like the male, but that the contents are different.

Contents of the musk sac.—Pallas states, that in young animals he found the sac empty and contracted; in adults it contained about a drachm and a half of musk, and in old animals more than two drachms. But the weights of musk here mentioned must be below the average, since there is generally more than two drachms in each *dried* pod of Chinese musk, and frequently nearly three. Gmelin says, that in the fresh state it had the consistence of an electuary, and was of a reddish brown colour.

Function of the musk sac.—The function of the musk sac is evidently to secrete and retain musk; but what is the use of this substance? When we take into consideration the situation of the sac, its existence in male adult animals, and its analogy to other odoriferous sexual secretions, we can have but little doubt respecting its relation to the sexual functions. It is possible that, by the action of the cutaneous muscle, and by the pressure of the animal, some musk may be squeezed out, and by its local action operate as a venereal excitant. Among the Chinese, musk has long been celebrated as an aphrodisiac. Gmelin tells us that the Persian females use astringent and aphrodisiac unguents, containing alum and musk, for lascivious purposes; and Prosper Alpinus makes a similar statement of the custom of the Egyptian females. Berzelius remarks that it is supposed the physiological destination of musk is to render the search for the male more easy to the female at the rutting season, as the animal lives isolatedly. If the female only had the sac, I could easily believe it was to enable the male to find her; but as it is, I think this explanation very unsatisfactory.

One thing is certain, other animals are furnished with secretory organs more or less analogous; for example, the *antilope gutturosa*; a drawing of whose preputial gland has been given by Pallas. (Fig. 68.) The preputial sacs of some rodentia, especially of the *castor fiber*, are also analogous. One remarkable distinction, however, exists between the musk and castor sacs; which is, that the former is found only in the male, the latter in both sexes.

Geography of the musk animal.—It is exclusively a native of Asia, and is found, according to Brandt and Ratzeburg, on the mountains of this part of the world, within the 16° and 58° of north latitude, and the 92° and 155° of longitude; more especially between the Altai and Himalaya mountains. China, Cochin China, Tonkin, Tartary, and Siberia, have all

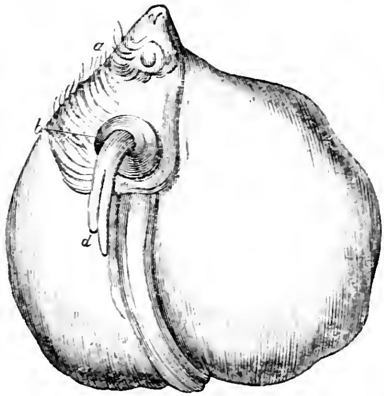


FIG. 68.—*Præputial Follicle of the Antilope Gutturosa.*

a, Orifice of the follicle.
b, Prepuce.
d, Penis.

been celebrated for it. The animal is timid, and dwells on cold mountainous districts, where coniferous plants are found.

Mode of catching the animal.—Various methods are adopted for obtaining the musk animals. Sometimes they are taken by snares or gins, sometimes by pitfalls, sometimes by shooting with the bow and arrow. The Tungouses, one of the native tribes of Siberia, employ the bow and arrow only.

Commerce and varieties of musk.—Musk bags are brought to this country principally from China and Russia, and sometimes from other parts, as Bucharia. I shall confine my description to the two first.

1. *China musk*, also called *Tonquin* or *Thibet* musk, is brought to this country in little rectangular boxes, about 7¼ inches long, 4⅝ inches broad, and 4½ deep; covered externally by silk, and lined with sheet lead and paper. These boxes contain about twenty-five sacs, or pods, each wrapped separately in paper. You observe that the box before us is marked on the outside "*Lingchong Musk*;" and on the inside of the lid is a rude Chinese representation of the musk hunters, some shooting the animal, others cutting out the musk bag.

The pods of China musk are roundish, or somewhat oval; generally broader at one end than at the other. The hairs are brownish-yellow, or greyish, or whitish, bristle-like, and stiff; arranged in a concentric manner around the orifice of the sac. The pods are about 2½ inches long, and 1¾ inches broad.

The following are the weights of six pods of musk of commerce:—

	Drachms.	Grains.
1	5	30
2	4	30
3	8	37½
4	9	47½
5	5	20
6	3	30
Total	37	15
Average ..	6	12½

The relative proportion of musk and coats of the sacs in these were as follows:—

	Drachms.	Grains.
Musk (grain)	16	15
Coats of the sacs ..	21	0
Total	37	15
Average quantity of musk in each sac	2	42½

For this information I am indebted to Mr. Noakes, druggist, of Snowhill.

The musk contained in these pods is granular, of a dark-brown colour, unctuous to the feel, of a bitter and aromatic taste, and of a well-known intense and remarkable odour. A number of anecdotes are told respecting its odorous property, which, although probably true, are almost incredible. Thus Alston says that a clean cork used as a stopper to a phial containing musk, and which it seemed never to have touched, in 1712, retained the scent more than twenty years after. It is also said that a single grain may, during many years, fill the air of a large apartment with its perfume, without its weight diminishing in any appreciable degree. These and other similar statements are sometimes instanced as proving the extreme subtilty of the particles of matter, and, therefore, the probability that matter is infinitely divisible. When mixed in small quantities with other odorous substances, it augments their smell without much imparting its own; and hence it is extensively used by perfumers. The odour of musk, which is promoted by the addition of a few drops of solution of potash, is not, however, peculiar to this substance, various other animals having the same; some vegetable also; and it has even been said some mineral solutions. As instances of other animals having a musky smell, I may quote the musk ox, (fig. 69), the musk rat, the crocodile (which

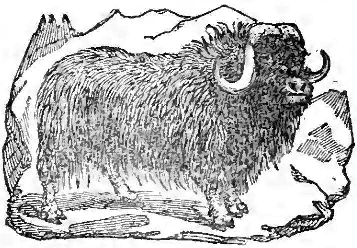


Fig. 69.—*Musk Ox.*

has a submaxillary gland, secreting an unctuous musky substance), the anas moschata, or Barbary duck, the formica rufa, &c. Some persons have declared the urine of a male cat to be similar in its smell to musk; though, I confess, I never could perceive the analogy. Among vegetables which have a musky odour, I shall only quote the erodium moschatum, malva moschata, and centaurea moschata.

The finest musk, on examination, appears to consist of dark-brown globules, usually intermixed with hairs and pieces of fine membrane.

2. The *Siberian*, *Russian*, or *Kabardine*, is considered an inferior sort. The pods are said to be more oblong or oval than the China kind; the hairs longer and whiter. But I have examined large quantities of Siberian musk, the pods of which were not distinguishable from those of China by any of these characters. The only invariable distinction I have observed is in the scent; which is remarkably different from that of the China kind: it is much less powerful, and more nauseous and disagreeable, being somewhat empyreumatic. Here are specimens. Geiger says it is sometimes accompanied by an odour similar to that of the sweat of a horse. This kind of musk is imported in wooden boxes, and all the pods that I have examined were in a good state of preservation; but frequently, I am told, this is not the case.

Chemical properties.—Musk has been analysed by Thiemann, by Guibourt and Blondeau, by Buchner, and by Geiger and Reimann. The substances which they have detected are the following:—

1. Volatile matters	Odorous matter. Water. Ammonia Volatile oil		
2. Fatty matters	Saponifiable	Solid .... Liquid	Stearine. Oleine.
	Not saponifiable	....	Cholesterine.

3. Resin.
4. Alcoholic extract (osmazome ?).
5. Aqueous extract (musk acid of Buchner).
7. Free acid (lactic ?).
8. Salts.

1. *Volatile matters.*—The first matter deserving our consideration is the *odorous principle.* It is a curious fact, that although the smell of musk is so strong and diffusive, yet the constituent of the musk which is the source of it, is not volatile. Analogy would lead you to suspect that it depended on a very volatile oily matter; but experiment seems to negative such an opinion.

Here is water distilled from musk: it has, indeed, the musky odour, but the residue in the retort retains the same. In fact, no distillation will deprive musk of its remarkable smell. What is the nature, then, of the substance in musk which is the cause of it? You will, perhaps, be surprised to hear, that hitherto it has evaded the researches of chemists. It is organic, for it is destructible by heat; it is not peculiar to musk, since so many other substances exhale an analogous odour. What, then, is it? Here is an experiment which throws some light on it: I place a little musk over a basin of sulphuric acid, and under this air-pump receiver. By exhausting the receiver, and allowing the musk to remain for some time in this position, it becomes well dried; the process being called "Leslie's method of desiccation." Now it is said that musk thus deprived of all its moisture is inodorous; but that by exposure to the air, by which it becomes damp, or by the addition of water, it regains its well-known smell.

Though I have several times performed the experiment, I cannot say that I have been able to satisfy myself that the musk becomes scentless. [The experiment was here performed; the musk, however, was not wholly deprived of its odour.] Geiger and Reimann say they have performed this experiment thirty times successively on the same portion of musk; that each time, by drying, it lost its odour, and subsequently regained it by the agency of moisture. Is it the result of putrefaction of one or more of the constituents of musk? The circumstances already mentioned, as well as some others, favour this notion; and they are easily accounted for by it. It is said that bile at a certain period of decomposition has a musky smell: if so, this is in favour of the opinion here mentioned. Robiquet has a notion that many odorous substances owe their odour to a certain quantity of ammonia, which is disengaged, and carries with it substances not otherwise volatile; but that these mask the smell of the ammonia.

In applying this hypothesis to musk, it must be admitted that it harmonizes well with several of the circumstances observed. It is true that musk emits ammonia, that water distilled from musk contains ammonia, and that liquor potassæ (which facilitates the evolution of ammonia), added to a solution of musk, heightens its odour. Neumann says, "some report that musk which has lost its smell, recovers it again on being hung in a privy; and that this is the common practice among the perfumers." If the statement be correct, it is certainly in favour of Robiquet's opinion; though, upon the whole, it is founded on very slender evidence.

Musk contains a little carbonate of ammonia and water: the quantity of these two substances appears to vary, according to the experiments of the following chemists:—

In 100 *parts of musk*—

	Water and Carbonate of Ammonia.
Thiemann found	15
Guibourt and Blondeau	47
Buchner	17·6
Geiger and Reimann	41

2. *Fatty matters.*—I have digested some musk in æther, and you observe I have obtained a slightly-coloured liquid, which has in solution a quantity of fatty matter, with some resin. I place a little of this ætherial tincture on glass, and by the evaporation of the æther a thin crust of fat and resin is obtained. The fatty substance consists of stearine, oleine, and cholesterine: the last mentioned substance has been so termed (from χολὴ, *bile*, and στεαρ,

suet or *fat*) because it is found in biliary calculi, sometimes in the pure and white state (as in this preparation), but usually mixed with other substances which colour it. This cholesterine is distinguished from the other fatty matter of musk by its not forming a soap by the agency of an alkali. Nysten says musk contains adipocire (so called from *adeps*, fat, and *cera*, wax), a substance which is formed by the spontaneous decomposition of fatty bodies, and with which cholesterine was at first confounded. It is not at all improbable that by the spontaneous decomposition of the constituents of musk, some adipocire might be formed; for this substance is an ammoniacal soap, and we have already mentioned that musk evolves ammonia, and we know that the acid (margaric) of this soap is readily obtained by the action of the ammonia on stearine. John says musk contains wax, but possibly that which he obtained was cholesterine.

3. *Resin* is a constituent of musk, to which I have already alluded. It is contained both in the etherial and alcoholic tinctures of musk. In order to show you that this alcoholic tincture contains resin, I must proceed as follows (for the addition of water to the tincture will not precipitate the resin):—Place some tincture on a capsule, and evaporate; when it has become sufficiently concentrated, add water, and you observe the white flocks of resin, which are thrown down. This resin has a bitter taste, and a musky odour.

4. *Osmazome* is a term which Thenard applied to an alcoholic extractive matter of meat, on account of its odour. It is derived from ὀσμὴ, *odour*, and ζωμὸς, *broth*. Berzelius, however, asserts it is a mixture of animal substances. Whatever its nature may be, it is a constituent of musk.

5. *Musk acid* of Buchner, or the *aqueous extract* of Berzelius, is another constituent of musk deserving notice. Its nature is imperfectly known. It exists in musk in combination with potash and ammonia; so that if it be really an acid, as Buchner fancies, we may say musk contains *muskates of potash* and of *ammonia*. Its characters are the following:—

It is insoluble in water by itself, but when combined with potash or ammonia, is soluble. Hence an aqueous infusion of musk holds it in solution, in consequence of the alkali with which it is combined. The acetic and gallic precipitate it, probably by removing the base. Here is an aqueous infusion of musk, and you observe acetic acid produces some coloured flocculi, while gallic acid causes a cloudiness. It is possible that the precipitate in these cases may be a compound of the acid employed with this aqueous extractive. Certain salts also precipitate this substance; for example, sugar of lead. [The experiments were shown on an infusion of musk.] Probably the matter here thrown down is a kind of muskate of lead, an acetate of potash and ammonia remaining in solution.

Three substances (namely, *fibrine*, *albumen*, and *gelatine*) have been stated to be in musk, the existence of which is doubted by some writers. Berzelius supposes this aqueous extract has been mistaken for them.

The *salts* present in musk are of little importance medically. The following have been detected: sal ammoniac, chloride of sodium and of calcium. Hence nitrate of silver precipitates an infusion of musk.

I shall conclude the chemistry of musk with a tabular view of two analyses:—

Guibourt and Blondeau.

1. *Volatilized by drying* — Water	46·925
Ammonia	0·325
2. *Extracted by æther*—Stearine, oleine, cholesterine, fatty acid with ammonia, traces of a volatile oil	13·000
3. *Extracted subsequently by alcohol* (alcoholic extract of Berzelius; osmazome of Thenard)—Cholesterine, fatty acid with ammonia, sal ammoniac, chloride of potassium, of sodium, and of calcium, and an undetermined acid combined with the same bases	6·000
4. *Extracted subsequently by water* (aqueous extract of Berzelius)—Gelatine, carbonaceous matter soluble in water, the preceding chlorides, and an undetermined combustible acid	19·000
5. *Extracted subsequently by ammonia*—Albumen and phosphate of lime	12·000
6. Fibrous tissue, carbonate and phosphate of lime, hairs, and sand..	2·750
	100·000

Geiger and Reimann.

1.	Peculiar volatile substance	Quantity undeterminable.
2.	Ammonia	Ditto
3.	Peculiar, fixed, uncrystallizable acid	Ditto
4.	Stearine and oleine	1·1
5.	Cholesterine (with some oleine and resin)	4·0
6.	Peculiar bitter resin	5·0
7.	Osmazome (with sal ammoniac, chlorides of sodium and calcium, and the above acid, partly free, partly combined with the bases)	7·5
8.	A mouldy-like substance, in part combined with ammonia, by which it is made soluble in water, with small quantities of phosphates of lime and magnesia, sulphate of potash, chlorides of potassium and sodium, carbonate of potash or soda, and trace of iron	36·5
9.	Sand	0·4
10.	Water, some volatile odorous matter, the above acid in part combined with ammonia, and loss	45·5
		100·0

Adulteration.—Musk being a very valuable article, we are easily prepared to admit the probability of its being extensively adulterated; and if we are to believe the statements made in books, it is difficult to procure a specimen of this substance which is absolutely pure. My own observation does not, however, coincide with these assertions; I think that at the present time, in this country, musk is not much adulterated; though sand, stones, &c. are sometimes found. It is said that the bags have been imitated by the skins of animals, or by the scrotum of the goat. I have never seen any of these; nor do I believe they are now to be met with. If ever the fraud should be attempted, it would, I presume, be easily discovered.

We are told that the musk pods are sometimes opened, and various foreign substances introduced to increase the weight; for example, pieces of lead, &c. I have frequently seen pods which, on examination, have been sewn up around the edges, or at the orifice; but it appeared in most cases as if this had been done to prevent the loss of the musk, rather than that the bag had been opened for the purpose of adulteration.

Dried blood, bile, snuff, &c. are likewise said to be mixed with musk. One of the best tests for the genuine, is that an infusion of it does not precipitate a solution of corrosive sublimate; at least the musk before me, which I believe to be genuine, gives no precipitate, as you observe; and Berzelius and Geiger both lay great stress on this test. Curiously enough, in Dr. A. T. Thomson's work on the Materia Medica, and in his London Dispensatory, we are told that a solution of corrosive sublimate precipitates infusion of genuine musk, and not the spurious; a statement which is incorrect, according to the experiments of Berzelius, Geiger, and myself. [The experiment was here tried.] Besides this test of good musk there are others: acids (especially the nitric) precipitate infusion of genuine musk; as also sugar of lead, and infusion of nutgalls. [The experiments were shown.] By combustion a grey ash is left behind; whereas dried blood leaves a reddish ash.

The *physiological effects* of musk next deserve our attention. The latest, and perhaps the best, series of experiments made on this subject are those of Jörg with the society to which I have already alluded in speaking of the effects of castoreum. The experiments were conducted on five men, two women, and two lads; and the quantity of musk taken was from two to fifteen grains, in water, or mixed with magnesia. The primitive effects were eructation, weight at the stomach, diminution or increase of appetite, dryness of the œsophagus, heaviness of the head, vertigos, and headache. The secondary effects were more marked on the encephalon than on the digestive canal: disposition to sleep, faintness, and a feeling of heaviness in the whole body. Lastly, deep and long-continued sleep. In very large doses the action on the nervous system was very marked; trembling in the limbs, and even convulsions, were observed. The pulse was rendered more rapid and fuller.

These symptoms show that musk is a cerebro-spinant or a stimulant to the nervous system, and that it also acts on the organs of digestion and circulation. In some persons the nervous

system appears to be peculiarly susceptible of the odour of musk; for it is reported that headache, vertigos, and even fainting, have been produced by it. When the digestive apparatus is previously in a state of irritation, musk increases the local disorder, giving rise to pain, nausea, vomiting, and diarrhœa. Sometimes the stimulus of musk is extended to the sexual organs, as is shewn by the increased venereal excitement, or the production of the catamenia. In persons disposed to epistaxis, the use of musk will sometimes bring on this disease. Occasionally it excites sweating; at others an increased discharge of urine.

Like all other agents which act on the nervous system, the operation of musk seems to be somewhat uncertain. While in some cases the odour of it merely will produce serious symptoms, as I have already mentioned, in others large doses have been administered internally with very little effect.

It is usually believed that musk becomes absorbed. Barbier states that the urine and the sweat of persons who have taken this substance are powerfully impregnated with its odour—now and then so strongly, that the hand applied, for the purpose of feeling the pulse, retains the odour for some time. On post-mortem examination, the brain and the cavities of the chest and abdomen, in those who have taken it during life, somtimes emit a strong smell of musk.

We have also the evidence of Tiedemann and Gmelin in favour of the opinion that it is absorbed. Their experiments (to which, on a former occasion, I directed your attention) were made with the view of determining the question of lacteal or venous absorption; and one of the odorous substances employed was musk. This medicine was given to animals, and its odour was subsequently discovered in the blood of the splenic and portal veins, but not in the contents of the lacteals. Notwithstanding this apparently positive evidence of the absorption of the odorous particles of musk, we find Professor Jörg denying the inference which has been drawn. He says that numerous experiments have satisfied him that the urine and fæces of persons who have taken musk do not acquire a musky smell, and that the mistake in this respect has arisen from the frequent musky eructations which communicate this odour to all surrounding bodies.

What is the active constituent of musk? I do not think we are prepared yet to answer this question. We are not necessarily to infer that it is the same substance which is the cause of the odour, for the odorous principle of opium and of ipecacuanha is certainly not their active principle.

Uses.—The effects of musk, already alluded to, show that it is a remedy which will be useful where we want to excite the nervous system; and, *vice versâ*, that it will be hurtful where there exists a determination of blood to the brain, and in those constitutions denominated plethoric. The cases in which experience seems to have shown musk is sometimes useful, are the following:—

1. Those diseases which are attended with convulsive movements, and which, therefore, are called spasmodic. Such, for example, as hysteria, epilepsy (especially of children, and where the disease does not depend on organic changes, or on plethora), chorea, and even in some cases of tetanus. The employment of musk here has led to its denomination of antispasmodic.

Dr. Cullen, on whose practical information I place the greatest reliance, says, "I maintain that musk (when genuine) is one of the most powerful antispasmodics that we are acquainted with. I have found it, with Dr. Wall, to be a powerful remedy in many convulsive and spasmodic affections, and in some of a very peculiar kind. I had once a gentleman affected with a spasm of the pharynx, preventing deglutition and almost respiration. This, when other remedies had failed, was relieved by the use of musk, which often shewed its power; for the disease continued to recur at times for some years after, and was only obviated or relieved by the use of musk."

As musk in large doses excites tremblings and convulsions in the healthy state, while it is beneficial in convulsive diseases, it is considered by the followers of Hahnemann to be in these cases a homœopathic remedy.

2. In *low fevers* which are accompanied with delirium, twitchings of the muscles, a small contracted pulse and convulsions, musk has been occasionally employed, and with benefit. Like opium, its use in these cases is always uncertain—in one instance relieving, in another increasing the malady, though the cases may be to all appearances parallel.

3. In *retrocedent gout*, as where gout attacks the stomach or the head, giving rise to headache or delirium, musk has been found beneficial. Cullen relates a case where immediate relief was obtained by the exhibition of fifteen grains of genuine musk.

4. Lastly, during the late severe visitation of malignant cholera, musk was one of the remedies tried. I saw it employed several times, but without obvious relief. The experience of others was various; but

the result is, that practitioners formed a very low estimate of its power in this disease.

Mode of administration.—It is best given in substance; indeed, this is the only proper way of exhibiting it. We may form it into boluses, or suspend it in water by means of some mucilaginous or saccharine fluid.

The formula of the London Pharmacopœia for "*the musk mixture*," is one drachm of musk, the same quantity of powdered gum and of sugar, and six ounces of rose water. But you will find it much better to employ twice or thrice the quantity of gum, and at least half as much again of musk, so that one fluid ounce would then be a dose; whereas a larger quantity is now required to be given.

In the Dublin Pharmacopœia a "*tincture of musk*" is ordered, consisting of two drachms of musk to a pint of rectified spirit. It is evident, however, that we must give four ounces of rectified spirit, if we wish to give fifteen grains of musk.

The "*essence of musk*" of the shops is usually prepared from the musk sacs after the musk has been extracted. They are digested in spirit; and to promote the odour a small portion of subcarbonate of potash is sometimes added. The following is a

Formula for essence of musk.—Take of grain musk, fourteen drachms (or of the membranes of the musk pods, seven ounces); boiling water, half a pint. Digest until cold; then add, rectified spirit of wine, six and a half pints; subcarbonate of potash, half a drachm. Digest.

The above was given me by a druggist, as the formula usually adopted.

REMARKS ON CERTAIN THEORIES REGARDING THE SOUNDS OF THE HEART; WITH THE SUGGESTION OF A FRESH HYPOTHESIS.

To the Editor of the Medical Gazette.

Sir,

Reading lately the third edition of the work of Dr. C. J. B. Williams on the Diseases of the Chest, I observed that he has prefaced the third part, relating to affections of the heart, by detailing a new set of experiments which he instituted for the purpose of further elucidating the cause of the sounds of that organ. From the observations which he made during the course of these experiments, he arrived at the conclusions that the first, or prolonged dull sound (which occurs at the time of the ventricular systole), is entirely caused by the muscular contraction itself, of the ventricles; and that the second short sound is owing to the shutting to of the semilunar valves by the re-action of the arterial columns of blood, at the moment of the ventricular diastole.

The idea of the first sound depending on muscular action is, I believe, originally Dr. Williams's; he mentioned it as being probable in the first edition of his work, though he then brought forward no direct evidence in proof of his opinion. Some authors, and among others M. Magendie, have supposed that Laennec believed the sounds of the heart to depend on this cause; but I think he never attempted to give any explanation of their physical origin. The view of the production of the second sound by the closing of the arterial valves, has been supported by the opinion of several writers; among others, M. Rouanet, Mr. Carlile, and M. Bouillaud, who have founded their hypotheses on various arguments.

The results of the experiments made by Dr. Williams, from which he drew his conclusions, appear very satisfactory; and on first looking over them, I thought that they afforded clear proof that his theory of the production of the sounds was correct; but on a more careful investigation of the different points in each experiment, and by considering the manner in which the various phenomena occurring in disease could be explained by these causes, I observed that different deductions might be drawn from the same observations; and that it was very difficult to account for the production of many morbid sounds by the above theories.

Thinking over these circumstances, and considering in what manner a better explanation might be afforded, I have ventured to bring forward a fresh hypothesis, stating the arguments upon which I found my own opinions, and my reasons for rejecting those of Dr. Williams.

I will commence with the latter, first briefly stating the observations upon which his theories rest.

1. During the first experiment which he has detailed, when the heart was exposed, and its action continuing by passing his finger through the mitral

orifice into the left ventricle, and pressing on the right, so as to stop, as he considered, all influx of blood, the ventricles continued to contract, and the first sound was still heard, though less clear than when the circulation was allowed to continue. 2. The same phenomena were observed when both arteries were severed from the heart.

From these facts it is inferred that the first sound depends solely on the muscular contraction of the ventricles.

With regard to the experiments on the second sound:—1. It was heard more clearly over the origin of the aorta and pulmonary artery than on any other part of the heart. 2. When pressure was made for some seconds on the origin of the aorta and pulmonary artery, the second sound ceased. 3. When the closure of the semilunar valves was mechanically prevented, by holding them back, so as to allow of regurgitation, the second sound was diminished, or stopped. It is concluded from these observations that the second sound is caused by the shutting to of the semilunar valves.

In opposition to these deductions, I must say that I think with Dr. Forbes, "that the two sounds, though characteristically different, have so great a similarity, and are so allied both in time and place, that it is unphilosophical to attribute them to different causes, but that in all probability they depend on some modification of the same;" and as I cannot exactly agree with Dr. Hope, who says that both sounds are caused by the collision of the particles of blood in the ventricles during their systole and diastole; or at all assent to the opinion of M. Bouillaud, M. Rouanet, and Mr. Bryan, who think that the first sound is produced by the shutting to of the auricular, and the second by that of the arterial, valves; I must find some other cause which will explain the production of both sounds on the same principle, without being open to the objections to which those that I have mentioned are liable.

But let me return to my reasons against the first sound being the product of muscular contraction.

In the first place, I think it far from proved that the contraction of muscles produces any appreciable sonorous vibrations. If the stethoscope is applied to a muscle in action, some sound is certainly heard both while it contracts, and also while it becomes relaxed, or returns to its original position. It is very possible that this sound altogether is only occasioned by the slight movement of the part against the end of the instrument, which is unavoidable when the muscle is in action. But granting that the sound heard is produced by the muscular movement itself, it is quite inadequate in intensity to account for the first sound of the heart. Listen, for instance, to the gluteus maximus in action, and then to the left side of the chest; where will you hear most sound? and which is the larger muscle, the one which I have mentioned, or the heart? Again, in dyspnœa from pleurodynia, where the diaphragm has to perform the whole function of inspiration, can its contractions be heard by the stethoscope applied to the chest?

Dr. Williams says, that when the layer of muscular fibre becomes thinner and more simple, as in ventricular dilatation, the sound from its contraction becomes louder and clearer than usual. If this is the case, why do not the auricles produce any sound? They certainly have muscular parietes; and if the vibrations are increased in the inverse ratio to the thickness of the walls, should give rise to a louder sound during their contraction than the ventricles; but, as has been clearly proved by the experiments of Hope and others, they do not cause any that is appreciable to the ear. As to the results of the observations on the first sound, made during the experiments of Dr. Williams, in all probability the circulation of blood was not entirely impeded through the heart by the passage of the finger into the mitral orifice, at any rate on the right side; and that therefore the sound still continued, though much diminished in intensity. As to the experiment of cutting off both the arteries, the mode in which it was conducted is mentioned very briefly, or I think that some fallacy might be detected in the result.

The most probable mode of production of the first sound is, that it is caused by the rush of blood over the arterial valves, and into the orifices of the aorta and pulmonary artery, at the time of the ventricular systole. Any alteration, from disease in the size or regularity of the openings, will modify the sound, and give rise to the different bruits which

are heard in disease of the arterial valves. In this mode the natural and unnatural sounds are referred to modifications of the same cause, which is more probable than to suppose them to have different seats; for the healthy sound does not merely seem to be accompanied by an unnatural one, but to be itself actually altered. It may be said, if that was the seat of the sound in the arterial orifices, it would be heard more intensely opposite that spot than at the apex of the heart, the reverse of which is the case. But I consider that the vibrations produced by the succession of the particles of fluid against the valves, are propagated backwards to the mass of blood in the ventricles; therefore the sound is heard over the whole surface of the heart, but more particularly at the apex, in consequence of that part being in more immediate contact (by the impulse) with the sides of the chest, at the time that the first sound is produced. In corroboration of this, morbid alterations of the first sound accompanying diseased arterial valves are always heard most intensely in the same spot (the apex), though I believe all pathologists agree in their seat being at the commencement of the arteries.

According to this theory, how can the phenomena of dilatation and hypertrophy of the ventricles be explained? Why, in the former disease the sound is rendered shorter, by the muscular walls contracting more quickly, on account of their thickness; it becomes louder and clearer, in consequence of the less thickness of muscle which the sound has to travel through; and it is heard over the larger space, from the extension of the sides of the cavity.

In hypertrophy, it has been said that the greater force with which the blood is propelled into the vessels, should increase the sound, instead of its being, as it is in that disease, diminished; but I think that there is no proof or probability of such being the case. If the quantity of fluid is not increased, nor the orifice through which it is transmitted diminished, it makes but little difference, as to sound, whether it passes rather more quickly or slowly; in fact, it will cause less noise in the former than the latter case; and in hypertrophy, the greater thickness of the walls of the ventricle, through which the sound has to be conveyed, will much diminish its intensity.

I have not yet said any thing about the second sound of the heart. Both the seat and mode of production of this phenomenon are points on which there has been much discussion and great difference of opinion; some referring it to the action of the arterial valves; others to the rush of blood against the sides of the ventricles, or the knocking of the heart against the inside of the chest, which M. Magendie most improbably supposes to be the cause of the second, as well as the first sound.

Thinking it most plausible to refer both sounds to similar causes, I consider that the second is produced by the rush of blood through the auriculo-ventricular orifice, and over the mitral and tricuspid valves, at the moment of the ventricular diastole. The difference in duration and character between the two sounds may be explained by the greater suddenness and quickness with which the blood rushes into the ventricle, than the muscular contraction can force it out again.

But from what arguments do I come to these conclusions? and upon what grounds do I reject those of Dr. Williams and others? First, is it probable that the valves close with sufficient force to produce such a loud sound, or, in fact, any at all? I think not; for they most likely act, not suddenly, but gradually. And if the arterial valves, by shutting, have such an effect, why are the auricular silent? They may as well be supposed (as is the opinion of some writers) to produce the first sound.

As to the experiments of Dr. Williams, he says that the short sound was heard more clearly over the region of the aorta and pulmonary artery, than on any other part of the heart when it was exposed. This would also be the case were its seat in the auriculo-ventricular orifice instead of the semilunar valves; for they correspond very nearly in situation. And if, as is probably the case, the vibrations are principally felt in the auricles, those cavities are so closely connected with the origin of the great arteries, that the sound will be heard most clearly over them.

With regard to the observation that pressure on the origin of the aorta and pulmonary artery stopped the second

sound, this might occur from the passage of blood being impeded from the ventricles, and therefore not a sufficient quantity rushing in with the diastole to cause perceptible sonorous vibrations. The reason of the second sound being diminished or lost, when the closure of the semilunar valves was mechanically prevented by introducing an instrument and holding them back, is sufficiently clear; the regurgitation which ensued, filling the ventricles in great part from the arteries, and not the auricles, and the hissing murmur occasioned by the backward passage of the blood over the injured valves, prevented the enfeebled sound which remained from being heard.

A further reason for my considering the seat of the second sound to be at the auriculo-ventricular orifice is, that when the mitral valves are diseased, this sound is altered, accompanied with a murmur, the first sound remaining natural, unless the morbid change is so great as to allow of regurgitation, when we have a bruit with both sounds. In lesions of the aortic valves, where much regurgitation does not take place, the second sound is unaltered—only the first changed. Now it is probable that obstructive disease of these valves would prevent them from closing with sufficient freedom to produce the clear sharp sound, if it had its seat there. And in a case described by Dr. Hope, where there was extensive disease of the aortic valves, so that they were in a state of complete rigidity, the second was heard very clearly on the left side of the heart before death.

From these and other circumstances, I have arrived at the conclusions stated above, but I must allow that fresh observations and new experiments are required to confirm these theories, particularly the mode of production of the second sound.

Since the above ideas occurred to me, I have understood that there is a committee appointed in Dublin for the purpose of investigating the sounds of the heart; and I believe that they have come to the same conclusions with myself as to the origin of the first sound, but have not decided concerning the second. Their proceedings are, I think, not yet published, and I am not acquainted with the experiments from which they suppose the first sound to be caused by the blood rushing over the semilunar valves.

I am, sir,
Your obedient servant,
R. H. Meade,
Clinical Clerk at St. Bartholomew's Hospital.

St. Bartholomew's Hospital,
Dec. 1, 1835.

P.S. If you should think these observations of any interest, and can find room for their insertion, you will oblige me by giving them a place in the Medical Gazette.

OBSERVATIONS ON THE TRICHINA SPIRALIS.

To the Editor of the Medical Gazette.

Sir,

Another instance of the trichina spiralis infesting the human body, having been recently met with in the dissecting room of St. Bartholomew's, the matter has again excited considerable interest; and as I am not aware of the occurrence of any other case since the publication of Mr. Owen's description of this entozoon in April last, I conceive that any further information upon the subject will be the more acceptable. This I feel some confidence in offering, inasmuch as I find the opinions which I previously entertained have been in almost every respect confirmed.

I take this opportunity, therefore, of communicating the result of my observations upon specimens procured from this as well as from the two former cases mentioned in Mr. Owen's paper*, read before the Zoological Society; and to which paper I refer for the general description of the animalcule, limiting myself here chiefly to those points upon which I am enabled to give additional information, or in which my observations have differed from those of Mr. Owen. In the first case, which occurred early in February, my attention was directed principally to the peculiarities of the cysts within which the worms are contained; and as the worms

* An abstract of this paper was published in the Gazette for April 25th of the present year. The whole paper will shortly appear in the 4th vol. of the Zoological Transactions.

themselves were all dead, and I had not then acquired a facility in extracting them, I do not place so much reliance in my observations upon them as upon those from the second case. This case occurring three weeks after the former, afforded me the opportunity of examining the worms alive. My attention was then, for the first time, attracted by the appearance of something like an alimentary canal, distinct from the parietes of the body, and extending from one extremity of the worm to the other. This appearance I found to be presented, more or less distinctly, by a great variety of specimens, but in consequence of the extreme delicacy and transparency of every part of the worm, this supposed structure was indicated by only a very faint outline. After examining a great number, I met with one in which the whole course of this apparent canal was clearly defined. This specimen I immediately copied by means of a camera lucida, fixed to the eye-glass of the microscope, in order to insure the accuracy of the representation. It forms fig. 3 in the annexed engraving; reference to which will render the description of this part more intelligible.

Commencing from the large end of the worm, *a*, the canal is seen bounded by two slightly irregular lines, running parallel to each other for a distance of rather more than one-fifth of the length of the body, where they terminate in a transverse line, *b*, presenting a slight concavity towards the large end, which line I have observed in almost every specimen that I have examined. From this point the canal puts on a sacculated appearance, *c c*; and these sacculi appear as if bound down by a line extending along the surface of the canal, in the direction of its axis. This sacculated appearance becomes gradually lost towards the smaller end, where the part assumes a zig-zag, or perhaps spiral course, *d*, and at length terminates at the small end *e* *.

But the lines indicating the course of this canal generally become so faint towards either extremity, that I have seldom been able to trace them so far as in the specimen figured; although the central puckered part represented at *c c* may be seen in most instances.

As I had observed these appearances in all positions of the animal, I could not attribute them to any accidental creasing of the internal parts, resulting from the curvatures of the body, nor could I explain them in any other way than by supposing that they were produced by a distinct tube contained within the external parietes, and having little or no connexion with them; this tube appearing to be considerably longer than the body of the animal, and hence the puckering would result from its being collected into a smaller space.

These suspicions were confirmed when I observed in one of the worms a distinct motion of the inner tube taking place, a large portion of it being slowly drawn backwards and forwards within the body of the animal, and the action being repeated eight or ten times, so as to leave very little doubt in my mind as to the separate existence of this part *. As, however, I might have been deceived by a motion communicated to the contents of the inner tube, and not taking place in the tube itself, I was still anxious for further proof; which was afforded me the other day, when, on cutting across one of the living specimens (from the last case), I observed the inner tube projected gradually, as if by its own elasticity, from the cut extremity of the body, until a considerable portion protruded. This operation I repeated several times, and always with the same result, the protrusion of the inner tube taking place to a considerable extent, and showing very distinctly the sacculated structure, together with the central line running along its surface.

This appearance is represented in fig. 4; where *a* is the extremity of one of the worms, *b* the point at which the section was made, *c* the length of intestine that has protruded. The granular matter with which the body is filled appears to be contained within this tube.

It may be thought, from an inspection of these engravings, that if the object is fairly represented, the existence of an alimentary canal cannot admit of doubt; but every one accustomed to the microscopic examination of minute animalcules, is aware that it is exceedingly difficult in a drawing to avoid giving too definite an idea of the object to be represented. When, more-

* To avoid confusion in the drawing, the minute granules with which the worm is filled are not here represented.

* I have witnessed this appearaance several times since, though in a less marked manner.

Fig. 1.

Fig. 2.

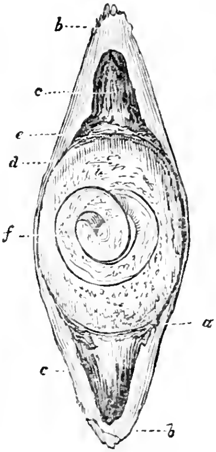


Fig. 4.

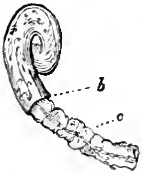
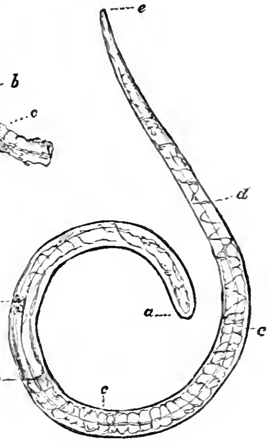


Fig. 3.



Explanation of Plate.

Fig. 1.—Portion of biceps muscle containing the trichinæ, with their cysts (natural size).
Fig. 2.—A separate cyst, with the worm coiled up in its interior, and seen through the transparent walls.
Fig. 3.—Worm extracted from cyst, shewing the alimentary canal and ovary.
Fig. 4.—Portion of worm showing the alimentary canal protruding from the cut surface.
The last three figs. as observed by powers ranging from 200 to 450 linear measurement.

over, it is remembered that the worm itself is not more than the 700th of an inch in diameter, so as to be scarcely visible to the naked eye, it will be seen at once that the determination of its structure is necessarily attended with considerable difficulty; inasmuch as it requires the aid of a very high magnifying power, which, even in the best glasses, is always accompanied by a corresponding diminution in the distinctness of the object examined. As, in such cases, therefore, great augmentation cannot be obtained except at the expense of clearness of definition, it is necessary to be guided by the constancy with which any particular appearance is presented; and as I have observed the appearances here described, more or less distinctly, in at least fifty instances, I cannot doubt but that they indicate the existence of a structure holding a very important place in the economy of the animal, and which, from its size, form, and situation, I believe to be the alimentary canal.

But the presence of an alimentary canal such as this appears to be, would lead to the supposition of the existence of a mouth and anus. Upon this point, however, I have no very satisfactory evidence to offer. In many of the specimens obtained from the second case, I observed the appearance of a linear mouth at the larger end, as it is described by Mr Owen; but in only one instance could I satisfy myself of the existence of an aperture in this situation, when, from the position of the worm, which happened to be with the large extremity towards me, I was enabled to see *into* what appeared to be an irregular rounded opening. But although I have searched diligently for this appearance in the specimens recently obtained, I have not been able to detect it in any one of them. I thought, however, that in two instances I could distinguish something like an aperture at the small end. These points, however, I leave for further investigation; and from the minuteness of the object they will probably not be determined without great difficulty; but I am anxious that my observations upon the former point should be confirmed, because, if found to be correct, they will have the tendency of approximating the trichina to the more highly organized groups of entozoa which constitute the

vers cavitaires of Cuvier, rather than to the simple parenchymatous forms among which it is now placed.

But there is another point in the organization of this animal which attracted my attention. It consists of an aggregation of small round granules, about a dozen in number, situated at about one-fifth of the length of the animal from the larger extremity, and generally close to that side of the coil which is outermost. It extends about half-way across the diameter of the body, and is composed of larger granules than those which are distributed irregularly through the body; but when viewed with a power of 1000 diameters, each appears to be made up of still smaller granules. This body is represented at *f* in fig. 3; it was present in almost all of the specimens which I examined from the second case, and in about three-fourths of those from the last case. This part I suspect, from its form and situation, to be the ovary. At any rate it cannot be considered an organ necessary to the existence of the *individual*, since many specimens may be met with in which it is entirely wanting, and those apparently of the same age, insomuch that I have met with two in the same cyst, the one having the ovary, and the other not. This latter is a curious point, if it is usually the case, but I am not aware that it is so. Although the entire absence of this organ in some instances may be considered as evidence in favour of the existence of separate sexes, yet in such cases I have not been able to detect any other difference of form or structure that would warrant this conclusion.

Another appearance which I have frequently observed in these worms, is that of a line of small dots running down the centre of the animal, and at a distance from each other of about half the diameter of the body; these dots are tolerably regular in their arrangement, and are situated apparently at the surface of the animal. I have not observed any traces of a nervous system.

With regard to the structure and mode of formation of the double cyst in which the worm is contained, and the relation which these two bear to each other, I think much yet remains to be learned. The investigation of this part of the subject is one of greater difficulty than that of the worm itself, as it will probably require for its complete elucidation that the cysts should be observed in their different stages of development. For this purpose, however, sufficient opportunities have not yet been afforded, since the greater part of the cysts that I have examined appear to have arrived at about the same period of their growth, although I have occasionally seen one of a larger size empty, and split down the middle, as if this were the last stage; whilst, on the other hand, I have met with smaller ones, opaque, and with very thick walls, and sometimes having no worm in their interior, but only a little granular matter; this is, perhaps, an early stage. But these are exceptions to the general form, which are not sufficiently numerous to enable me confidently to draw inferences from them. But although one general form of cyst has prevailed, still this has presented so many varieties in minute points, as to convince me that if these were collected and well arranged, much light might be thrown on the question.

This part of the subject, however, if pursued, would lead me to too great a length: at present, therefore, I shall content myself with selecting from a number of drawings that I have made to illustrate these points, one which shows the form of cyst that I have most frequently met with. This is seen in fig. 2, where *a* represents the outer cyst, *b b*, its two elongated extremities, *c c*, the centres of these, so opaque as to appear black when viewed by transmitted light. These appear to be attached to either extremity of the inner elliptical cyst (as represented at *e*), the walls of which are so transparent as to admit of the worm *f* being seen through them. This form, although the most frequent, is perhaps not the best calculated to show the difference between the inner and outer cysts. This is most distinct in those specimens where the whole of the extremity of the outer cyst is opaque, while the central portion enveloping the inner cyst remains sufficiently transparent to admit of the latter being seen through it.

In most of the specimens from the last case, the whole of the outer cyst was so opaque, that little either of the worm or the inner cyst could be seen, except by dissection. These cysts were much harder than in either of the former cases; and from their greater whiteness, the

speckled appearance which they produced in the muscles was very remarkable. The inner cyst can seldom be displayed by dissection; but I think there can be little doubt of its existence as a separate sac from the outer one; its structure, however, is much more delicate. Once or twice, when it was very firm, I have turned it out entire; but generally only broken portions of it can be extracted. When viewed, however, *in situ*, by transmitted light, its boundaries may be often seen well marked.

I am still doubtful whether the dark parts marked *c c*, fig. 2, belong to the inner or the outer cyst; but, for various reasons, I think to the latter.

One other circumstance I may mention before leaving this part of the subject—namely, that in the last case I found the occurrence of two worms in a single cyst to be quite as frequent as that of a single one; and from one of the former cases I have preserved a specimen containing three. I have not yet been able to gain any particulars with regard to the history of the case from which I obtained the last specimens; but the examination of the body, which was that of a middle-aged man, and emaciated, shewed that the individual had died of phthisis. Crude tubercles were thickly distributed throughout both lungs; and in the upper lobe of each were contained large tubercular cavities, together with others of smaller size in various parts of the lungs. The surfaces of the pleura pulmonalis and costalis were closely united together by firm adhesions, as were also the two surfaces of the pericardium. No other morbid appearances were noticed in the internal organs.

The muscles presented the peculiar speckled appearance produced by the presence of the minute white cysts, in a more marked degree than in the former cases. The superficial muscles were found to contain them in far greater numbers than the deep-seated ones, and especially the broad flat muscles, as the pectoralis major and latissimus dorsi. They were present, however, in a greater or less degree in all the muscles of the trunk and extremities, in those of the eye and external ear, in the tongue and soft palate, the constrictors of the pharynx, and the œsophagus, both the crura, and the radiated portion of the diaphragm, in the levator and external sphincter ani, and the muscles of the urethra. Indeed, the only muscular structures that seemed to be free from them, were the heart and muscular envelope of the stomach, intestines, and bladder, together with one or two other exceptions.

Their unequal distribution in the muscles of the body, and their total absence from certain vital organs possessing the same structure, are interesting points. It does not, however, appear that this exemption extends to all those muscles which are usually considered as involuntary.

As I am not aware that these animalcules have been noticed in any other of the numerous dissecting-rooms in London or elsewhere, except in the case mentioned by Mr. Hilton as having occurred at Guy's*, and probably that also by Mr. Wood, at Bristol†; and as the appearances which they produce have been observed occasionally for some years at St. Bartholomew's (although their nature was not then understood), I cannot help thinking that they may be still overlooked for want of sufficient attention being directed to the subject. I have subjoined, therefore, a drawing, representing the ordinary appearance of a portion of muscle beset with these animalcules (see fig. 1), which, while it may serve to convey an idea of the appearance produced by their presence, will also tend to correct any false estimate that may have been formed of their size, from the very highly magnified views with which these observations are illustrated.

To those who may meet with opportunities of pursuing the subject, the following observations as to the method which I have found best, may not be without use.

To examine the worm, a very thin slice of the muscle, containing about half a dozen cysts, should be placed upon a slip of glass with a drop of water. This being placed on the stage of the microscope, under a lens of a half or a quarter of an inch focus, one of the cysts is to be separated from its attachment to the surrounding cellular tissue, by means of a couple of needles fixed in handles, leaving it, however, adherent

* See Med. Gaz. Feb. 2, 1833.
† Ibid. May 9, 1835.

at one extremity, which serves to fix it, while the other is cut off by a cataract needle, or other fine and sharp instrument, just within the dark line *e*, fig. 2, so as to open the inner cyst, but without injuring the worm. This is the most delicate part of the operation, and requires some practice to effect it dexterously. As soon as the cyst is opened, the worm, which is free within it, generally starts out, from the pressure used during the operation; or its expulsion may be effected by a very gentle pressure upon the opposite extremity of the cyst. Every thing being then removed from the glass except the worm, this is to be covered by a very thin piece of talc, taking care that there is sufficient water between the talc and glass to prevent the worm being injured by pressure. The object may then be examined by a power ranging from 200 to 500 linear measurement, always using daylight in preference to any other. These examinations should be made, if possible, upon the living worms, at least as far as the internal parts are concerned, since the natural appearances are often entirely lost when the worms are dead, or they are replaced by others, which are likely to convey erroneous impressions. The living worms, moreover, will sometimes uncoil themselves, so as to admit of their structure being more clearly seen than when two or three coils are lying over one another, as in the usual position of the animal. The uncoiling, however, may generally be effected by means of a couple of hooked needles.

The cysts are best examined by placing the thinnest possible slice of muscle between two slips of glass, or one of talc and one of glass, and slightly pressing them, so as to distribute the muscle in a thin layer. If the edges of these are surrounded by white paint, so as to prevent evaporation, the specimens may be preserved for several months; but become at length decomposed. This method is far preferable to that of drying and placing them in Canada balsam, which renders the cysts too transparent.

I regret that my observations upon the specimens procured from the last case were cut short by an accident which deprived me for a time of the use of my right hand. This circumstance, together with want of leisure, and the fatigue which accompanies the examination of an object so minute for any length of time, has had the effect of rendering these remarks less perfect than I could wish. As the subject, however, is new, I have thought it better to give them in their present state.

I am, sir,
Your obedient servant,
ARTHUR FARRE, M.B.

Charterhouse Square,
Dec. 2, 1835.

PHYSIOLOGY OF THE FIFTH PAIR OF NERVES

ILLUSTRATED BY A CASE OF CARCINOMATOUS TUMOR DESTROYING THE WHOLE OF THE LEFT NERVUS TRIGEMINUS.

To the Editor of the Medical Gazette.

SIR,

HAVING observed in your journal a short time back some remarks by Mr. Mayo, on the researches concerning the nerves, by Professor Panizza, of Pavia, I find there still exists some difference of opinion relative to the functions of the third or lingual branch of the fifth, and the glosso-pharyngeal branch of the eighth pair of nerves. As the details of the following case, on which I based some physiological observations respecting the senses, read before the Royal Society in the year 1833, appear to furnish very decisive proofs of the functions of the whole of the fifth pair, you will perhaps oblige me by inserting it in the columns of your journal.

In the month of November, 1831, I was called to attend Miss S., aged 55, born of parents in affluent circumstances, and who lived to an advanced age, but in whose family, on the maternal side, there was a disposition to cancer, of which complaint her sister died in 1823, in the fifty-second year of her age. At my first visit to this lady she gave me the following history of her local as well as general health previously to that period. She stated that until the age of fifty years she had usually enjoyed good health, with the exception of an attack of typhus, and afterwards intermittent fever, which however, she added, did not leave behind any results.

In the year 1825 she first perceived a small tumor in the left breast, immediately under the nipple. For some time it occasioned no uneasiness, and during five years remained almost stationary, but at the end of that period it began to enlarge, and ultimately suppurated, and no doubt was entertained by many eminent medical practitioners whom she consulted, that it was decidedly of a scirrhous structure: it did not, however, appear to have excited much constitutional irritation. The loss of some near relatives shortly before this period had produced deep and lasting grief, which preyed incessantly on her mind. In August, 1830, she had an attack of acute rheumatism, from which she did not recover until the month of December following, when she came to London, thinking the change of air and scene might be beneficial to her: but in this hope she was disappointed, having from the same cause relapsed into a state of severe suffering, which during the space of six weeks prevented her returning home. It was about this time that she began to feel an unusual creeping sensation on the left side of her chin, accompanied with numbness and stiffness, and a sort of itching pain. The tip of the tongue was painful and sore, and a constant pain was felt along the whole course of the lingual nerve. The numbness and stiffness gradually extended towards the cheek and left eye, until the month of July, when having received a shock by unexpectedly hearing of the death of an intimate friend, she was greatly alarmed at finding the left eye had turned obliquely inwards, and she subsequently observed that all objects appeared double. The eye regained its natural position for a short time, after which the obliquity permanently returned, and was attended with a sense of pressure at the bottom of the orbit. Soon afterwards the whole of the left side of the head, face, nostril, mouth, and tongue, were deprived of sensation, while their muscular motion remained entire. At this time blisters placed on the temples produced considerable inflammation, but did not excite the slightest pain, and she stated that fomentations which had been applied to the face too hot, scalded it without her consciousness. The globe of the left eye, which, independently of its obliquity, appeared healthy, was quite insensible to the touch, and she felt a conviction that it might be punctured with a sharp instrument without pain. At first, any object brought within its axis of vision appeared quite natural, but for some time before her death the optic nerve had lost its faculty of distinguishing colours, persons appearing to the eye, as she expressed herself, "white as statues." The left nostril could not be stimulated by the most acrid substances. Neither snuff nor ammonia produced the slightest impression on the Schneiderian membrane, nor could any irritant excite an effort to sneeze, although the sense of smell remained unimpaired. The tongue and fauces on the affected side were also perfectly insensible to touch or taste. A little acetic acid was placed on the tongue, but she could neither detect its quality nor even perceive its presence. She always masticated her food on the healthy side. During this process she often injured unconsciously the other side with her teeth, which were likewise utterly deprived of sensation, and small particles of food frequently lodged at the left angle of the mouth, without her being aware of it. Latterly deglutition became extremely difficult; solid aliments required to be pushed over the root of the tongue, and liquids frequently returned unperceived by her from the left side of the mouth. She at length became slightly deaf in the left ear. For a long time previous to her death a wasting of the left temporal muscle was observed, producing the appearance of a protuberance in the situation of the temporal ridge of the frontal bone. A few days before her death a perceptible alteration was taking place in the structure of the diseased eye, which was probably occasioned by flies, and other foreign substances, of whose presence she was unconscious, and which caused a slight inflammation of the external membranes. The pain which attended these symptoms was exceedingly acute, and almost without intermission day or night, until, after two years of the most agonizing suffering, she died August 27, 1833

During the treatment of this case none of the remedies suggested afforded any relief, although I had the advantage of consulting, in succession, Dr. James Johnson, Dr. Roget, Sir Benjamin Brodie, and Sir Charles Bell.

Post-mortem Examination.—The head was well formed, the pia mater very vascular, and a small portion of coagulated lymph was found between this membrane and the arachnoid. The lateral ventricles were full of serous fluid. The foramen of Monro unusually large. The plexus choroides were somewhat displaced. On raising the anterior lobes of the brain, a large tumor was seen occupying the left cerebral fossa of the sphenoid and temporal bones, to which it was firmly attached, completely obliterating the foramina rotundum, ovale, and the greater part of the foramen lacerum anterius. It extended inwards to the sella turcica, pushing up the optic nerve, which it had slightly flattened. It rested posteriorly on the superior petrosal sinus and pons varolii, where ulceration had taken place. The remaining portion of the brain appeared healthy.

Remarks.—The extensive distribution of the fifth pair of nerves, its connexion with, and influence on, the senses, render its functions an object of considerable physiological interest; but the situation and primary division of its trunk within the skull, render any experiments on it difficult or impossible, without endangering life. In the case just related the whole of the fifth nerve was completely destroyed, whilst both the eye and the nostrils of that side had lost their sense of touch, retaining the faculties of vision and smelling, but in the tongue both the senses of touch and taste were extinguished. It rarely happens that this nerve is destroyed by disease, without involving the surrounding structures, of which insanity is not unfrequently the result. The intellectual powers were, however, unimpaired, and the patient was able to give very accurate descriptions of her feelings. It is probable that the obliquity of the eye was occasioned by the sixth, or abductor nerve, having been involved in the disease; the partial deafness which ultimately occurred by a similar affection of the portio mollis of the seventh pair, and the loss of sensibility to colour, by the compressed and flattened state of the optic nerve. The wasting of the temporal muscle was a natural consequence of its diminished action, mastication having been for some time performed on the opposite side. The eighth pair of nerves lying at some distance from the seat of disease, were not implicated. The senses of touch and taste, which in this case were quite abolished on the left side, cannot therefore be ascribed to the glosso-pharyngeal nerve, which was uninjured, but must depend on the trigeminal, which was obliterated.

I am, sir,
Your obedient servant,
John Bishop.

38, Bernard-Street,
Russell-Square, Nov. 25, 1835.

P.S. The above case does not appear to accord with the views of Mr. Noble, contained in a recent number of your journal, the common sensibility of the tongue having been extinguished with the senses of taste and touch.

SEQUEL OF THE EXTRAORDINARY CASE OF ABSTINENCE AT AYR.

POST-MORTEM APPEARANCES.

To the Editor of the Medical Gazette.

Sir,

In the course of the judicial investigation into the accident at Kilgramie coal works, it was found necessary to disinter and inspect the body of John Brown, whose case you were so kind as to record in your journal of the 21st instant. If you think the following report of the appearances ten days after death worthy of insertion, you will much oblige, sir,

Your obedient servant,
C. F. Sloan, M.D.

Ayr, Nov. 30, 1835.

On removing the skull, the brain and its membranes presented a healthy appearance; the former, on being cut into, was found firm and fresh, as in a subject twenty-four hours dead; there were fewer marks of blood-vessels than usual; the ventricles contained fluid, just sufficient to moisten their walls; choroid plexus pale. On opening the cavity of the abdomen, the small intestine was seen lying collapsed; the stomach and large intestine moderately distended with air. The omentum had almost disappeared; it could be extended only about two inches, and was en-

tirely destitute of fat. The stomach contained two or three ounces of a greyish fluid; mucous coat entire, and healthy. At the posterior part of the cardiac extremity there was increased redness, which, however, presented no arborescence, was confined to the muscular coat, and had all the appearance of a post-mortem change; peritoneal covering natural. Intestines in every respect normal, containing some fluid resembling that in the stomach, but darker, as if mixed with coal-dust, which he might have swallowed with the water. About three inches from the anus this fluid terminated in the dark fæces which he voided during life. There was no trace of bile before arriving at this black matter, which seemed to owe part of its colour to that secretion. Liver rather small, but natural in colour and consistence. Gall-bladder distended with bile; no mechanical obstruction. Spleen dark-coloured; easily ruptured. Kidneys healthy. Bladder containing about half a pint of urine. Heart small and pale; no trace of fat. Pericardium containing about ℨj. of serum. Lungs affected with melanosis.

The following particulars, which I have learned since my first communication, may perhaps prove interesting:—Brown never suffered from his urine becoming acrid. On being brought out of the pit he was aware of the time he had been confined, having reckoned by the working of the men in another pit. He was lying several feet above the level of the place where the lamps of the workmen were extinguished.

STARVATION IN A COAL-MINE.

EIGHT MEN AND A BOY SHUT UP FOR EIGHT DAYS.

To the Editor of the Medical Gazette.

Sir,

The remarkable case of abstinence reported by Dr. Sloan in the last number but one of your journal, brought to my recollection a similar occurrence which took place in our neighbourhood upwards of twenty years back; and as the particulars were mentioned to me by one of the sufferers, I have thought that a brief narration of them would not be uninteresting to some of your readers. Although conclusions may be deduced from it somewhat similar to those arrived at by Dr. Sloan in his case, yet I think that there is evidence sufficient to prove that the aphorism of Hippocrates is far from being true, which states that "when a person in health abstains from food for seven days, even though he received nourishment at the end of that period, he never survives."

You would oblige me by correcting a mistake which has inadvertently crept into the remarks suggested by my case of hydrophobia, as it completely perverts the idea I intended to convey. In page 269, line 27 of the first column, *instead of* "a measure would have been altogether unjustifiable," *read*, "a measure would *not* have been altogether unjustifiable."—I am, sir,

Your obedient servant,

Charles Thornhill.

Darlaston, Staffordshire, Dec. 2, 1835.

John Taylor, ætatis 45, with seventeen others, on the 10th of August, 1813, went to work as usual in one of the pits of the Moorcroft colliery, belonging to Sir Joseph Scott, Bart. They descended the shaft at about six in the morning, and had been at work little more than an hour, when a large piece of rock fell from the roof into one of the avenues leading to the gateway, and almost completely blocked up the road. The lump of rock, together with the sand which accompanied its fall, was several tons in weight; and the men experienced great difficulty in making clear the passage. They had not long returned to their wonted labour when they were again alarmed by a loud rumbling noise, after which came an immense fall of sand; and they made a general movement towards the bottom of the shaft. A rapid escape was effected by those who were nearest the gateway; but the majority were less fortunate. One person, named Keeling, being a cripple, was buried beneath the falling earth; but Taylor and nine others (viz. eight men and one boy) perceived there was no hope of safety for them, and accordingly retreated into one of the side passages, where they were shut up for the space of *eight days*.

The avenue in which they were confined was about 30 yards long, 12 feet wide, and 7 feet high; but considering it insecure, in consequence of finding here and there large stones and heaps of sand, they explored a way into one of the old openings, about eight yards square, from which they feared to proceed lest there should be a further falling in of the sand. They had two or three candles which afforded them light for a few hours; and knowing that they could not be released from their situation unless a new gateway, parallel with the old one, were driven, and feeling assured that their friends would make this effort, they commenced excavating from within in the most probable, and, as it providentially happened, in the right direction; they actually succeeded, by working alternately, in making a narrow passage through the solid rock to the distance of several yards. They were provided with no kind of food; and the only support within their reach was a little water which constantly dropped from the roof, and which they partook of by stooping down and licking up from the furrows that had been made by their feet in the sandy floor. One man had about half an ounce of tobacco, but he could not prevail upon any of his companions to join him in the use of it.

No complaint of hunger fell from the lips of any except the boy, who was heard to cry several times; but they were all troubled with a sense of sickness, and with excessive thirst. Taylor states that he made water freely and regularly, though his bowels were not acted upon at all during his confinement; but that some of the others had one or more dejections.

After the first two or three days they suffered much from cold, and having on no clothing except their breeches, shoes, and stockings, they sat in a circle close to each other, in order to keep up animal heat. Taylor was so benumbed in his lower extremities, that at length he could not raise himself up without assistance; the rest also complained of numbness, but were able to move about at pleasure. During the whole period Taylor slept but once, though the others were sleeping and snoring nearly all the latter part of the time. They were all troubled with ocular delusions, which evidently arose from their physical debility and want of sustenance.

They heard the men at work for their release a day or two before they were emancipated from their prison-house; and they made frequent signals with the pike to encourage them to proceed more briskly in their labour. By the morning of the eighth day the miners had driven a fresh gateway through the solid coal, which was upwards of 120 yards in length, and they reached the sufferers at about mid-day. The air was only allowed to enter at first by a small aperture, lest there should be any accumulation of hydrogen gas, and a consequent explosion.

A small quantity of gruel was portioned out to each, and repeated two or three times in as many hours; and having been furnished with a complete change of warm linen, they were wrapped in blankets, brought up the shaft, and conveyed to each of their homes in a close carriage. Their weakness was excessive, and they were confined to their beds for several days. They were allowed nothing but gruel for the first two days; afterwards a little light pudding and broth; and on the sixth day they were supplied with a due proportion of porter.

The whole of the sufferers are at the present time living in the enjoyment of tolerable health, with the exception of the youth, who was burnt to death in the collieries about twelve months ago.

OBSERVATIONS AND RESEARCHES

ON

A NEW METHOD OF CURING CANCER.

By Alexander Ure, M.D., M.R.C.S.

Late House-Surgeon to the Royal Infirmary at Glasgow.

The chloride of zinc has hitherto been turned to little or no account by the medical world. Very recently, however, Dr. Canquoin, of Paris, has published some interesting papers, pointing out its peculiar characteristic properties, and the manner of applying it with skill and apparent success in many cancerous affections.

His first memoir on the subject was presented to the Academy of Medicine of Paris on the 24th November, 1834. It contained a very general and some-

what enigmatic account of his method. He states, that it was as far back as the year 1824 he discovered the valuable caustic properties of the chloride of zinc, and then thought it might be applied, with safety and advantage, to the treatment of cancer.

In a supplementary memoir* just published by him, which has been forwarded to me by my kind correspondent Dr. Donnellan, of Paris, Dr. Canquoin has fully communicated the principles of his plan of treatment, and has frankly divulged his methods of preparing and applying this chemical agent in the cure of a disease which has heretofore baffled all the resources of the healing art. This publication details a great variety of cancerous cases, where the chloride of zinc has been applied with perfect success. He, however, cautions the profession that practice alone can guide the surgeon in its judicious application, and that, without minute study and discrimination, it may prove inefficacious, or even prejudical. Hence a failure in unskilful hands ought to be no reason for regarding the method with distrust.

Dr. Canquoin has shown that the chloride of zinc, by itself, is an unmanageable application, but that certain combinations of it with other substances furnish a caustic which may be advantageously employed in those cases of cancerous degeneration in which surgical operation affords no hopes of relief, and arsenic, the medicine usually employed, is fraught with no little danger.

The superiority of this phagedenic paste (for so the preparation may be justly named) over every other caustic, consists in its susceptibility of being applied over very extensive surfaces without any risk of injury from absorption, and in its being available wherever the surgeon's hand can reach. The depth to which it will corrode the morbid texture can always be estimated beforehand; its action is unfailing; the separation of the eschar is prompt; and it imparts an excellent character to the sore, and soundness to the suppuration. The favourable modification of the tissues, the rapidity with which cicatrization follows, and the mildness of the general phenomena that accompany its action, are additional recommendations.

To the reproaches urged against him, of having so long carried on, in a clandestine manner, the treatment of one of the most grievous ills that flesh is heir to, he ingenuously replies, that, as he was fully persuaded of arriving at the knowledge of a certain means of destroying cancerous tumors, which should altogether supersede the employment of arsenic, the sole remedy in which any reliance had been hitherto placed, but of whose fatal effects an example had occurred in his own practice, he deemed it his duty to determine with precision its efficiency by a sufficient variety of well-established cures, before submitting it to the tribunal of the public. Thus supported by uncontrovertible evidence, he would be able to conquer incredulity and disarm malevolence.

Such, indeed, he foresaw to be the only proper course to pursue, in order to secure to himself the honour of an invention that must be regarded as a most valuable contribution to science. He was conscious that, had he published his method in a less mature state, he would have been exposed on all hands to have had his plans cavilled at, and possibly placed in jeopardy, by individuals more actuated by jealousy than the love of professional improvement. The injudicious experiments of such persons might tend to impugn the veracity of his results, and compromise the introduction of a practice fraught with eminent service to the healing art.

The leading proposition enunciated in his first memoir, is that to cure a cancerous affection, the object is not merely to destroy the diseased part, but at the same time to modify more or less profoundly the abnormal vitality of the subjacent tissues; failing which, the cancer will be reproduced. This twofold result may be obtained by means of the arsenical preparations and the chloride of antimony; but the impossibility of foretelling, in every case, whether the former might not be productive of fatal consequences, and the violent constitutional disturbances, such as the syncope, intermittence of the pulse, vomiting, alvine dejections, &c. which the latter has frequently occasioned when applied over a wide surface, have led prudent surgeons to abandon their employment. An unobjectionable substitute for them has been found in the chloride of zinc,

* Mémoire sur un Nouveau Mode de Traitement des Affections Cancéreuses, par Dr. Canquoin. Paris, 1835.

as it fulfils the above indications, and is free from the serious inconveniences involved in their use. The rapid deliquescence of this chloride formed the main obstacle to its application. By rendering it extremely difficult to manage, and by causing its conversion into a hydrochlorate, impaired its power. When used alone, in the form of small fragments or in powder, it was equally impossible to set limits to its operation, or to define the injury which might accrue from its application. To obviate these evils, Dr. C. invented a paste, formed by mixing it with a certain portion of gum, or, what is still better, flour; a mixture perfectly adapted for the purpose, eroding the tissues from half a line to two inches in depth, according to the thickness given to the layer employed and to the prolongation of its contact with the diseased surface, and which never extends its action beyond the space it covers. Thus is its operation, both as to superficial extent and profundity, most entirely under the control of the surgeon.

Chemists offer the three following modes of preparing the chloride of zinc, which I have made the subject of comparative experiments:—

1st, The neutral solution of zinc in muriatic acid may be evaporated to dryness, and fused. Or it may be obtained, 2dly, By distilling in a retort a mixture of one part of zinc with four of corrosive sublimate. Or, 3dly, by exposing to a strong heat, in an earthen retort, a mixture of six parts of decrepitated sea-salt with seven of dry sulphate of zinc; this process requires a red heat to separate the chloride by double decomposition. The product of distillation is of a greyish-white colour, translucent like wax; it enters into fusion a little above 212° Fahr., and becomes, on cooling, at first viscous, then solid.

The only notice recorded of its habitudes with organic matter, is the single fact which Black has stated, of its forming with a concentrated solution of glue a substitute for birdlime, preferable to the ordinary birdlime, in so far as it does not dry up, and may be washed off with water. The following facts have occurred in my experiments:—

When a few drops of a strong solution of the muriate of zinc are added to the albumen of the egg, a white viscid compound is formed, which, when put into water, falls to the bottom, and remains undissolved. A layer of this substance closely resembles the eschar following the application of the phagedenic paste to an ulcerated surface; and it dries in the air into a brittle, horny mass.

The solution of muriate of zinc is a good test of albumen, though not so delicate as that of corrosive sublimate.

A single drop of it produces cloudiness in a dilute solution of gelatine.

With serum of blood it forms a viscid cream-coloured compound, analogous to that with white of egg. The liquid expressed from serum of blood coagulated by heat, becomes turbid on the addition of the muriatic solution.

A portion of the crassamentum, previously washed in distilled water, and then macerated in a solution of one part of the salt to ten of water, acquires in the course of some hours a firm coriaceous consistence and aspect, not unlike hepatized lung, or spleen that had been for a while immersed in the antiseptic saline solutions of the anatomist.

The preceding results may serve in some measure to illustrate its action on the living tissue. Chloride of zinc, either by itself or in the pasty form proposed by Dr. Canquoin, applied to an ulcerated surface or portion of the chorion, stripped of its epidermic covering, constitutes with the organic texture, replete in albumino-gelatinous matter, a white inanimate crust or eschar. The great affinity of the chloride for albumen, may in some measure account for its energetic action on scirrhous and other morbid formations in which that principle predominates. M. Foy, in an elaborate analysis, has shown that scirrhus contains 42, and encephaloid tissue no less than 47 per cent. of albumen *. From seven to eight hours are requisite for effecting this change. In the layer thus acted upon, the chloride still retains some of its primitive energy, and modifies the subjacent parts, causing them at last to detach the slough. The chloride applied to the surface of foul cancerous sores, will instantaneously effect the decomposition of the carbonate and hydrosulphuret of ammonia, which Morin has ascertained to exist in the sanies which they secrete,

* Archives générales de Médecine, tom. xvii. p. 185.

and to whose presence its fœtor is in all probability owing. The renovated surface, after the separations of the slough, ceases to generate these morbid products.

This seems a probable way of explaining its physiological agency, and one, moreover, equally applicable to all analogous escharotics.

The following is a summary view of its effects, compared with other caustics:—

The painful sensation produced by chloride of zinc usually ceases in twenty-four hours; whereas that excited by arsenious acid, sulphate of copper, or chloride of antimony, commonly continues for two days. In the former case the eschar produced is white, very firm, and varies in thickness according to circumstances. The ordinary time required for its separation, according to Canquoin, is from eight to twelve days, but I have seen it completed in a shorter time. With most other caustics, from twenty-five to thirty days elapse before the sloughs are entirely removed.

Besides the pain, each caustic, after its application, gives rise to certain general phenomena, such as a local tumefaction, more or less erysipelatous in its nature, with discharge of thin fluid serum. After fused potash, and especially arsenic, these appearances are very conspicuous; whereas with the chloride of zinc they are very slight.

A degree of fever is by no means an unfrequent occurrence after its application, but that is common to arsenic, and, indeed, every other caustic, when largely applied.

Dr. Canquoin states, that the chloride of zinc, in addition to its utility in the treatment of cancer, may advantageously serve as a substitute for moxa, and as a useful means of cauterization in scirrhous affections of the neck of the uterus, and caries. I observe it has likewise been employed with success by Dr. Hanke, of Breslau, in cases of nævus maternus, fungus hæmatodes, malignant pustule, and syphilitic ulceration with a carcinomatous appearance. Dr. H., it may be remarked, employs the chloride in powder, and tries to prevent its spreading by a covering of adhesive plaister.

It now remains to notice Dr. Canquoin's manner of preparing the phagedenic paste—the mode of its application, with certain peculiarities to be attended to according to circumstances—the cases in which benefit is to be expected from its employment—and finally, the appropriate internal treatment to be observed.

[To be continued.]

SOME REMARKS

ON THE

NERVOUS SYSTEM OF INSECTS.

To the Editor of the Medical Gazette.

SIR,

I TRUST that Mr. Roberton will excuse the liberty I take in venturing to suggest some little anatomical inaccuracies in the following paragraph of his second letter on Dr. Elliotson's views respecting life and mind.

"Third—'Superiority of mind in the animal creation is exactly commensurate with superiority of brain.' This is an incautious assertion, if by 'superiority' is to be understood largeness, or some particular form of the head, since these are at present points confessedly of doubtful disputation, and likely so to continue. Besides, Dr. Elliotson is well aware that insects, the most reasoning, perhaps, of all the inferior creatures, have properly no brain, a slight enlargement of the upper extremity of the spinal marrow being all that stands for that organ."

This latter assertion, anatomy tells us, is quite unfounded: it tells us that insects have a nervous organization superior to every other tribe of invertebrate animals,—that in this respect the whole division of the Articulata is above the Mollusca,—and that the restricted modern class "Insecta" stands at the head of all. The very circumstance of Mr. Roberton's calling the ganglionic cord the spinal marrow, would elevate them to this high position.

I will, as briefly as possible, detail a mere sketch of their nervous system, with a view of partly proving these statements.

The nervous system is found in every order of the true Insecta to consist of a ganglionic nervous cord running along the abdominal surface of the body, and a supra-œsophageal nervous mass, called by Cuvier

"the brain *," and by many anatomists the cerebral ganglion †. This nervous cord consists of a varied number of ganglia, giving off lateral nervous filaments, and connected to each other by longitudinal sensitive columns. A motor tract has also been recently described by Mr. Newport ‡, passing along the dorsal surface of these columns, and giving off lateral nervous branches. There is also a minute nerve, which I have traced in the larva of the *Saturnia pavonia minor*, passing off at the angle of separation between the divided longitudinal cords connecting the fourth and fifth abdominal ganglia, and which, midway between these ganglia, divides into a right and left transverse filament, each of which had connexion with the anterior of the lateral nerves arising from the ganglia themselves. Some have considered these nerves as sympathetic, others as motor; but Mr. Newport argues, and I think very justly, that from their distribution they must be respiratory nerves.

These anatomical details, and many other collateral circumstances, would tend to show a great analogy between the ganglionic cord of insects and the spinal cord of the Vertebrata. Its situation in the body, and the relative position of the motor and sensitive columns, are different in the two groups; but a moment's reflection will render the reason for this inverted position obvious.

The cerebral ganglion is situated above the œsophagus: I have generally found it of a bilobate form, and it is connected with or produced from the first sub-œsophageal ganglion by two minute lateral commissures; from it are given off nerves both for sensation and motion, viz. to the antennæ, eyes, maxillæ, palpi, &c.; a remarkable single nerve also has its origin from it, which passes backwards along the dorsal surface of the œsophagus, giving off branching filaments to the stomach: this is the recurrent nerve of Lyonnet. It is considered by Straus Durckheim as the rudiments of a sympathetic system §, and by Mr. Newport as the analogue of the par vagum. The form of this cerebral ganglion I have found very remarkable in the bee tribe. It was of immense proportional size; it had a distinctly bilobate appearance; a deep fissure traversed it transversely; from its anterior part were given off two nerves, which passed forwards to the base of the antennæ, and had their origin marked by a very distinct conical-shaped ganglionic enlargement. Surely this cerebral ganglion (the rudimentary optic lobes of the inferior Vertebrata, the subsequent tubercula quadrigemina of the Mammalia), cannot be considered "as a slight enlargement of the upper extremity of the spinal marrow." It is singular that Mr. Roberton should admit the importance of the ganglionic cord, by calling it the spinal marrow, and destroy the importance of the cerebral ganglion, by not considering it analogous to a brain.

Thus I have endeavoured to show the existence of motor and sensitive columns in the spinal cord, a respiratory system of nerves, a par vagum, a remarkably developed cerebral ganglion, and consequently a highly-developed nervous organization generally. I refrain from entering more minutely into these subjects, as they will be detailed at greater length on a future occasion.

I remain, sir,
Your most obedient servant,
JOHN ANDERSON.

George Street, Richmond,
Nov. 23, 1835.

* Règne Animal.
† Amongst others, Carus, in his Anatomie Comparée.
‡ Philosophical Transactions for 1832 and 1834.
§ Considerations générales sur l'Anatomie Comparée des Animaux Articulés, &c.

ON THE NATURE OF THE NERVOUS INFLUENCE;

IN REPLY TO DR. WILLIAMS.

To the Editor of the Medical Gazette.

SIR,

ALTHOUGH a reference to the opinions of others is still a favourite argument with Dr. Williams, if such it can be called, he has changed his mode of reply; but I am sure he will acknowledge not for the better, when he finds he has taken pains to oppose opinions which I never entertained, and some of which I have put myself to much trouble to refute. Before proceeding farther, however, I may remark that I cannot see how what he now says respecting his quotation from Dr. Alison's work af-

fects my observation on what he said before, farther than that it confines it to the writers he enumerates, who, having paid particular attention to the subject, may surely be allowed to be as well acquainted as others with the physiological knowledge of Haller. The words to which I replied were, "that the province of the nervous system was more accurately understood by Haller."

In the commencement of the present discussion, I observed that one of the inconveniences I had laboured under, was that from the protracted nature of the investigation in which I had been engaged, and the number of publications among which its results are distributed--namely, besides my Inquiry into the Laws of the Vital Functions, eleven papers in the Philosophical Transactions, during the course of twenty years—those who have replied to any one part have often been unacquainted with others with which that part was immediately connected. Dr. Williams's present reply is a good illustration of this observation.

I have never maintained that after the removal of the brain and spinal marrow the capillaries would not gradually lose their power. How could it be otherwise? I only stated that the animal being properly prepared, the removal of the brain and spinal marrow produces no immediate effect whatever on the circulation, either with respect to the heart or vessels; proving that the effect of their removal on these organs is not direct, that the power of the latter does not immediately depend on the former organs, because in that case their action would have immediately ceased, the only object I had in view; which Dr. Williams evidently loses sight of when he speaks of their action having been observed for a longer time than I had observed it.

Dr. Williams's reply to what I say of Chossat's experiments, is the consequence of his not being aware that the facts I had stated show that the destruction of the semilunar ganglion must necessarily produce similar effects with that of a great portion of the brain or spinal marrow. Had Dr. Williams been acquainted with my opinions, he would have seen nothing at variance with them in any of the experiments of Chossat to which he refers.

With respect to the division of the spinal marrow, so far from conceiving that it could affect the maintenance of the temperature, as Dr. Williams intimates, I have been at much pains to prove that neither this nor any other vital function of the spinal marrow is affected by dividing it, and to explain why this is necessarily the case, except in as far as relates to the disturbance caused by the minute portion of that organ necessarily bruised in making the division, which certainly can have no sensible effect on the temperature, although I could trace its slight effect on the function of the stomach. I have already had occasion to allude to this subject in the present discussion. Enough has been said to show how much Dr. Williams has mistaken my opinions, and I shall only add a few observations, of a different kind, on the more essential parts of his reply.

I THINK Dr. Williams will at once see that his reference to certain properties of electricity, as they appear in the inanimate world, can have no weight in the present discussion, since one of the fundamental principles in all I have said is, that its properties are modified by those of life, under the influence of which it operates in the living animal, as it operates in the magnet under the influence of the properties peculiar to it. If Dr. Williams will attempt to apply his mode of reasoning to the electricity of the magnet, he will perceive how inapplicable it must necessarily be to that of the living animal. Does not terrestrial electricity uniformly affect the common electrometer? Yet the electricity peculiar to electric animals, respecting the nature of which there is no difference of opinion, produces no effect on it. The manner in which electricity is modified by the powers of the living animal, is a question altogether different from that before us.

Such objections cannot at all affect the inference from the fact, that electricity performs all the functions of the nervous influence; for it does not, as Dr. Williams states, "affect certain vital functions in a definite manner," but performs all the functions of the nervous influence as perfectly as they are performed by that influence itself, and that during the whole remaining life of the animal. Both the function of the stomach and that of the lungs are as well performed, and their healthy structure as perfectly maintained, as

while the nervous influence remains, the structure of the latter being such as admits of our judging even of its slight deviations.

When the nervous influence is withdrawn, without the substitution of electricity, the secretion of gastric juice ceases, and the lungs gradually become so obstructed as to be the immediate cause of death, their structure in many parts being wholly destroyed. When electricity is substituted for the nervous influence, as we cannot apply it in the way in which it is applied by nature, it at length (that is, long after their structure would have been impaired, had electricity not been employed) excites inflammation, and the animal dies of inflammation of the lungs, but without change of structure, precisely as it would do were the same degree of voltaic electricity, or any other powerful cause of irritation, affecting the lungs, applied while the nervous influence is unimpaired. In short, as far as the functions of the nervous influence are concerned, the animal is in all respects in the same state as if that influence had been entire.

None of the circumstances mentioned by Dr. Williams can at all affect the inference from these facts, of which I think Dr. Williams will be aware when he considers the subject more at leisure, —an inference which he will find it impossible to get rid of, except by disproving the facts on which it is founded. Let him show how this inference may be refuted, and he will do more respecting the question before us, than by writing volumes in the style of his present communications; for whatever else he does, this he must do, not only in order to disprove my position, but even to render it doubtful, because the ground I take is, that while the facts remain, the inference is unavoidable. It is sufficient to refute all I have said, to show that such is not the case.

This reduces the discussion to a very narrow compass; all else is superfluous, because it can be of no avail. Nothing but this is required, and for this nothing can be substituted. If this cannot be done, my inference must necessarily be regarded as established.

If it be our sole aim to arrive at truth, we must keep the attention steadily directed to the point on which the discussion rests. All deviation from it tends only to perplex the reader, and consequently, as I have already had occasion to remark, to obscure the truth; in arriving at which "the latest and best authority," unless it be proved to be a correct authority, will give us no assistance.

With respect to Professor Faraday's researches, which Dr. Williams so justly commends, I believe that he himself in no degree views them as in opposition to my opinion. If they had been so, I presume he would not have taken the trouble, as he was so good as to do at my request, to try to cause the electric nature of the nervous influence to become sensible to the very delicate tests discovered by him; and I am permitted by him to repeat the spirit of the observation he made on the failure of the attempt. He said, we have now failed, but I have great expectation we shall sooner or later succeed; for, from the time that it was shown that electricity could perform the functions of the nervous influence, I have had no doubt of their very close relation, and probably as effects of one common cause. And, indeed, his great knowledge of the subject of electricity enabled him to state the cause of our failure, without having recourse to the incredible supposition that there are two distinct principles of action; one of which is capable of all the functions of the other.

I believe it will not be necessary to say much more of Dr. Williams's observations on the nature of electricity or chemical action. He forgets that carbon and oxygen will not combine by mere juxtaposition, either in the living blood or the laboratory of the chemist. In the latter they may be made to combine by the electric power; in the former, as appears from facts I have stated, by the nervous influence; so that here also we see electricity possessing the same property as that influence.

I shall only add, that in this long discussion I have obtained no reply to either of the only two questions I put to Dr. Williams—namely, seeing that voltaic electricity performs all the functions of the nervous influence, can they be different principles? — and seeing that the nervous influence is capable of existing in other textures than that of the nervous system, can it be a vital power, properly so called?

As, in all he has written, he makes no

reply to either of these questions, I am left to conclude that no other replies but those I have given can be made.

Had the style of Dr. Williams's papers been as well directed to the points in discussion, as it has been gentlemanlike, our discussion would have been more useful. No degree of ingenuity can avail in opposition to simple matter of fact. A reply, to be of any weight, must be direct, and to the facts. The want of such a reply in the present instance, may be regarded as a presumption of its impossibility; because, by a man of Dr. Williams's talents, it will always be preferred when it is possible.

As in my last communication I stated the grounds of my opinions respecting the nature of the different powers of the animal body, I shall in a communication next week state my opinions respecting the sources of these powers, and the relation they bear to each other. These two communications will comprehend the general results of all I have done on the subject, and may at least be the means of preventing opinions being attributed to me which I not only never entertained, but put myself to much trouble to refute.

I am, sir,
Your obedient servant,
A. P. W. Philip.

Cavendish Square,
Dec. 8, 1835.

FAULTY and DEFECTIVE SYSTEM OF OUR HOSPITALS

IN RESPECT TO NURSES AND SISTERS.

To the Editor of the Medical Gazette.

Sir,

It is a constant theme of well-grounded complaint among medical men, that their treatment of disease is frequently frustrated by the ignorance or carelessness of the nurse who superintends the patient during their absence. Every practitioner can call to mind numerous instances of prolonged cases and untoward terminations, justly attributable to this cause. My object is not merely to deplore a state of things so generally acknowledged, but likewise to point out what I consider one fertile source of the mischief.

When we reflect upon the skill, discretion, and kindness, which should form the attributes of a good nurse, we readily admit that the knowledge of her art cannot be intuitive, but must result from careful training and long experience among the sick. I should have imagined that our large hospitals would have formed the best schools for instructing females in the important duties of nursing the sick; but so faulty are their arrangements in this particular, however admirable in many others, that the matter of surprise is, that a good nurse should ever be found within their walls. My observations apply especially to St. Bartholomew's, at which alone I have had personal opportunities of observing the injurious effects of the system; but I believe the same remarks will more or less extend to most of the others.

In the first place, the *salaries* are far too small to induce efficient and respectable females to undertake the duties longer than sheer necessity compels them. The *sisters* even, upon whom the whole responsibility of the ward devolves, receive so miserable a pittance that it has excited the surprise of many to find, occasionally, very respectable women in the employment, while it affords a sufficient reason for the carelessness and ignorance of the majority. But the inferior *nurses*, upon whom many of the comforts of the patients mainly depend, are so wretchedly remunerated, that even bare subsistence seems to me hardly possible*. I will ask, can a greater reproach attach to these wealthy institutions than this niggardly proceeding—offering, as it does, every temptation to extortion from the patients or their friends?

Next, *their duties are by far too onerous* to permit them to pay proper attention to the patients. This remark applies only to the nurses, for I do not consider that if the sisters were better paid they are too much worked. The nurses (two in number for each ward) are required to sit up alternate nights, and have to undergo various household employment in the wards during the days; and no doubt, owing to their

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* The sisters receive from 14*s*. to 16*s*. per week, and the nurses 7*s*.; and I believe in the accident-wards rather more. The nurses also have a small portion of food allowed them, but far short of sufficient for subsistence.

miserable remuneration, would be glad to do any thing elsewhere to increase their pittance. I do not constitute myself a judge of the precise duties each nurse ought to be called upon to perform, but I can speak from experience, that, under the present arrangement, they have too much to do. Many nights have I seen the night-nurse, exhausted from her previous labours, quietly enjoying that repose in which she stood so much in need of, heedless of the cry of pain or the call of thirst—forgetful of the administration of medicine, or the renewal of some necessary application; and is not the night season that in which her duties are most urgently called for, whether we consider the usual exacerbation of diseases at that period, or the sleepiness of all around, except those who require her assistance? So impressed was I with the truth of what I have stated, that during my attendance at the hospital, when I had a patient whose case urgently demanded attention during the night, I felt it always my duty to remain with him, rather than leave him to the care of one of these exhausted females. Another evil of great magnitude, resulting from this system of over-working, is the engendering of disease, and the habit these women acquire of stimulating themselves to wakefulness by ardent spirits. In several cases I have distinctly traced serious illnesses of the nurses to excessive exertion and want of rest; and have seen diseases fatal, or terminate most seriously, from these causes. A few years since, a gentleman who was dressing with me, united with me in presenting to the Governors a memorial of the wretched state in which the nurses were placed by the arrangements of the hospital, and of the magnitude of the evils which must result thence, both to themselves and the patients under their charge. We received no reply, nor do I perceive any considerable amelioration has taken place. It seems to me, in order to be adequate to their duties, the attention of the nurses should be *solely confined to the patients*, and that measures should be taken to *ensure them that rest* which they are otherwise obliged to snatch at the expense of the patient's interest.

Lastly, the mode of their *appointment*, *removal*, and *promotion*, is very faulty; being entirely independent of the medical officers, and vested in the hands of the matron, or steward; who, of course, are perfectly inadequate to judge of their capabilities. The consequence of this system is, that many females are placed in the office who are perfectly unfit, and that several whose conduct is praiseworthy do not meet with the encouragement they deserve. I consider the apathy the medical officers display upon this point is highly discreditable, and perfectly inconsistent with the anxiety they so often display for the welfare of their patients. The appointment of nurses and sisters should rest with them, as should their dismissal or promotion. If this were the case we should not see (as I have done) inefficient, idle, and frequently drunken women, maintained in their places in spite of their unfitness; while those whose conduct has merited approval have been suffered to remain unnoticed. The reason is plain; the matron, or steward, can be no judges of the matter, and must often be the victims of intrigue and deceit. In connexion with this subject, I think it will hardly be believed that these females falling ill, in the service of the institution, so far from being especially provided for during their illness, run great risk, upon their recovery, of finding themselves placeless.

I think I may say the above facts are worthy the attention of our hospital medical officers, and need advance no argument to prove that the existence of this state of things must be hurtful to the reputation and interest of the surgeon himself, and much more so to the comfort and well-doing of the patient, who (in the hospital especially), removed from the soothing and endearing attention of his friends, requires increased care, and feels with more poignancy neglect or unkindness.

It is no reply to me to tell me, that, notwithstanding, many of the hospital nurses are excellent; I have frequently borne witness to the fact: but they are so in spite of the system, not in consequence of it.

In applying these observations to the subject with which I commenced this letter, I ask, if our hospitals are not to furnish us with a supply of well-schooled nurses, where are we to look for it?—and I also ask, do they fulfil this important office? True it is, in private practice, we daily witness instances of the friends of the patient devoting themselves with unceasing assi-

duity to his interest; but, however pleasing this spectacle is to the philanthropist, the medical man knows well the evils too often resulting from well-meaning but ill-timed acts of kindness, to trust implicitly to this source. It is also true that we meet with many excellent hired nurses; but yet these are so few in number, and their terms, in consequence, so exorbitant, as to place them beyond the reach of the middling and working classes, where their services are most required. I believe that if the system of nursing were properly regulated at our hospitals, these would furnish us with women in whom we could trust, and who would be content to give their services for a moderate remuneration.

I trust, sir, you will distinguish the motives which actuate me in making this communication, from that spirit of indiscriminate opposition which is often indulged in for the mere sake of finding fault. I believe I have here pointed out one error in the system of hospital management, the rectifying of which would confer benefit upon the parties concerned, as also upon our profession and the public at large.—I am, sir,

Yours obediently,

John Chatto.

15, Leigh-Street,
Burton Crescent, Nov. 12, 1835.

EFFICACY OF IODINE IN SECONDARY SYPHILIS.

To the Editor of the Medical Gazette.

Sir,

Like most preparations used medicinally, iodine has its friends as well as enemies. That the virtues of this medicine have been in some degree overrated, cannot be disputed by those who have good opportunities of testing its properties. Dr. Addison, our much respected President of the Westminster Medical Society, stated, a few weeks back, during a discussion on this subject, that the physicians of the hospital with which he is professionally associated, had tried the preparations of iodine in all the diseases in which this medicine is supposed to exert a peculiar efficacy, and the result of their experience was to the effect that iodine was all but *inert*. Dr. Elliotson states, as the result of his experience, that iodine is a valuable remedial agent. How are we to reconcile the conflicting opinions (both said to be based on *facts*) of two such eminent physicians? Is it attributable to a difference in the *quality* of the drug used? Dr. Addison says the iodine he used was unadulterated; it was detected in the urine of the patients to whom it was administered. Are we to account for this juxta-position of opinion by supposing that the iodine, in the cases in which Dr. Addison made trial of it, underwent some modification, owing to idiosyncrasy of constitution? If he tried it in a variety of cases, no such inference could be drawn.

In questions involving *theoretical* points we need not feel surprised that great dissimilarity of opinion should exist; but it certainly is, particularly to non-professional individuals, singular, that, on questions purely of *fact*, medical men should come to such opposite conclusions. Perhaps we are justified in believing that both Dr. Elliotson and Dr. Addison err in entertaining *extreme* opinions; Dr. E, in recommending an almost (I was going to say) indiscriminate use of iodine, and Dr. Addison, in denouncing the medicine as inert. Truth generally lies between extremes, and I think in this matter, such is the case. In the course of my practice for the last few years, I have given iodine a fair trial; the result has not realized my anticipations. It certainly is beneficial in many cases; but in a variety of affections in which it is said to be extremely efficacious, I have found it of no benefit. In secondary syphilis I have found this medicine extremely serviceable. I have had under my care a considerable number of cases of this description, in all of which the disease was either cured or considerably alleviated by the exhibition of the hydriodate of potash. In one case which I attended lately, in conjunction with an eminent physician, in which nearly the whole of the body, with the exception of the face, was covered by a venereal eruption, evidently the consequence of the primary disease not having been attacked with mercury, a decided cure was established in three weeks by the combined administration of the hydriodate of potash and sarsaparilla. In the hard periosteal node, the result of my experience coincides

with that of Dr. Williams; I have found the hydriodate of potash attended with decided relief in such cases. Without entering into a minute detail of cases, I may observe that the hydriodate of potash will be found a valuable remedial agent in secondary syphilitic affections. I have not confined my observations to cases which have come under my own care, but I have, with a view to the publication of a treatise on the efficacy of the preparations of iodine in secondary syphilis, carefully observed the practice of others in this disease, and I feel fully warranted in classing iodine amongst our most useful therapeutic agents. I think I may do so without incurring the risk of having imputed to me the charge of entertaining an extreme opinion as to the remedial efficacy of this drug. I do not wish it to be discarded, neither do I wish to see the confidence of the profession in the medicine removed, by its being overvalued. Perhaps I was wrong in saying that Dr. Elliotson recommended an indiscriminate use of iodine. I know, having had the honour of being his pupil, that he has a decided *penchant* for this medicine. Lovers, they tell us, are blind to the faults of their mistresses; perhaps a similar influence is exerted on Dr. Elliotson's mind with reference to the virtues of iodine.

From what I have said, I would not wish it to be inferred that I question the accuracy of Dr. Elliotson's facts,—I believe him to possess a mind capable of close and attentive observation, and power of reasoning equalled by few in the profession.

Should the above observations deserve a corner in your valuable journal, you will oblige me by their insertion.

I am, sir,
Your obedient servant,
FORBES WINSLOW,
Member of the Royal College of Surgeons, London, &c. &c.

33, St. Martin's-le-Grand,
Nov. 29, 1835.

I saw a case, a few weeks back, in which iodine brought on salivation. I am not aware that such an effect has been attributed to the use of this medicine.

MEDICAL GAZETTE.
Saturday, December 12, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri: potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

MEANS OF ESCAPING SUFFOCATION IN HOUSES ON FIRE.

WHENEVER some frightful calamity by fire has taken place—like that lately at Hatfield House, or in Tottenham-court-road, where so many lives were lost—the public are amused for a time with the speculations of ingenious persons who propose various methods for escape; but the interest soon passes off—not to be revived again until after the next conflagration.

The subject, perhaps, is distasteful to most people: the mind naturally revolts from the contemplation of the terrible mischiefs done by fire, and both ears and eyes are shut against the possibility of fatal accidents, and the suggestion of measures of safety. A dream of security is enjoyed, till destruction itself comes upon the sleepers, and wakes them to an awful sense of their condition. The principal precaution taken by some of the most prudent members of the community scarcely goes beyond the insurance of their property, which seems to render them even more than ordinarily careless of their lives. The very frequency of dreadful fires inures them to their carelessness; and seldom is a thought entertained of how persons should act in case the devouring element should approach them.

Those who have recently been attempting to engage public attention to the subject, have scarcely gone farther than recommending certain staples and noosed ropes to be provided, in order to exchange the danger of burning for the risk of being precipitated from a window. Few or none have

recollected that often in such circumstances there are objects to be saved as valuable as, and even dearer than, life itself. When, at Hatfield House, the noble owner of the mansion, with all his retainers around him, stood aghast at the door of his mother's burning room, and none dare enter to rescue the unfortunate lady, living or dead, nobody thought of a simple expedient, by which that object might possibly have been accomplished—the simple application of a wet cloth or handkerchief to the mouth, and boldly venturing in. Unless there were absolute flame to oppose such a step, breathing in the densest smoke could be effected for a few minutes; and what good, even in that short space, might not have been done? Better still if the courageous rescuer should enter on hands and knees, with his mouth and nostrils protected in the manner just mentioned. The expedient is by no means new, however newly it may recur to the public attention; nor is it the less valuable, perhaps, for having been sometimes tried with success.

It is now about ten years since the contrivance of John Roberts, a poor miner, was the subject of general wonder and admiration. This man invented a covering for the head, with glass or talc eyes, and a tubular mouth-piece; with this he was put to the test, both in this country and in France, and under its protection could resist the most suffocating vapours of burning sulphur and resinous matters, shut up in a room for above half an hour, where no other mortal, without such a defence, could exist for half a minute. Why this valuable contrivance has not been more generally adopted we can form no conjecture, except we refer it to that apathy on the part of the public already alluded to. That it has not lost any of its original reputation, or prospect of usefulness, is shown by the Parliamentary Report on Accidents in Mines, just published. There the Safety-hood is mentioned in high terms. The following is what the Committee say:—

"Mr. Roberts produced to your Committee his safety-hood......Your Committee report with pleasure their opinion of its great value, and of the merit of the inventor. The advantages to be obtained by having the safety-hood always ready for use are by no means hypothetical: interesting proofs of what may be effected by its use have been received; and the practicability of saving life after explosions, when no hope remained, has been demonstrated*."

The history of the origin of this contrivance is curious. It is thus related by Roberts himself, in his evidence before the Parliamentary Committee:—

"Did you meet with accidents from carbonic acid gas while at Newbottle colliery?—On one occasion I had nearly met with an accident. In the shaft there were two very large furnaces to draw the foul air out, and to cause a quick current of air to circulate through the works. At this time the firemen, who took care of the furnaces, had thrown a fresh supply of fuel on them: owing to this circumstance, on descending the shaft, I was nearly suffocated. It struck me at the moment to wrap my jacket round my face: it happened to be very moist, and when I got to the bottom of the shaft I found I was very comfortable, and a sudden change had taken place in the apparent state of the air. Some time after I was thinking over the matter, and it occurred to me that the cause was the woollen, and the perspiration with which it was saturated, that preserved me in this instance. From trying further experiments in the same shaft, I found it was the woollen, which I then moistened with my bottle of water. I often used to be turning this matter over in my mind, and was led to make an apparatus to prevent accidents from suffocation, and thus enable the workmen to work, and to have the use of their hands and eyes, in an impure atmosphere†."

* Report: Accidents in Mines, p. viii.
† Ibid.: Minutes of Evidence, p. 261.

The apparatus here mentioned is the Safety-hood, for the invention of which its ingenious author was rewarded with 50 guineas and a medal by the Society of Arts, and 100*l.* by his late Majesty.

Can there be a doubt that the lives of a party mentioned in Mr. G. Stephenson's evidence would have been saved, if even one of the men were provided with a safety-hood.

"With regard to sulphuretted hydrogen gas, there was a case at Newbottle, where there were six men lost by that gas. I imagine it was that kind of gas, for the lamps did burn when the men died. There were three men working in a drift, and one of them said he felt himself unwell: it was quite a separate place from the other part of the workmen, and he thought he would go out; and the other one said he thought he was not well also, and agreed to go out: but they did not go very far when one of them said he must sit down, he could not go any further: they had a light with them, and the other man went on. An alarm was given. Mr. Hill, the viewer, happened to be at the colliery at the time, and immediately prepared to go down the pit with all the men that had any charge in the mine. I think there were five besides himself, and they could not make out the cause, as the lamps burnt, of the men stopping: but Mr. Hill said to his men, "Let us use all the caution we can in proceeding to seek these men." He desired the men, in going forward, not to speak unless they felt a change in their breathing, or any other change that they conceived was a change of the vital powers. They left guides at different places to give the alarm to the shaft, in case there was any thing amiss took place with them. They went on some distance, when at last Mr. Hill spoke and said, "I feel a ringing in my ears: do any of you feel a change?" When he spoke they all thought they felt some change: and he said, "Let us go back." They turned round to go back, and all fell. There were some of them so strong as to give the alarm to the others, and fresh men came running in, and got, I think, two out of the six brought out alive. Mr. Hill was one; he was brought out as a dead man; but he and the other man recovered; the other four that were not got out so readily all died *."

But to return to the subject of fires. The suffocating smoke in burning houses is probably by no means as deleterious as the choke damp, and other occasional gases in mines. The fumes in burning apartments must of course be of a mixed nature, according to the substances accidentally consumed, and the supply of oxygen which is present; but in general they consist of carbonic acid gas, carbonic oxide, carburetted hydrogen, &c., and always leave an interval between them and the floor, which is filled with ordinary atmospheric air. This latter interval is even present where straw, shavings, wet hay, and sulphur, have been ignited near the floor.

We have a valuable analysis, by Mr. Pepys, in which these facts are ascertained. When Roberts was introduced, by way of ordeal, into a room filled with the fumes of burning resin, sulphur, &c., and in which he remained, with free powers of locomotion—principally, indeed, occupied in stirring the fire and keeping the fuel together—he was entrusted with three bottles of mercury, which he was to empty at different altitudes from the floor, in order that, by corking the bottle immediately, specimens of the air, at the given levels, might be secured for examination. The result was, that

The air taken from the middle of the room, as to elevation, appeared to contain the largest quantity of carbonic acid, and the smallest quantity of oxygen; the following being the results of a very accurate analysis: 2 carbonic acid + 18 oxygen + 80 azote = 100*.

If the resultant compound in this case be compared with the destructive gases of mines, it will be perceived how much the balance is in favour of the fumes generated in the conflagration of houses; the more so, as in the latter case there may not often be present sulphurous acid, and the vapours given out in bituminous combustion; so that the gaseous products of burning timber, furniture, &c. may not present such for-

* Report already quoted, p. 120.

* Letter of Mr. Pepys, March 1825.

midable obstacles to respiration. The fact is, that, on correct chemical principles, the nature of the bane, as well as the antidote, are indicated and made apparent. There is no remarkable diminution of oxygen, (according to Mr. Pepys' analysis,) even where these combinations take place; and the noxious effects are chiefly owing to the smoke and vapour, and carbonic acid, with which the air of the burning apartment is loaded. Now the former may be strained, and the latter neutralized, or absorbed. In the construction of Roberts's safety-hood, both these indications are provided for: the mouth-piece consists of a flexible tube, somewhat resembling, on a small scale, an elephant's proboscis; the length is about three feet, and its lower extremity, which is bell-shaped, contains a sponge, moistened with chloruret, or cream of lime, or common water, according to circumstances, or the nature of the medium in which respiration is to be sustained. The noxious gases, on coming in contact with the sponge and its contents, are intercepted, and what air enters the tube is mechanically filtered before admission: add to this, the length of the mouth-piece, which permits the purer air to be inspired from the lower strata of the apartment, and it will readily be admitted that the contrivance is admirably adapted for the purpose for which it is designed. This was the apparatus with which the inventor not only stood the fiery and mephitic ordeal in the instances above mentioned, but thé same with which he entered the old sewer of the Bastile at Paris; which had not been cleansed for thirty-seven years, and cost two men their lives fifteen years previously, when, under the superintendence of distinguished *savans*, it was attempted to be opened.

For minor operations, and where exposure to suffocating vapours may not be requisite for above a few minutes, we understand there is a much simpler contrivance employed by Roberts; one that merely covers the mouth and nostrils with the moistened sponge, secured by wire-work. There is every reason to believe that it would be possible to move for some minutes in the midst of suffocating vapours, with the assistance of so simple an instrument: even the mere application of a wet handkerchief to the mouth and nose, affords much protection; and still more are these simple means rendered available for a number of minutes, in which great good might be done, if the adventurous individual would take the precaution to confine his breathing to the lower strata by crawling on hands and knees.

How much valuable property has been lost—how many people have been burnt to death—from the impossibility of reaching them,—those willing to venture their lives to the utmost for the rescue being ignorant of the plain facts just stated. We do not say that such accidents cannot possibly happen in future, if only the aid of the safety-hood be secured. But we think that some such apparatus ought to be at hand: if not in the possession of private individuals, at least in the keeping, and under the control, of the public authorities. Something of the sort ought surely to be kept in readiness at the police station-houses. Why the safety-hood of Roberts has not been more generally received and adopted we cannot tell; perhaps it is too dear; but this we are convinced of, that the principle on which it is constructed is a sound one. We repeat that it cannot be too generally inculcated, that the great danger of suffocating fumes in burning apartments may be in a great degree obviated by a wet handkerchief applied to the mouth and nose, (if the handkerchief be wetted with soap and water it is still better,) and by moving beneath the vapour on hands and knees.

We have already alluded to the apathy of the public in respect to the attention which a subject of this kind demands: perhaps the legislature is not less to blame for neglecting to provide for that almost infatuated indifference; and we cannot conclude without expressing our full concurrence in what we just find stated by a writer in one of the public journals (the *Times*):—"It would, perhaps," says he, "be found a difficult task to adduce many cases occurring in a town wherein life was lost by the burning of a house, which life might not have been saved if an act of Parliament had made it the duty of the public authorities to keep and to use those means for the preservation of human beings which scientific men, and even men of science, have had the fortune to devise, make known, and live to see neglected." We entirely approve of the suggestion which follows, namely, that public meetings should be immediately held, in order to urge the authorities to adopt proper measures.

DEATH OF DR. WARREN.

THIS distinguished physician died on Wednesday the 2d instant, at Worting House, near Basingstoke, in the 58th year of his age, of organic disease of the liver. Dr. Warren was elected one of the physicians to St. George's Hospital in April, 1803, an office which he held exactly thirteen years, having resigned it in the same month, 1816, before which period he had already obtained a large share of business.

Dr. Warren made no contributions to medical science of which we are aware, except a paper on Headache, which he published in the Transactions of the College of Physicians, and a case of Ossification of the Aorta, read at one of the evening meetings in Pall-Mall East. His character and conduct, however, were well calculated to support the profession to which he belonged. His sentiments were in all respects those of a gentleman; and as he was too independent not to express them when the occasion required, titled impertinence has more than once been overmastered by the caustic bitterness of his retort. His manners were peculiar, and not always pleasing, being generally cold, and sometimes abrupt. He took a prodigious quantity of snuff, and was plain and untidy in his dress—perhaps to affectation. For many years he appeared to take no more exercise than in walking from his carriage to the sick chamber, and looked much older than he really was; but he had a remarkably keen black eye, which retained its vivacity long after the effects of disease were visible on his countenance. He moved in the highest rank of his profession, and though long in indifferent health, continued to discharge the duties of a very extensive practice up to the accession of the illness which proved fatal to him.

LECTURES

ON THE

DISEASES OF THE NERVOUS SYSTEM.

BY M. ANDRAL.

From the *Gazette des Hôpitaux*, in which they are in course of publication, with his approval.

OF HYPEREMIA, OR CEREBRAL CONGESTION *.

Anatomical characters—External causes—Alimentary substances — Narcotics — Internal causes—Digestive system—Circulating system—Qualities of the blood—Respiration—Secretions—Generative apparatus—Symptoms—Congestions of the cerebrum—of the cerebellum—of the spinal cord—Treatment—Chronic congestion of the spinal marrow—Treatment.

HYPEREMIA of the brain is a common disease, which presents itself under a variety of different forms. It may attack all parts of the nervous centres, and be either partial or general. When partial, it is most common in the two hemispheres, the rest of the cerebral mass and the spinal cord remaining entire. It may happen that only one hemisphere is affected with congestion, and then one side of the body presents symptoms similar to those which attend hæmorrhage into the brain. The congestion may take place not over an entire hemisphere, but in a portion of it, and follow the established anatomical divisions, occupying, for example, the an-

* The present discourse comprises lectures 2 and 3, as delivered by the Professor.—ED. GAZ.

terior middle or posterior lobe, — a limitation analogous to that which we meet with in the inflammation of many other organs—the lungs, for instance.

In the cerebral hemispheres, as in the other parts of the nervous system, the congestion may implicate either the cineritious or cortical substance; and there are some cases in which this limitation is well defined. The congestion may be deep-seated or superficial. As the hemispheres may have general or partial congestions, so it is also with the other parts of the cerebro-spinal mass: thus we may have isolated hyperemia of the entire cerebellum, or of one of its lobes; and again, the congestion may attack only the spinal marrow, leaving the brain intact. Such are the different seats of congestion in the nervous centres. Let us now consider the lesions which we find in the dead body.

Anatomical characters.—The most marked, or even the only one, is a certain degree of redness, presenting different shades according as the congestion has more particularly affected the white or grey matter. If the white, we observe, on slicing the centre of the hemispheres, a fine mottleing of innumerably small red points. The greater the number of these points, the greater the congestion; they are particles of blood exuding from the vessels, rendered apparent by the disease. If it is the grey substance which is the seat of the congestion, we no longer observe a red mottleing, but the substance is throughout of a rose colour, occasionally heightened to a brighter tint. Along with this change of colour are other lesions: thus the meninges are often considerably injected, the large veins of the pia mater are gorged, the sinuses filled with black blood, and sometimes, when the congestion has been very intense, we may discover a little blood in the texture of the pia mater; this is a transition from congestion to hæmorrhage, only that it does not take place in the cerebral substance. We may also find in marked cases some serum within the pia mater, on the surface of the brain, or in the ventricles, and which seems to result from the obstacle to the return of the blood through the overloaded veins and sinuses; and this serum acts an important part in the production of certain symptoms.

It is necessary, however, to inquire into the circumstances occurring before death, in order to determine whether the redness depends upon a state of gestion or on other causes. In fact, if the individual has died of an acute disease, we may find more blood than is natural in the brain, although there may have been no congestion. The kind of death must also be taken into consideration: thus the state of the brain and its meninges, gorged with blood from asphyxia, may resemble the injection of active hyperemia.

In examining into the particular seat of congestion, we must keep in mind that the different parts of the encephalon are not supplied with the same quantity of blood-vessels: thus, in the cerebellum there are more than in the brain. The age also causes a difference in the degree of injection, and in the colour of the nervous centres.

External causes.—One undoubtedly exists in the atmosphere, and in the different degrees of temperature. Let us see, first, the action of an elevated temperature, taken from 20 to 50 degrees (Cent. therm.); I shall stop at this last degree, above which life cannot be maintained. From 50 to 40 degrees the individual perishes, or exists but for a time, to die of cerebral congestion, in a manner more or less speedy; from 40 to 35 degrees the same phenomena take place, but are developed with less intensity; sometimes simple congestion is found, sometimes congestion and hæmorrhage: this is remarkable in persons who, like mowers, are exposed all day to the sun. From 35 to 30 degrees, accidents become more rare and less sudden; from 30 to 35, the tendency to congestion disappears.

If a very elevated temperature be an undoubted cause of cerebral congestion, it is not the same with the heat, notwithstanding it may be considerable, of our ordinary summers; but if the temperature be very much reduced, congestions again become more common: frequent examples of this occurred in the retreat from Moscow.

Individuals who pass suddenly from an intense cold to considerable heat, are much exposed to cerebral congestion. There are some singular cases where this has appeared to occur as an epidemic. M. Leuret could only explain an event of this kind, which occurred at the Maison de Charenton in 1823, by referring it to the circumstance of a violent south-west wind, which suddenly blew upon the establishment.

We have nothing very precise on the subject of atmospheric electricity, as a cause of cerebral congestion. However, I have seen all the symptoms of this affection appear in a paralytic individual, who had been subjected to electro-puncture; one of the needles having been applied to the neck, and the other to the lower extremities. Certain circumstances would lead us to the belief that the action of light is not without its influence in producing this affection. It would seem, from some accounts, that one-sixth only of the cases take place during the night.

Alimentary substances.— It is necessary that a predisposition should exist, in order to make the ingestion of our habitual aliment productive of cerebral congestion. But spirituous drinks have undoubtedly a direct action upon the brain; and the odour of alcohol is even said to have been detected in the serum of the ventricles.

Narcotics.—The influence of these, in the production of cerebral congestion, is incontestible, whether they be introduced by the mouth, the rectum, or the skin: but there is here, besides, something specific, for the narcotism is not altogether identical with the congestion resulting from other causes. It would seem, from the experiments of M. Flourens, that opium acts especially on the cerebral hemispheres, belladonna on the tubercula quadrigemina, alcohol on the cerebellum, and nux vomica on the spinal cord.

Internal causes.—If we now inquire how affections of the internal organs can give rise to congestion of the nervous centres, we shall there find causes much more numerous than exist among external agents: and first the brain itself, by its physiological or pathological action, may occasion its own congestion; for example, great mental exertions, strong emotion, or even an hysterical or epileptic paroxysm: indeed, it is from the continuance of congestion after an epileptic attack, that certain other symptoms arise—such as coma, delirium, paralysis; all of which exist only in a transient form, and disappear with the congestion.

Congestion may also result from accidental productions; as the formation of irritants attracting at intervals blood towards the brain, and thus producing the disease of which I speak. Softening and apoplectic affections, more or less chronic, may also give rise to this condition, and we have then two series of symptoms; the one permanent, which depends upon the accidental productions, the other transitory, and which arises from the congestion caused from time to time around the source of irritation.

The diseases of the meninges may excite congestion of the cerebral substance; and, in fact, meningitis is, for the most part, accompanied by injection of the brain itself.

Hitherto I have considered only those causes which have their seat in the nervous centres, or their investing membranes; but there are others.

Digestive system.—Certain states of the stomach may give rise to hyperemia of the brain; such, for example, as a too active digestion in persons who are predisposed. Cerebral congestion also frequently shows itself, particularly in infants, as a consequence of acute gastro-enteritis. We must take care, however, that we do not always attribute to congestion of the brain the delirium, coma, &c. which we see at certain periods of follicular enteritis; for these symptoms, which depend sometimes, it is true, upon hyperemia, may be met with in individuals whose brain is found, on post-mortem examination, to be of remarkable paleness.

Chronic diseases of the digestive canal are not without influence in promoting the disease of which I speak; thus, in certain persons affected with chronic gastritis, each exacerbation is ushered in as if by an attack on the brain, determining congestion to it. We may observe in such an individual, at each exacerbation of his gastritis, a partial paralysis of the arm, which always appears on the same side, and which recedes in proportion as the stomach ceases to suffer.

The diseases of the small intestines are of some account in the production of hyperemia of the brain; the same is the case with affections of the large intestine, where, besides, a special cause exists in prolonged retention of its contents.

Circulating system.—We find numerous causes of cerebral congestion in the organs of circulation. First, if the impetus of the blood exceed certain limits, congestion results. If the palpitations with which a patient is attacked become very strong, there often occur dimness of sight, vertigo, ringing in the ears, numbness, &c. Hypertrophy of the heart ought, therefore, to be ranged among the causes of congestion; but it is not by the mere impulse of the blood by this stroke, as of a piston, that the effect may be produced; if, for example, an obstacle exists to the free return of the blood, the cerebral congestion is produced in a manner quite inverse of the former. M. Tonnele has given some cases of hyperemia of the brain in infants, with obstruction to the circulation in the sinuses of the dura mater; and M. Gintrac, of Bourdeaux, has recorded a similar instance in an adult.

An obstacle to the return of the blood capable of producing congestion may exist exterior to the cranium, as in the jugular veins, or tumors of the neck of different kinds, and goitre among others. Strangulation — a cravat too tightly tied for instance, may produce the same effect. The horizontal position, the head being lower than the trunk, may also occasion it. Let me remark, however, that persons have been known to be immediately seized with cerebral congestion every time that they quitted the horizontal posture. It has also been seen to take place in the fœtus during birth; and hence

results what has been called the apoplexy of new born infants, although it is very seldom that hæmorrhage is found to have occurred in such cases.

Every violent effort impeding the return of blood from the brain may cause this affection, as we witness occasionally in the act of vomiting in individuals who are predisposed; and hence the danger of emetics, which were freely used in cerebral congestion, before Portal had directed attention to the accidents which may occur from this method of treatment. It is not very uncommon to observe fatal congestions of the brain from violent straining at stool. Certain obstacles resulting from vicious formation of the apertures of the heart, on the right side, may produce it, although this be very rare; and simple increased action of the heart, without any defect in its organization, is also sometimes sufficient to excite congestion.

The state of fever without the acceleration or mechanical retardation of the blood of which we have spoken, is productive of hyperemia in certain cases; and it is this congestion which often constitutes the danger in the disease which is called "pernicious intermittent."

We find other causes in the inflammation of other organs. Hyperemia, which is one of the necessary elements of inflammation, may develop itself in the brain in three different manners:—1st, Before the appearance of the inflammation, as, for example, in the premonitory stage of eruptive diseases. 2dly, Simultaneously with the inflammation. 3dly, On the premature disappearance of an inflammation, as of erysipelas of the face.

There are certain morbid conditions to which we can scarcely give another name than that of migratory fluxes, in which in turn, and without ceasing, the different organs are affected with congestion; one day it is the uterus, another the liver, another the lungs, and another the brain. In a word, there are individuals who seem to be under the necessity of having some hyperemia; and it is in such subjects that these congestions frequently end in hæmorrhage.

Qualities of the Blood.—If rich, abundant, fibrinous, high-coloured blood—the plethoric and sanguineous temperament, in fine—predisposes to this disease, the absence of these conditions does not carry with it absolute immunity from its attack.

Respiration.—The disturbances of this function only influence the production of cerebral hyperemia in as far as they impede the circulation; their action is therefore altogether indirect, as in croup, and œdema of the glottis.

Secretions.—While these are normal, they have no marked influence; but it is different with certain morbid secretions, as, for example, the hæmorrhoidal flux, the suppression of which may be immediately followed by cerebral congestion. The suppression of old ulcers, or drains, has a real influence, though one which has been much exaggerated, in the production of the disease of which we speak.

Generative Apparatus.—It cannot be denied that this system has great influence on the brain, and may be productive of congestion; thus at the appearance of the catamenia, and before their establishment, numerous congestions take place, which cease as soon as the menses are established. At the *critical* period we observe those migratory discharges of which I have spoken, and one of which implicates the brain; these conditions, if badly treated, may be productive of formidable organic affections. The absence, or accidental delay, of the catamenia, may cause hyperemia of the brain. In some individuals there is a tendency to congestion at each period; but its locality often depends on the germ of some disease existing in an organ. If the individual is at a later period to have symptoms of disease of the brain, it will be in this organ that the congestion will take place; if it be the lungs which contain the rudiments of disease, it is towards them that the flow of blood is directed, and hæmorrhage may then occur, to which, perhaps, no attention is given; but in this a great error is committed, for under such circumstances there is seldom wanting an organic affection of the lungs.

The abuse of sexual intercourse, as well as too rigid continence, may prove the exciting causes of cerebral congestion.

This disease may also find its cause in certain movements, as whirling. A fatal cerebral congestion has been known to result from walking, an instance of which occurred in a law student here in 1829. (*Vide* Lancette Française, No. 80.)

Age.—All ages are subject to this affection, but it is most frequent after forty.

Symptoms.—These are very variable, and differ very much from each other: 1st, according to the intensity of the congestion; 2dly, according to its duration; 3dly, according to the difference of the parts affected.

Let us first direct our attention to congestion of the hemispheres; of which we shall find several forms, according to the duration and intensity of the disease. In one of the first forms, which is very frequent, the grand cerebral functions—intelligence, motion, and sensation—are not much affected; the sensibility alone is

somewhat disturbed; there is a little headache, dimness of sight, some giddiness, noises in the ear, numbness, sleeplessness; but all this is slight. Occasionally there is a sluggishness of motion, and at other times extraordinary activity, a constant impulse to walk about; sometimes there is a sense of pricking about the limbs or face, and palpitations may occur, whether dependent upon hypertrophy of the heart or on nervous disturbance of that organ. If the congestion be intense, the pulse is strong and vibrating; the pulsations of the temporal artery are sensible to the eye as well as the touch; the capillaries of the face, the cheeks, and even the forehead, as well as those of the conjunctiva, are injected; the veins of the forehead are distended. Sometimes bleeding takes place from the nose.

This first form presents two varieties, according as there is fever or not. If fever be present, it is of the inflammatory type, with determination to the head: its duration under such circumstances, is limited. If there be no fever, the duration is not thus limited, but extends from a few minutes to some hours, or months; and sometimes a constant disposition to cerebral congestion exists for many years. This congestion may shew itself only once, or many times; it is one of those diseases which are very prone to return, recurring sometimes unexpectedly and at irregular intervals. Thus some have such congestions at the return of some particular season; in others we witness those congestions of the brain which are intermittent, presenting the tertian, quotidian, or some other type; and this congestion is described under the name of intermittent apoplexy. After cerebral congestion in this its first form, the health may be entirely re-established, but the other forms may succeed to it, and it also may become complicated with other diseases—as apoplexy, softening, &c.

A second form is much more formidable. At the instant of attack the patient falls down in a state of insensibility, and motionless, just as in apoplexy: this constitutes a *coup de sang*. The cases in which we cannot distinguish this from hæmorrhage are precisely those in which they are of the greatest possible severity—those in which the four extremities are paralysed. In a less considerable hæmorrhage there is generally only hemiplegia; whilst in the *coup de sang* there is for the most part general paralysis. It is only as an exception that we sometimes see hemiplegia: cases of this, however, do occur.

When these symptoms are present, death may take place almost instantly; in some, life is prolonged during several hours without any movement taking place, and without the slightest appearance of sensibility; in others, the symptoms gradually disappear, sensation and movement slowly returning. Nor is this by any means uncommon, but it is more frequent for some affection of certain important functions to remain: the intellect, for example, may continue for some time more or less impaired. There is a sort of stupor or intellectual sluggishness and occasionally delirium, which lasts four or five days, after which the intellect resumes its power. The sensibility is blunted; there are formications; the hearing has lost its acuteness; the sight is less perfect; and there are pains about the head, which are frequently long before they are dissipated. As to motion, the weakness of the limbs is general, or may be more particularly observed in one. Often there is an embarrassment of speech. The frequency of stuttering after the *coup-de-sang* is remarkable. As to the rest, the symptoms disappear more or less rapidly; and if their duration seem likely to be prolonged, there is reason to apprehend greater mischief in the brain than simple congestion.

In the third form, there is loss of intelligence and sensation; but we observe, with regard to motion, a phenomenon which approaches to congestion, accompanied with hæmorrhage—namely, partial paralysis, or in a certain number of cases hemiplegia; however, the hemiplegic symptoms disappear with those of the original attack.

There is one disease in which, although there be no effusion of blood, we often see the same symptoms reproduced. In epilepsy, for instance, we have sometimes a true hemiplegia after a paroxysm, but which disappears one or two days afterwards; and if we have occasion to open the body in a case of this kind, we only find traces of great congestion, but nothing like effusion of the smallest quantity of blood. In some cases intelligence and sensation return, while the hemiplegia remains, although, nevertheless, there has only been simple congestion. M. Leuret has recently published some cases of this kind; I have also seen some instances of it, and there may perhaps be about fifty such on record.

In certain cases, particularly in infants, motion is so effected as to give rise to convulsions, at the same time that intellect and sensation are abolished.

In a fourth form of congestion of the hemispheres, the intellect remains undisturbed, but there are obvious modifications of motion and sensation; similar phenomena are observed in certain hæmorrhages. M. Collier (Bibliothèque Medicale, tome lxi. page 218) quotes the case of a man, 53 years of age, who from his youth had suffered from headache occupy-

ing the anterior part of the head, on the right side. This person was all at once seized with violent giddiness, his sight was disturbed, and he was unable to move. At the same time he had hemiplegia; but his intellect remained entire. These symptoms continued during five minutes, and then became completely dissipated, returning, however, many times each day, for a month, and concluded by being permanently removed. M. Gintrac, of Bourdeaux, relates the case of an infant affected at intervals with complete loss of motion, while hearing, sight, and intellect, remained entire. The movements of the tongue concerned in speaking were as completely abolished as those of the other muscles. What is remarkable in this form is the preservation of intellect.

In a fifth form the principal phenomenon is the loss of intellect. We observe delirium with violent perturbation, screaming, violent muscular agitation of the limbs, and death. It is remarkable to witness the sudden cessation of this great display of strength produced at once by the irrevocable extinction of life, which takes place in the midst of delirium and agitation which has lasted for some days.

Such are the different aspects which congestions of the hemispheres of the brain may assume. How is it that a disease which is identical in its anatomical characters gives rise to symptoms so dissimilar? Can we find any explanation of this difference of form, in the degrees of intensity, and duration of the disease? Or is it necessary to take into consideration the seat of the hyperemia among the causes of these diversities? We can, I think, make out that the symptoms depend for their difference upon the portion of the nervous system which is implicated; but if we wish to go further, and to fix precisely what that point of the brain is, the congestion of which will produce such and such of the forms which have been above described, the attempt fails; it has, however, been made. Thus M. Foville says that delirium without loss of motion and sensation depends upon a change of the cineritious substance—a fact which is far from proved, although it may be possible; and, in truth, it is particularly in irritation of the meninges that delirium occurs. Now inflammation in them reacts upon the convolutions; it is not the meninges which think, but the brain placed beneath these membranes.

Congestions of the Cerebellum.—Different symptoms may manifest themselves, according as the middle, anterior, or posterior part of the brain is attacked, according as the lesion has its seat in one of the large ganglions of Gall, the optic thalami, the corpora striata, &c.; this is intelligible, but it is not demonstrated. There are parts to which especial functions have been attributed; and it has been thought that certain specific symptoms were indicative of their congestion. Take the cerebellum as an example: according as it is held to be the regulator of motion, or the seat of sensibility, or the centre of the generative functions, different inferences are drawn from the symptoms which are observed. Many phenomena have been referred to congestion of the cerebellum: thus the symptoms of intoxication have been attributed to it, because the cerebellum, as I have already said, has been regarded as regulating voluntary motion. But this idea is a bare hypothesis, unsupported by a single anatomical fact.

In certain cases of congestion, the most striking circumstance is the excess of sensibility, which is carried to the highest possible degree; and those who think that the cerebellum is the special seat of sensibility, say that it is this organ which is congested. But neither is this opinion supported by conclusive evidence. There are some facts which seem to favour the idea that the cerebellum rules over the generative functions. I have seen some patients labouring under uterine disease, with dysmenorrhœa, who had acute and continued pains in the occiput at each period. A man came to me one day, who complained that he suffered from very acute pain all over the occiput every time he had sexual connexion. [The gentleman by whom these lectures are reported has a friend who cannot indulge in the least excess in this respect, without having severe pain in the cerebellum and corresponding part of the cranium.] A young man was annoyed by violent priapism; he had severe and constant pain of the occiput for three months; local and general bleeding were employed in vein. Meningitis supervened, and he died; but I regret that no autopsy could be obtained.

Congestion of the Spinal Cord — Let us now see by what symptoms hyperemia of the other parts of the nervous centres becomes known to us: let us take the spinal marrow. This congestion is less frequent than that of the brain or cerebellum, and different parts of the cord may be the seat of the affection; it may attack the anterior column or the posterior, the cervical portion, the dorsal, or the lumbar. The symptoms consist in lesions of motion, sensation, and some acts of organic life; they may not implicate the brain, and they are quite different according as the congestion takes place slowly or rapidly; from this arise two principal forms, viz. acute and chronic hyperemia.

Acute hyperemia of the spinal cord.—This

admits of three subdivisions. In the first form it is motion which is principally affected: thus we observe sudden paralysis of the four extremities, or only of both upper or both lower; in some very rare cases the paralysis is confined to one side and one limb. When cases of this kind occur, we must give close attention to the state of the brain; the mechanical powers of breathing may be impaired, and paralysis of the diaphragm and of the intercostal muscles may cause death by asphyxia; partial or general convulsions may also be present.

In the second form it is on sensation that the morbid effect exerts its special influence; it may be at once abolished either partially or wholly. In some persons, in place of this abolition, there exists only a simple numbness of the limbs, supervening either suddenly or gradually, and deep-seated pain of the limbs, sometimes following exactly the course of the nerves.

In the third form, there is simultaneous lesion both of motion and sensation; and it may happen that these lesions are not of the same kind. Sensation and motion may be increased or abolished, but sometimes there is loss of the power of motion, while sensibility is increased, and *vice versa*. This acute congestion of the spinal marrow may terminate in recovery or death; and this last takes place rapidly when the mechanical powers of respiration are implicated.

Chronic hyperemia of the spinal marrow.—The accidents which occur in this are analogous to those met with in the acute form, but they develop themselves more slowly, and in a different manner. Its duration may be long, and it may be gradually dissipated or be productive of different lesions. When the congestion terminates favourably, we may observe certain critical symptoms, such as hæmorrhage from the nose, intestines, or uterus. A female patient was affected with frequent cerebral congestions, which always relieved themselves by a flow, not of blood, but of watery fluid, which ran from the nose in sufficient quantity to wet a number of handkerchiefs.

Treatment.—In the treatment of every congestion of the nervous centres, three fundamental indications present themselves.

The first is to combat the causes, and these are numerous, as we have already seen. They are found in the atmosphere, in the diet, and in narcotic medicines, in the mode of action of the brain, in the circulating system, in digestion, in respiration, and in the sexual functions.

The second indication consists in emptying mechanically the overloaded vessels by general or local blood-letting. The general bleeding is sometimes preferable when practised on the foot, and congestions have been known to yield to this, after having resisted venesection from the arm; but a grand condition of its success is, that, above all, it be copious; and this is often difficult. Leeches may be applied to the nostrils in persons subject to epistaxis. They may also be placed on the mastoid apophyses, on the neck, along the sagittal suture, on the thighs (particularly when we desire to restore the catamenia), or on the ankles. Generally rapid recovery follows this treatment; nevertheless, in some cases the congestion continues undiminished, notwithstanding the most copious abstractions of blood; and then we must not trust too much to antiphlogistics.

In the third indication, the object is to excite a flow of blood to a part distant from the seat of congestion.

The ancient writers are filled with examples of internal congestions following the disappearance of gout, &c; and I have witnessed some analogous cases. We restore various eruptions to the skin by irritating frictions, with croton oil, tartarised antimony, &c. We may also induce new action on the skin, or bowels, in the former case, by blisters, cauteries, moxa, &c. and in the latter by purgatives, which may sometimes prevent congestions when administered so as to act two or three times a day. If the congestion be already produced, and of an acute kind, very active purgatives require to be employed. In some cases an attempt has been made to produce an abundant salivation. It is a precarious method, because the increase of the salivary secretion necessarily attracts the blood towards the head, and may thus render the congestion more intense.

MIDDLESEX HOSPITAL.

Extracts from a Clinical Lecture by Dr. Watson.

Cerebral Hæmorrhage, combined with, yet not depending upon, Hypertrophy of the left ventricle of the Heart.

Mary Nagle, aged 60, was brought to the hospital on the 26th of October, insensible, with palsy of the left side of the body. Pulse 70, labouring. Pupils contracted.

She had suddenly fallen down in a fit of apoplexy on the 24th. Between the attack and her admission consciousness had in some degree returned, and she then complained of pain in the right side of the head. Afterwards she relapsed into a state

of deep coma. She had been bled in the right jugular vein.

She gradually sank; and died early in the morning of the 29th.

The body was examined the same day.

The bony skull-cap adhered firmly to the dura mater. The external veins of the cerebrum were full of dark blood. The surface of the arachnoid was quite dry.

The right lateral ventricle contained a large, dark, soft coagulum of blood, and was further distended by a considerable quantity of bloody serum. The foramen of Monro was widely open; and the left lateral ventricle was also full of serum, tinged with the colouring matter of the blood. On further examination it appeared that the coagulum occupied a large portion of the right thalamus opticus, of which the texture, to a certain depth, was crushed into a pulp. The cerebral matter immediately adjacent to the broken part was dotted with blood. The vessel from which the blood had escaped was not detected. The basilar arteries were much altered by disease; presenting many irregular opaque spots, and a manifest inequality of calibre. The arachnoid at the base of the brain had lost its natural transparency.

The heart appeared to be of the normal size; but the thickness of the walls of the left ventricle was very greatly increased, while its cavity was unusually small. The right ventricle also was small. The mitral valve was thick, stiff, and puckered; so that nothing larger than one's thumb could pass easily from the auricle to the ventricle. The other valves were quite healthy. Just behind and beyond the aortic valves, a few small atheromatous specks were visible, of a whiter colour than the surrounding membrane. Elsewhere the aorta was free from disease.

In a clinical lecture, among other remarks upon this case, Dr. Watson called the attention of the pupils to the coincidence it presented of cerebral hæmorrhage with hypertrophy of the left ventricle of the heart: a coincidence so frequent as to have led to the notion, often, if not always erroneous, that the one was the physical cause of the other. In this instance it was plain that no such relation of cause and effect could have obtained. The cavity of the left ventricle was so much diminished, that the momentum of the column of blood driven forwards by its contraction must have been less, instead of being greater, than what is natural. The true explanation of the hæmorrhage in the brain is to be found in the diseased condition of the cerebral arteries. Usually, indeed, with such a morbid state of those vessels we find associated the same kind of change (a deposit of albuminous matter, diminished elasticity, and dilatation) in the commencement of the aorta. And this disease of the main artery will virtually obstruct the free passage of the blood out of the heart, and thus account for the increased bulk and thickness of the ventricle. But in the present case the aorta was very nearly healthy: and the hypertrophy had resulted from the alteration which the *mitral valve* had undergone. One consequence of this alteration must have been that the ventricle did not receive its due proportion of blood from the corresponding auricle: and it is presumable that the ventricle, thus imperfectly supplied, was teased (if the expression may be allowed) into preternatural effort or frequency of contraction: and this increase in its natural action would explain, upon well-known principles, its augmented muscular bulk. Dr. Watson had more than once seen this combination of hypertrophy with diminished capacity of the left ventricle, when the passage of the blood from the auricle had been mechanically impeded by disease at the mitral orifice. It is not, however, a very common accompaniment of disease in the mitral valve: usually there is disease of the aortic valves also, or of the aorta, and *dilatation* as well as hypertrophy of the ventricle.

The lecturer observed that the facts of this case were directly opposed to the opinion generally prevalent respecting the kind of connexion that subsists between apoplexy and hypertrophy of the heart; while they tended to confirm the views he had long entertained, and frequently expressed, on the same subject*.

In the course of the same lecture, after noticing three cases of poisoning which had been brought to the hospital during the preceding fortnight—the poison swallowed in one case having been aquafortis, in another arsenic, and in the third oxalic acid—Dr. Watson made some observations, of which the following is the substance,

Upon the use and the abuse of the Stomach-Pump.

It is too much the fashion to employ the stomach-pump in every case of poisoning; with the view either of removing the poisonous substance, or, if that has already been accomplished by the efforts of nature, of introducing antidotes and diluents. I wish to caution you against this indiscriminate use of an instrument which, under proper regulation, is really invaluable. Doubtless many lives have been saved by it which without it would inevitably have been lost; but it is no less true that the same instrument is very often resorted to when

* See Med. Gaz. vol. xvi. p. 21.

it need not be used at all; and even when its employment can scarcely be otherwise than injurious. What, in fact, are the uses of the stomach-pump? They are simply two: that of conveying fluids into the stomach is one; that of extracting fluids from the stomach is another.

Now, when a patient is both able and willing to swallow, *that* is at once the quickest and the easiest method of effecting the first of these purposes. On the other hand, should the patient be insensible; or too weak to perform the necessary acts of deglutition; or should he obstinately refuse to swallow, as not unfrequently happens when suicide has been attempted; in all these cases, the stomach syringe offers an admirable resource, and overcomes a most perplexing obstacle to our curative endeavours.

Again, if a person is vomiting, or can be brought to vomit, and the vomiting is or can be kept up by the frequent administration of warm drinks, this is the easiest, the safest, and the most effectual way of ridding the stomach of its contents. But, if the patient be in a state of insensibility; or if the stomach itself have been rendered by the poison insusceptible of the impression of emetics, which commonly have a speedy operation; or if, from the nature of the poison, such emetics be even likely to fail, and danger is impending from further delay; then, again, in this simple instrument we often find the sure and ready means of relieving the stomach of its deadly burden.

There are some cases, then, and those of poisoning by opium are pre-eminently of the number, in which the aid of the stomach pump is almost indispensable: and there are others in which its employment is unnecessary, and therefore, in my opinion, wrong and blameable. But this mechanical way of getting rid of the noxious matter which the stomach may contain has something very satisfactory and attractive about it, to the operator at least, if not to his patient: and I make no doubt that it will continue to be insisted upon by the one, and submitted to by the other, in numerous instances in which the operation is not actually requisite; yet in which it does good and palpable service, and inflicts no injury.

What I wish, however, chiefly to get you to attend to and consider, is, that there are cases in which the use of this instrument may be hazardous, or positively hurtful. The introduction of the tube always implies some degree of violence to the parts along which it passes—to the fauces, œsophagus, and stomach. Slight hæmorrhage from these parts is no unusual evidence of this. In the hands of an inexpert or clumsy person it may become, even under ordinary circumstances, a most formidable weapon. I am not sure that I have not myself seen inflammation of the larynx produced by its mismanagement; and mischief of a still more surely fatal character has been witnessed by others. Some time ago a patient was brought here who had taken poison (sulphuric acid, I think it was), and for whose relief the stomach-pump had already been resorted to. Death presently ensued; and the body was opened in the dead-house below. I was not at the hospital on that day; but the examination was conducted in the presence of many persons, pupils and others, and of Mr. Alexander Shaw among the rest. Upon his information and authority I relate these circumstances. Besides other appearances, which I need not specify, a large quantity of *chalk mixture* was discovered in the *wind-pipe*: and the air-passages being thereupon carefully traced, many of the larger bronchi, and considerable portions even of the vesicular texture of the lungs, were found to be choaked and plugged up with the same substance. This had been thrown in by means of the pump; unquestionably with the best intention, but it had passed in a most unfortunate direction: it had missed the road to the stomach, where it might perhaps have been of some service; and it had entered the lungs, where it could scarcely fail to prove destructive, even if the patient's life had not been already endangered by the poison. Even when the tube has been carefully and skilfully introduced, it is by no means an impossible thing for the mucous membrane of the stomach to be sucked into the orifices at its extremity, and for serious mischief to be *so* produced; and where corrosive substances have been swallowed, and the gullet and stomach have already been inflamed or abraded by the poison, the friction of the pipe will be likely to augment the existing injury, and may even cause irreparable damage. If, after taking caustic poison, the patient cannot swallow, still less can he with impunity receive the tube of the instrument. I am not stating merely possible or imaginary dangers: I remember seeing the stomach of a girl who died in St. Bartholomew's Hospital many years ago, and in whose case the stomach-pump had been used. There were three long strips of the mucous membrane of the stomach hanging loose in its cavity: they had evidently been torn up by the tube. You may see an accurate representation of that stomach in the first part of Dr. Roupell's admirable set of plates illustrative of the effects of poisons. I do not imagine that, in this particular instance, the condition of the patient was rendered materially worse by this mishap: she had no chance of recovery from the effects

of the arsenic she had swallowed, *a considerable portion of which still remained in the stomach.* But I mention these untoward occurrences to shew you that the stomach-pump is not always a harmless instrument; that care, and some, though surely not much skill, are required for its introduction; that it can never be considered a matter of indifference whether it be employed or not; that it is chiefly, if not solely, admissible where narcotic poisons have been taken; that in the case of corrosive poisons especially, it is to be used, if at all, with great caution; and that it never should be used at all if the patient be able and willing to swallow, and if full vomiting can be excited without it.

I have still to warn you of another danger to be apprehended from the prevalent application of the stomach-pump in all cases of poisoning indiscriminately,—a danger quite distinct from any that have yet been pointed out: the danger, namely, under certain circumstances, of the *delay* which it implies. There are poisons of which the deadly operation is capable of being averted by antidotes, and yet is so rapid that remedies are of little avail unless they are administered very soon after the poison has been swallowed. Oxalic acid is a poison of this kind; and of its fatal effects you have just seen an example. Dr. Christison has already admonished the profession that the time expended in giving emetics, or in using the stomach-pump, is time lost, in cases of poisoning by this substance, *when antidotes are at hand;* and you will observe that the best antidotes, chalk and magnesia, are much more frequently at hand, and in nine cases out of ten are much more readily procurable, than is the stomach pump.

I fear that the error against which I am endeavouring to guard you has been fostered by the high degree of favour that the stomach-pump has obtained with the public, who readily apprehend its advantages, but are not so quick in perceiving its possible demerits. Medical men are sometimes, I suspect, induced to employ the instrument merely in compliance with this popular sentiment, and lest it should be imputed to them, as a fault, that in circumstances of urgent danger they had neglected an important remedy. A gentleman well known to you all, who was formerly house-surgeon here, Mr. Charles Beevor, has told me that he was once rated by a coroner's jury for having, in the sound and proper exercise of his discretion, abstained from introducing the stomach-pump in a case of poisoning which terminated fatally. The poison was oxalic acid. Similar absurdities are not uncommon at inquests. It requires, perhaps, some share of moral courage to disregard prejudices of this kind, especially when they are likely to be publicly expressed, and may, however wrongfully, prove injurious to the reputation of the practitioner. The requisite degree of this mental firmness may best be ensured by a thorough comprehension of the circumstances which, in certain cases, justify, and even demand, the adoption of the expedient in question; and in certain other cases as plainly forbid it. I am anxious not only that you should satisfy your own minds upon these important points, and act according to your conviction; but also that you should do what you can, each in his own circle of influence, to diffuse sounder notions than have hitherto prevailed respecting them, throughout the community.

CONDUCT OF THE "COUNCIL OF THREE" AT SOMERSET-HOUSE TOWARDS THE MEDICAL PROFESSION.

To the Editor of the Medical Gazette.

Sir,

I should not have intruded so soon again on your notice but for the perusal of the leading article in the Lancet of November 21st, in which the Editor (perhaps displeased at the castigation you have so deservedly bestowed on the Poor-Law Commissioners) attempts to exonerate them from censure, and to fix the onus exclusively on the Boards of Guardians.

Now there is a manifest injustice in any such endeavour to shift the responsibility of these enormities from the *superior* to the *inferior* functionaries, or to attach the entire blame to a numerous and *unpaid* body of country gentlemen, while the high salaried "council of three," who have the power of absolute dictation in the administration of the poor-law, are excused and justified.

If the view which your contemporary takes of the matter were correct, and if the Poor-Law Commissioners did really intend to afford adequate and efficient medical attendance to the pauper population, why did they not issue their directions expressly to that effect?

The "permissive authority" delegated to guardians, to appoint as many medical men as they think proper (with which the commissioners attempted to satisfy the Kent deputation, and upon which your contemporary lays so much stress) is practically useless as a preventive to the present wide spreading evil.

As long as there are inducements held out to the local boards to limit the medical appointments to a few individuals, and as long as the commissioners *enforce* the absurd and unjust system of pecuniary com-

petition, so long will a "permissive authority" of this kind be a mockery of redress.

The plea of non-interference with the medical arrangements of the boards, with which the commissioners have sometimes met the remonstrances of the profession, is equally destitute of foundation. Facts which will probably ere long be before the public, *entirely disprove it.* It is true the commissioners have not interfered to remedy the alleged grievances, but they interfered in numerous instances to uphold and encourage the boards of guardians in the course complained of, and to check and oppose them when inclined to act in a reasonable and considerate manner.

Again, where the gentlemen at Somerset House have not themselves interposed, their representatives (the assistant commissioners) have frequently done so; upsetting the previous medical arrangements of the guardians, threatening and calumniating the resident practitioners, and, if still unsuccessful, putting their threats into execution by introducing some of their unemployed protegés.

The truth of this may be verified on a large scale by a reference to the various counties in which opposite plans are pursued. Comparatively few complaints are made from *Hampshire* and *Northamptonshire*, the assistant commissioners in these counties having apparently acted in a considerate and liberal manner; while the public prints of *Suffolk*, *Buckinghamshire*, and *Kent*, (as well as private communications) where a different order of gentlemen hold the sway, teem with cases of grievance.

If further evidence were wanting, the last Report presented to the Secretary of State, which I suppose to be the production of the Commissioners of both grades, will prove conclusive. A document more calculated to embue the public mind with feelings of jealousy and suspicion towards the whole body of country practitioners, could hardly be penned. It is a wanton and unprovoked *general* attack on men whom they have previously been maltreating individually. The impression made by this report is evinced strongly by a letter in the *Times* of November 28, signed "Rusticus," to which, if not anticipated by an abler hand, I shall endeavour to reply to-morrow.—I am, sir,

Your obedient servant,

RURICOLA.

Dec. 33 5.

SPURIOUS EDITIONS OF MEDICAL WORKS.

To the Editor of the Medical Gazette.

SIR,

IN your journal, No. 414, a correspondent has exposed the attempt at imposition upon the public by a bookseller in the Old Bailey; and if Henderson merits (and I *know* he does) the censure of your correspondent, I think you, sir, and your numerous readers, will agree with me in opinion, that the proceedings of a bookseller in the Borough strictly merit the same treatment.

Perceiving an advertisement, a few days since, of a new London Dispensatory, by Drs. Cox and Gregory, to which was appended a translation of M. Magendie's Formulary, for twelve shillings, I was induced to purchase the same. On examining it. however, I found the Dispensatory was an *old* work, published *eleven years since!* by *a son* of the publisher's, and to which a Dr. Gregory has furnished a translation of Magendie's Formulary. This *New* Dispensatory, therefore, is a cheat, and ought to be exposed.

But, sir, this is not all. When in the country in the summer, I saw Mr. Lawrence's Lectures on Surgery, Medical, and Operative, advertised on the cover of your excellent journal, and I ordered it accordingly, when, to my astonishment, I found only *twenty* lectures out of the course, which extends to (I believe) eighty-eight. Is this conduct honourable? Surely the industrious student ought not to be silent when such aggressions are being made upon his purse. The publisher's name and address is E. Cox, St. Thomas's Street, Southwark.

I am confident your sense of justice will induce you to give insertion to this letter, if only as a caution to the numerous students now in London.

I am, sir, with much respect,
Your obedient servant,

WM. C. NIAARP.

1, Mount Terrace,
Whitechapel Road, Nov. 14, 1835.

MORISON'S PILLS.

ANOTHER EXAMPLE OF THEIR DELETERIOUS NATURE.

To the Editor of the Medical Gazette.

SIR,

ONE would suppose that the mind of the public had, by this time, been tolerably well enlightened regarding the nature and fatal effects of this pernicious nostrum. Nothing can exceed the self-sufficiency and utter ignorance of the itinerant quacks of the establishment who scour the country, imposing upon the weak and credulous. It were, indeed (as suggested in the Gazette of last week), to be desired that every medical man would endeavour to rid the world of such detestable knavery, by giving publicity to cases of violent injury, or death, resulting from this medicine,

falling within his immediate observation. I lately visited a gentleman at Oxford, who was mourning the loss of a friend snatched prematurely from his family. Relating the particulars, he told me that the deceased had been for some time unwell, but not so greatly as to cause serious apprehensions. In a weakened state of body he had resorted to Morison's pills, and the result was, that after two or three full doses, hypercatharsis was induced, and the patient was carried off. No doubt existed in the mind of my informant as to the real cause of death; but as the unfortunate gentleman had been ailing for a considerable period previously, no public notice was taken of the catastrophe.

I am, sir,
Your obedient servant,
R. H. Allnatt, M.D.

Wallingford, Dec. 10, 1836.

HENDERSON'S EDITION OF CUVIER.

To the Editor of the Medical Gazette.

Sir,

I perused with much pleasure the spirited letter relating to the publication of the above work, signed "P." published in the number of the Medical Gazette for November 7th, page 190. I suppose you have his address; if so, I will thank you to send him the inclosed letter. I trust the generality of the subscribers will unite with him in the attempt to punish the agent of so vile an imposition.—I remain, sir,

Your obedient servant,
R.

Nov. 28, 1835.

NEW MEDICAL WORKS.

Clinical Illustrations of the Diseases of Bengal. By Wm. Twining. 2d edition 2 vols. 8vo. 14s. cloth.

An Experimental Guide to Chemistry. By Edward Davy. 12mo. 3s. 6d. cloth.

Treatise on the Diseases of the Eye and its Appendages. By R. Middlemore, M.R.C.S. 2 vols. 8vo. 1l. 15s. bds.

On Perforation and Division of Permanent Stricture of the Urethra. By R. A. Stafford. 8vo. 3d edit. 9s. bds.

Practical Observation on Midwifery. By Robert Collins, M.D. 8vo. 12s. 6d. bds.

The London Dispensatory. By A. Todd Thomson. 8vo. 8th edit. 18s. bds.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

December 10, 1835.

John Andrew Field, Milford, South Wales.
Joseph Dickinson, Cumberland.
John Bennett, Chapel en la Frith.
Charles Elwyn, London.
Charles Hogg, London.
Richard Smith, Winchcomb.
George Fearnley, Dewsbury.
John Bland Wood, Pontefract.
Wm. Thomas Callon, St. Helen's, Lancashire.
George Yarnold, Romsey, Hants.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Dec.* 8, 1835.

Abscess	11	Inflammation	85
Age and Debility	77	Bowels & Stomach	1
Apoplexy	24	Brain	4
Asthma	17	Lungs and Pleura	14
Cancer	4	Insanity	6
Childbirth	15	Jaundice	1
Cholera	1	Liver, diseased	11
Consumption	137	Locked Jaw	5
Constipation of the Bowels	1	Measles	12
Convulsions	56	Miscarriage	2
Croup	3	Mortification	8
Dentition or Teething	30	Paralysis	2
Diarrhœa	10	Rheumatism	4
Dropsy	23	Scrofula	2
Dropsy on the Brain	11	Small-pox	23
Erysipelas	7	Sore Throat and Quinsey	3
Fever	32	Spasms	5
Fever, Scarlet	14	Stone and Gravel	4
Fever, Typhus	2	Stricture	1
Fistula	3	Thrush	2
Gout	5	Tumor	3
Hæmorrhage	1	Worms	1
Heart, diseased	9	Unknown Causes	39
Hooping Cough	22		
Indigestion	3	Stillborn	29

Increase of Burials, as compared with the preceding week 414

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N.
Longitude 0° 3′ 51″ W. *of Greenwich.*

Nov. 1835.	Thermometer.		Barometer.	
Thursday . 26	from 46	to 55	29·67	to 29·43
Friday . . 27	50	54	29·40	29·32
Saturday . 28	44	51	29·31	29 53
Sunday . . 29	35	49	29 52	29 34
Monday . . 30	45	55	29·17	29·18
Dec.				
Tuesday . . 1	42	53	29 34	29·37
Wednesday 2	42	49	29·39	29·57

Prevailing winds, S.E. and S.W.

Except the 2d, generally cloudy; with frequent showers of rain.

Rain fallen, 1 inch, and ·35 of an inch.

Solar Spots.—The spots on the sun are yet numerous and large.

Thursday . 3	from 31	to 52	29·60	to 29·65
Friday . . . 4	38	47	29 65	29 85
Saturday . 5	33	45	30·07	30 15
Sunday . . 6	34	43	30 09	30·05
Monday . . 7	28	42	30·04	30 07
Tuesday . 8	33	43	30 03	29·74
Wednesday 9	36	42	29·67	30·09

Prevailing winds, S.W. and S.E.

Generally cloudy; with frequent showers of rain.

Rain fallen, ·225 of an inch.

Charles Henry Adams.

NOTICE.

"P.," who lately exposed the trickery of Henderson's Cuvier, will find a letter for him at our publishers.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 19, 1835.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

By Jon. Pereira, Esq., F.L.S.

Lecture XII.

The last order of mammals in the Cuvierian arrangement is that denominated

CETACEA,

from which we obtain Spermaceti. In exterior form these aquatic animals are like fishes; they have no posterior extremities, the tail terminates in a horizontal fin (in this respect, however, differing from the tail of fishes, which is always vertical), and the anterior extremities form likewise fins. You will not, therefore, be surprised to find that all the early naturalists classed them among fishes. Our countryman, Ray, called them "*pisces pulmone respirantes;*" and Linnæus even, in the earlier edition of his *Systema Naturæ*, committed the same error. But a little examination will soon satisfy you they deserve a much higher position in the animal scale. They respire by means of lungs, and hence are compelled to rise frequently to the surface of the water (in which they are assisted by the horizontal direction of their tails) to take in air. They have warm blood, the sexes associate in the manner of terrestrial animals, they bring forth living young, and they have two mammæ (either abdominal or pectoral) to suckle their offspring; so that having the same general structure as mammals, and performing the same general functions, they must of necessity be classed with them.

To the characters I have already mentioned as distinguishing this order, I have only to add, that the neck is very short, the pelvis rudimentary (consisting merely of two bones unconnected with the rest of the skeleton), the lateral expansions of the tail are cartilaginous, and not osseous as in fishes; and in some of the cetacea there are teeth, while in others these organs are replaced by horny plates (*whalebone.*) Although there is a considerable variation in their development, yet the enormous size of many of these animals is one of the most amazing facts in their history.

Zoologists make two families of cetacea: *one* is called the *Sirenia*, by Illiger, Goldfuss, and others, but by Cuvier the *Cetacea herbivora*. In this are comprehended all those animals endowed with the faculty of leaving their watery habitation to crawl and pasture on the shore, and which have two pectoral mammæ. Here are contained those beings which have given rise to the fabulous accounts of the mermaids, the sea-nymphs, &c. The *second* is the *Cete*, or *Cetacea ordinaria*, of Cuvier, and which are piscivorous. The animals of this order have a smooth, shining, naked skin, covering a thick fatty layer. The mammæ are two in number; but instead of being pectoral, as in the herbivorous cetacea, they are placed near the genital organs. They swallow an immense quantity of water along with their prey, and get rid of it through the spout hole at the top of the head; it is expelled with great violence by the action of powerful muscles. From this circumstance these animals have been termed *hydraula*, *souffieurs*, or the *blowers*. The following table will explain the subdivisions of cetacea:—

				Genera.
Cetacea	with two pectoral mammæ (*Sirenia*, seu *Cetacea herbivora*) ..			Manatus. Halicore. Rytina.
	with two abdominal mammæ (*Cete*, seu *Cetacea ordinaria*) ..	Head in ordinary proportion to the body		Delphinus. Monodon.
		Head inordinately developed ..	Teeth in lower jaw	Physeter.
			no teeth	Balæna.

PHYSETER MACROCEPHALUS.

History. — The genus physeter (from φυσαω, to blow) contains those whales usually denominated *cachelots*, a term derived from the word *cachau*, and signifying in the Biscayan language, a *tooth*. The number and characters of the species are badly determined; but the *long-headed cachelot*, called by Shaw and Bonnaterre the *P. macrocephalus* (from μακρος, long, and κεφαλη, head) is the most common. It is supposed by Cuvier to be the *orca* of the Latins, and the *physeter* of Pliny. The first correct figure and description of the animal was published by Clusius in 1605.

Description.—The length of this animal is from 50 to 60 feet; its head constitutes about one-third of the length of the whole body. The jaws are small; the lower one is shorter, smaller, and lower, than the upper, and is furnished with from twenty to twenty-three teeth on either side. Shaw states that an accurate inspection of the upper jaw proves there are corresponding teeth in that also; but they are very small, and situated so deeply within the sockets, as to be totally invisible on a general view.

The presence of teeth in the lower jaw readily distinguishes the cachelots from animals belonging to the genus Balæna, which in the adult state are destitute of teeth, though in the fœtus they are observed, but soon disappear. In the upper jaw of the Balæna we meet with numerous broad, horny, flexible plates, disposed in regular rows on each side, and which are commonly known by the name of whalebone. The fringed edges of these serve as a kind of strainer, and to retain the prey, the water being allowed to escape. The great Greenland, or whalebone whale, is the *B. mysticetus*, and is from 50 to 60 feet long.

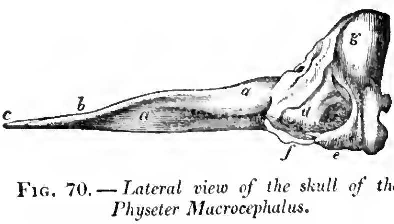

FIG. 70. — *Lateral view of the skull of the Physeter Macrocephalus.*

The snout of the cachelot, notwithstanding its prodigious length, is formed, as in the dolphin, only by the maxillæ (*a a*, fig. 70,) on the sides, by the intermaxillæ (*b b*) towards the median line, and by the vomer (*c*) on this line. The intermaxillæ project to form the anterior part of the snout. Posteriorly the right one ascends higher than the left. The spout hole is single (in most cetacea it is double), and directed towards the left side, so that whenever the animal spouts water, it is to that side only.

Seat of spermaceti.—Spermaceti is found in several parts of the body of the animal, mixed with the common fat. The head, however, is the grand reservoir for it. Here it is found (mixed with oil) in a large excavation of the upper jaw, anterior to, and quite distinct from, the true cranium which contains the brain. (See fig. 70, where *d* represents the parietal bone, *e* the zigomatic apophysis, *f* the jugal bone, and *g* the occipital bone.) Mr. Hunter states that the spermaceti and oil are contained in cells, or cellular membrane, in the same manner as the fat in other animals; but besides the common cells there are larger ones, or ligamentous partitions going across, the better to support the vast load of oil, of which the bulk of the head is principally made up.

There are two places in the head where this oil lies; these are situated along its upper and lower part: between them pass the nostrils, and a vast number of tendons going to the nose and different parts of the head. The purest spermaceti is contained in the smallest and least ligamentous cells; it lies above the nostril, all along the upper part of the head, immediately under the skin and common adipose membrane. These cells resemble those which contain the common fat in the other parts of the body nearest the skin. That which lies above the roof of the mouth, or between it and the nostril, is more intermixed with a ligamentous cellular membrane, and lies in chambers whose partitions are perpendicular. These chambers are smaller the nearer to the nose, becoming larger and larger towards the back part of the head, where the spermaceti is more pure.

Such is Mr. Hunter's account of the locality of spermaceti. He adds that he discovered about the nose, or anterior part of the nostril, a great many vessels, having the appearance of a plexus of veins, some

as large as a finger. On examining them, they were found loaded with spermaceti and oil; and some had corresponding arteries. They were most probably lymphatics; therefore he supposes their contents had been absorbed from the cells of the head.

At one time spermaceti was supposed to be the seminal fluid of the whale, and from this received its name. It was then thought to be the brain of the animal. We now know its nature, though we are not acquainted with its precise use in the animal economy. The enormous quantity of fatty matter with which the cetacea are supplied, probably serves to preserve the heat and to diminish the specific gravity of these stupendous hot-blooded animals.

Has spermaceti been found in any other organized being than the cachelots? Chevreul states he obtained a substance very similar to, though not identical with, cetine (the pure part of spermaceti), in *Delphinus globiceps*; Vauquelin found a substance analogous to cetine in the *Agaricus campestris* (fig. 71); Gmelin says spermaceti is also procured from *Delphinus edentulus*. A crystalline matter (cetine?) is deposited from the common whale and other fish oils.

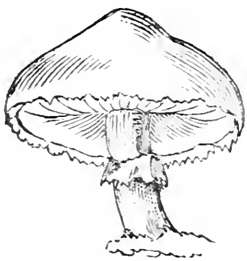

Fig. 71.—*Agaricus Campestris.*

Preparation of spermaceti.—When the fatty substance found in the head (hence called *head matter*) is exposed to the air, it concretes into opaque masses. It is dug out, and the oil allowed to drip from it. When brought to England, it has a yellow semifluid or unctuous appearance; and in order to remove as much of the oil as possible, it is submitted to compression in hair bags, placed in an hydraulic press. It is then melted in water, and the impurities skimmed off: subsequently it is remelted in a weak solution of potash. By fusion and proper cooling, it is obtained in the form met with in the shops.

Properties.—The spermaceti of commerce usually contains a small portion of yellow oil: if this be separated, the pure product is called *cetine.* The oil is best removed by boiling the spermaceti in alcohol.

Cetine is a white laminated substance, without taste, and almost odourless. It is insoluble in water, and slightly soluble only in alcohol when at a boiling temperature. By saponification with alkalies, we obtain margaric and oleic acids, a yellow extractiform substance, and *æthal.*

100 parts of *cetine* yield (with probably 2·5 of water)	Margaric and oleic acids ..	60·96
	Æthal	40·64
	Extractiform matter	0·9
		102·50

To obtain these products, we proceed as follows:—Digest one part of cetine with one part of hydrate of potash and two parts of water: this yields us spermaceti soap. When this is decomposed by an acid, the extractiform matter is left in solution, while the fatty acids and the æthal are separated. As there is an increase of 2·5 per cent., we must refer this either to the air or water (most probably the latter), the elements of which have entered into new combinations.

Æthal is a crystalline, solid, non-saponifiable fat; it is odourless, tasteless, combustible like wax, and may be distilled. When fused, it solidifies at about 118°. It is insoluble in water, is readily soluble in hot alcohol and in æther. It is composed of carbon, hydrogen, and oxygen: the two first constituents were supposed by Chevreul to be in the same relative proportion as in æther and alcohol; and hence the word æthal was taken from the first syllables of these words.

	Carbon.	Hydrogen.	Oxygen.	Nitrogen.
Spermaceti (Berard)	79·5	11·6	8·9	0
Ditto (Saussure)	75·474	12·795	11·377	0·354
Cetine	81·660	12·862	4·578	0
Æthal	79·766	12·945	6·289	0

Physiological effects and uses.—Like other fatty bodies, the local action of spermaceti is emollient—that is, it relaxes the tissues to which it may be applied. When used to sheath surfaces it is called demulcent. The usual mode of exhibiting it internally is to suspend it in water by means of the yelk of an egg; but being a very troublesome preparation, on account of the fatty matter mixing less readily with water than many other oily substances, it is now rarely employed. In irritation of the alimentary canal, as dysentery and diarrhœa, and of the pulmonary membrane, as catarrh, it is occasionally beneficial.

Its most common medicinal use is in the formation of cerates and ointments. In the London Pharmacopœia we have a cerate and ointment of cetaceum, each of which is ordered to be composed of spermaceti, white wax, and olive oil, but in different proportions. As, however, most of the white wax met with in commerce is mixed largely with spermaceti to render it white, these preparations are never made according to the intentions of the College. Practically this is of no consequence.

AMBERGREASE.

This substance is a product of the spermaceti whale, but as it is not an article of medicine in this country, my notice of it must be very brief.

The best account of the method of procuring it is in the 57th volume of the Philosophical Transactions, for the year 1783. It is found floating on the surface of the sea. The best is brought from Madagascar, Surinam, and Java.

Ambergrease seems to be the fæcal matter of the cachelot, probably hardened and otherwise altered by disease. Some have considered it analogous to biliary calculi: its chemical properties favour this supposition.

It is a solid, opaque, greyish, striated substance, having a pleasant musk-like odour, and which is supposed to be derived from a species of cuttle-fish (*sepia moschata*) on which the animal feeds. In favour of this opinion must be mentioned the fact, that the horny beaks of a sepia are found imbedded in it. Its specific gravity is from 0·908 to 0·92.

It consists principally of a non-saponifiable fat, analogous to cholesterine, but mixed with excrementitious matter. This fatty substance has been termed *ambreine*. It is soluble in alcohol, and by the action of nitric acid furnishes a peculiar acid (*ambreic acid*).

Analysis of Ambergrease, by John.

Ambreine	85
Sweet balsamic alcoholic extract, with benzoic acid	2·5
Aqueous extract, benzoic acid, and chloride sodium	1·5
Loss	11
	100·0

Its action on the system is considered to be analogous to that of musk.

Class 2.—Aves, or Birds.

Having now finished our examination of all the orders of the class *Mammalia* which yield us any pharmacological agent, we advance to the next class—that of *Aves*, or Birds. Here the only substance which I have to notice is the egg of the common fowl; but before examining it I must premise that this class has received its name *Aves* from the Hebrew, and literally signifies *fliers*. Blainville calls them *Penniferæ*, from the circumstance of their skin being furnished with feathers. I regret that I cannot permit myself to detain you with any detail of the distribution, the structure, or the mode of development of feathers; the subject is an exceedingly interesting one, but I do not feel warranted in consuming so much time as would be requisite for its proper investigation. All that I think it right to say respecting birds is, that they are oviparous bipeds, and, therefore, have no mammæ; their blood is warm and red, with elliptical particles; their heart is furnished with four cavities. Being destined to live and be sustained in the air, birds have their anterior extremities formed into wings; their respiration is by lungs, the air penetrating even into the bones. In the skeleton of the animal we observe a peculiarity about the pectoral region: the two clavicles unite at an angle to form the *furcula* or merry-thought, and they are furnished with two coracoid bones. They have two larynges, the superior one simple; the inferior one, placed at the bifurcation of the trachea, is provided with muscles, and is the seat of the voice.

Orders of aves.—In the Cuvierian arrangement six orders are admitted, as follows:

1. *Accipitres* (from *accipiter*, a hawk), called also Raptatores, the birds of prey—such as the vulture, eagle, &c. They have short feet; their toes are armed with claws, and the bill curved or hooked.

2. *Passeres* (from *passer*, a sparrow), or the passerine birds, includes all the small singing birds; they have four toes, three in front and one behind, the outer toes being wholly or in part united.

3. *Scansores* (from *scando*, to climb) or the climbers,—sometimes called Picæ or the

Pies. It includes the Parrots. They have four toes, two in front and two behind.

4. *Gallinæ* (from *gallina*, a hen) called also Rasores, Gallinaceous birds, or commonly poultry, includes the domestic fowl. They have their toes in front, united at their base by a short membrane.

5. *Grallæ* (from *grallæ*, stilts), or the Waders, as the Ibis, have elevated naked tarsi, the two outer ones being united.

6. *Palmipedes* (from *palma*, the palm of the hand, and *pes*, a foot), the web-footed animals, or the Natatores, as the Duck. Their toes are united by broad membranes.

PHASIANUS GALLUS.

History.—Some doubt exists as to the origin of our common domestic cock and hen. Sonneret affirms that all the varieties originate from the jungle fowl (*Gallus sonnerati*, fig. 72); while Temminck refers them

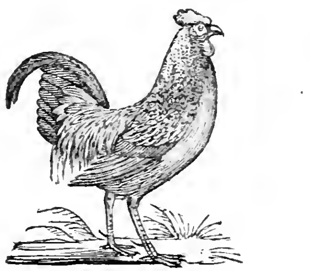

FIG. 72.—*Gallus Sonnerati.*

to the Javan fowl (*Gallus lanckiva*). If external appearance is to be a guide, the latter assertion is the more probable.

No mention is made of this animal in the Old Testament, but in the New, the male and female are each mentioned. However, the reference to the cock is involved in some doubt; since it has been supposed that the *crowing of the cock*, alluded to by Christ when speaking to Peter, had reference to the third watch of the night, called ἀλεκτοροφωνια, or *cock-crowing* watch. Aristotle calls the male animal ἀλεκτρυὼν, the female ἀλεκτορις: both terms are derived from ἀλεκτρος, without sleep.

Description.—This animal is so well known as to require very little description. The zoological characters of the genus are the following: — Bill somewhat thick, strong, and curved at the apex; the nostrils are placed at the base of the bill—they are half closed by an arched scale. The head is surmounted with a fleshy vertical crest, and the lower beak is furnished on each side with fleshy wattles. Tail compressed, and composed of fourteen feathers. The tarsi of the male are furnished with a long and bent spur. The varieties are very numerous.

Production of eggs.—The *yelk* of the egg is formed in the ovarium, from whence it passes into the oviduct, where it receives the *white*, or glaire; proceeding onwards, it afterwards obtains its *shell*.

1. *Ovarium.* — First let us commence with the *ovarium*, called also the *racemus vitellorum*, or the egg organ. This consists of a cluster of ova, in a hen beginning to lay, about 500; the stalk by which each ovum is attached to the ovarium is called the *petiolus.* The size of the ova is exceedingly various: when quite ripe, they are as large as the yelk of an egg; the smaller ones are white, the larger ones yellow. Each ovum, when ripe, is composed of a calyx—the yelk bag—and the yelk.

(*a.*) The *calyx* constitutes the outer coat or covering of the ovum, and consists of two layers—an outer one, derived from the peritoneum, and an inner one, which is somewhat thicker. Between these two coats the vessels ramify. The *petiolus* is merely a prolongation of the calyx: it is studded with a number of small ova resembling vesicles. On that part of the calyx of a ripe ovum which is opposite the petiolus, is a whitish curved stripe, called the *stigma*, indicating the spot where the calyx bursts, to allow the escape of the yelk.

(*b.*) *Yelk bag*, or *membrana propria vitelli.*—This is within the calyx, and closely invests the yelk. It is a flocculent, delicate, fine coat.

(*c.*) *Yelk*, or *vitellus, with the cicatricula*, or *tread.*—In the early state of the ovum, the yelk is constituted of a pellucid fluid lymph, and is hardly distinguishable from the *vesicula cicatriculæ.* It then becomes whitish, and subsequently yellow, globules of oil making their appearance. In a ripe ovum, it is viscid, tenacious, and of an orange-yellow colour; and lies in the calyx, with its long axis towards the petiolus. It is composed of three layers, (fig. 73,) the middle one having the deepest

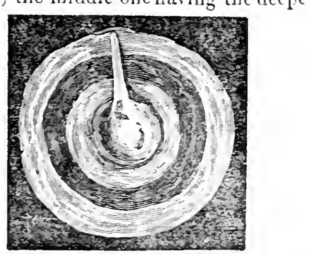

FIG. 73.

colour: the innermost inclosing a white fluid, called the *albumen centrale*, or the *substantia alba vitelli*; from which, according to Purkinje, passes a *little canal* to that part

of the surface of the yelk called the *cicatricula*.

The internal surface of the yelk-bag is lined with a very thin stratum of globules, in form and figure like those of the blood, but arranged organically. The *cicatricula*, or *tread* (as it is improperly called), is formed by an accumulation of these globules forming a mammiform heap, the convexity of which is towards the centre of the yelk, and is usually situated nearer the petiolus than the stigma. In the top of this is the so-called pellucid *pore*, which is occupied by a small vesicle discovered by Purkinje, and called by him the *vesicula germinativa*, or *vesicula cicatriculæ*. It is found in all

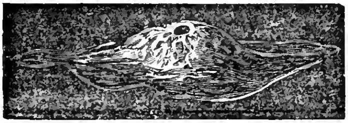

Fig. 74.—*Cumulus Cicatriculæ*, with the *Pore* in the centre: from Purkinje.

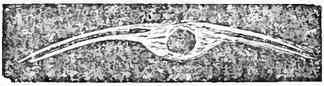

Fig. 75.—Section of the *Cicatricula*, shewing the *Vesicula* in sitû.

the ovarian ova, and seems to be a natural organ, since it is found in the ova of fowls which have never had access to the male, When the yelk falls into the infundibulum this vesicle disappears; in all probability being broken by the contractions of the oviduct. It is adherent to the yelk-bag.

2. *Oviduct*.—The oviduct has some resemblance to a convoluted intestine. It is situated on the left side of the animal. Its superior expanded free extremity is called the *infundibulum*, the edges of which are fimbriated. Inferiorly, the oviduct opens into the cloaca. It is attached to the spine by the *mesometrium*, usually regarded as analogous to the mesentery, but which Purkinje calls "a muscular membrane." From its upper end (called mesometrium superius) a process termed the *ligamentum infundibuli* passes to the last rib but one on the left side. The coats of the upper part of the oviduct are exceedingly thin; those of the lower part are considerably thicker. As the structure and functions of the different parts of the oviduct are different, some anatomists have divided it into three portions—namely, the *genital tube* (tractus oviductus, or oviduct, properly so called,) the *uterus*, or middle portion, and the *vagina*, or lower part, which opens into the cloaca. Of the propriety of some division of the oviduct, no person who examines it can doubt; but it must be admitted the above terms are objectionable, since the parts to which they refer do not perform the functions which their names would lead us to suppose. Thus the part called the *uterus* does not contain the ovum while the chick is developed. Let us examine the changes which the ovum undergoes in its passage through this tube.

The *infundibulum*, or expanded portion of the tube, receives the ovum as it escapes from the calyx of the ovarium. The upper part of the oviduct is lined by a fine villous membrane, covered with follicles secreting the albumen, or glaire and thrown into a number of longitudinal folds. The first layer of albumen which the ovum receives, forms the *membrana chalazifera* of Dutrochet; at either end of which is a soft, pellucid, albuminous nodule, which may be regarded as the *rudimentum chalazarum*. During the descent of the ovum in the oviduct, it receives fresh deposits of albumen; and as it undergoes spiral rotations in its passage, the above-mentioned processes become curved spirally, and in the perfect egg constitute the *chalazæ*, *grandines*, *appendices albuminis*, or the *poles* or *treddles*. From one chalaza to the other is observed in many eggs one or more white striæ, formed by a thickening of the membrana chalazifera. Vicq d'Azyr called this appearance the *zona albicans*.

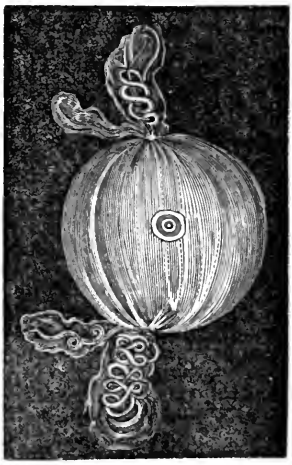

Fig. 76, showing the yelk, with the *spiral chalazæ* at the extremities, the circular *cicatricula* in the middle, and the *zona albicans* extending from one chalaza to the other.

The *albumen*, *glaire*, or *white* of the egg, is not uniform in its consistence. The thickest is that which is first deposited around the yelk. Proceeding from without inwards, the three layers of albumen are termed *Albumen primum*, *A. secundum*,

and *A. tertium.* Just before the egg arrives at that part of the oviduct called the uterus, it receives its outer coat, the *pellicula ovi.*

In the so-called uterine portion of the oviduct is formed the *calcareous shell.* Some are expelled without it; these are termed *oon eggs.* The chalk is first deposited in small polygonal pieces, having a crystalline appearance (fig. 77); but when the de-

FIG. 77.

posit has attained a certain thickness, all traces of crystallization are lost, though small pores are left, so as to allow a passage for the air. This is proved by the fact, that if an egg be varnished, so as to prevent the influence of the atmosphere, incubation is stopped.

Having now traced the egg from its first appearance to its most perfect state, we may proceed to describe its general properties.

General description of the egg.—The elliptical or oval form is well known. The specific gravity is from 1·080 to 1·090. By keeping the water evaporates, and in its place air is substituted; so that eggs become lighter. Dr. Prout found that in two years an egg lost $544\frac{3}{10}$ grains, or about 0·75 grain in twenty four hours. The relative weights of the different parts of the egg were, according to the same authority—

Shell and membrane	106·9
Albumen	604·2
Yelk	288·9
	1000·0

By boiling in water, an egg loses from 20 to 30 grains (supposing the original weight of the egg to be 1000 grains.)

1. The *putamen ovi* or *egg-shell* consists principally of carbonate of lime. Here are the analyses of it by Vauquelin and Prout:—

	Vauquelin.	Prout.
Carbonate lime	89·6	97
Phosphate lime and magnesia	5·7	1
Animal matter (with traces of sulphur and iron)	4·7	2
	100·0	100

This calcareous matter renders the shell absorbent and antacid, and hence its employment to neutralize the acid of wines.

2. The *pellicula ovi*, or the membrane lining the shell, possesses the characters of an albuminous substance; it dissolves in alkalies, and from this solution is precipitated by acids. It weighed about 2·35 grains (the whole egg being 1000 grains.) At the larger end of the egg it forms the *folliculus aëris.* According to Bischoff, the air in this cavity is similar to atmospheric air, with the exception that it contains more oxygen—namely, 23·475 per cent.

3. The *albumen ovi*, called commonly the *glaire*, or white of the egg, is on the whole, perhaps, the most interesting part to us in a medicinal point of view. It consists of two or three laminæ, which are deposited on the yelk while in the oviduct. These laminæ, however, are not homogeneous; for there are at least two parts detectable in them, namely, a solid, probably organized, albumen, having the appearance of a very fine delicate membrane, forming a series of cells, in which are contained the liquid albumen. The solid albumen is brought into view in two ways—either by shaking the glaire with water, when you observe a white, membranous, and insoluble tissue; or by examining the glaire by means of a microscope, two unequally refracting substances are evident. On moving the mirror of the microscope from right to left, the play of light (according to Raspail, and my own observations bear out his assertions) renders visible an indistinct interlacing net-work, an appearance which could not take place if the albumen did not consist of at least two substances Again, when the albuminous layer begins to dry, the transparent

texture is seen gradually to exhibit large indentations, and, as it were, a kind of large globules more or less agglutinated together, as well as anastomosing folds, resembling vessels. Now this could not take place were the albumen an homogeneous substance.

The glaire, or white of egg, has been analysed by Dr. Bostock. It consists of a coagulable substance (*albumen*), an uncoagulable matter (called by Bostock *mucus*, by Marcet *muco-extractive*, by Gmelin *salivary matter*), salts, and water.

	Gmelin.	Bostock.
Albumen	12·0	15·5
Mucus	2·7	4·5
Salts	0·3	0·0
Water	85·0	80·0
	100·0	100·0

Albumen is distinguished from all other animal substances by the circumstances attending its coagulation. It is not spontaneously coagulable, and hence is readily distinguished from the liquid fibrine of the blood; but it coagulates by heat, galvanism, acids, alcohol, many metallic salts, &c., and hence we readily know it from non-coagulable animal substances, as mucus, gelatine, &c. Caseum and serai are distinguished from the albumen of the egg by not being coagulable by heat, though they are by some other agents. The theory or cause of its coagulation is, in many cases, still a mystery, no explanation yet offered being satisfactory. It is probable, however, that the phenomena are different under different circumstances of coagulation.

It appears that albumen is a substance which can combine with either acid or basic bodies: with the first it forms, for the most part, insoluble, with the second soluble compounds. Thus most of the acids, added to an albuminous liquid, cause a precipitate, which, according to Thenard, possesses acid properties. I may show you, as examples, sulphuric, nitric, tannic, and gallic acids, as having this effect. Now I think we can best understand these changes by regarding the precipitates as insoluble salts, the base of which is albumen, and therefore we might term them *sulphate*, *nitrate*, *tannate*, *gallate*, &c. of *albumen*. On the other hand, albumen forms soluble compounds with the alkalies, which Berzelius has denominated *albuminates*, as if the albumen acted the part of an acid: in this white of egg, then, we have the *albuminates of potash and soda*. But certain metallic salts cause precipitates with albumen: as examples, I shall select corrosive sublimate, the persulphate of copper, the perchloride of tin, and the nitrate of silver. What is the nature of the change here? Thenard tells us that the precipitates consist of albumen, the metallic oxide, and some of the acid. Orfila says that the precipitate caused by corrosive sublimate is composed of calomel and albumen; though from a remark made by Berzelius, it may be suspected to be corrosive sublimate and albumen. I cannot help suspecting that these precipitates are also to be regarded in the light of double salts. We know that corrosive sublimate, sulphate of copper, chloride of tin, and the nitrate of silver, are all capable of forming double salts, in which they act as the acid or electro-negative ingredient. If this be correct, the precipitate caused by corrosive sublimate is to be regarded as a *chloro-hydrargyrate of albumen* (query? of chloride of albumen). Alcohol coagulates albumen possibly by abstracting water. How heat acts I will not pretend to offer an opinion, never having met with any hypothesis, in my opinion, at all probable. The liquid albumen of the white of egg is distinguished from that of the blood by its being coagulated by æther, which the latter is not.

The following are the relative proportions of the saline principles of the albumen of eggs, according to Prout, as determined by three experiments; the weight of each egg being assumed to be 1000 grains:—

Yellow oil (with crystallizable fat)	28·75
Albumen (containing phosphorus in some unknown state of combination)	17·47
Water	53·8
	100·02

The *mucus* of white of egg is not affected by heat, a solution of corrosive sublimate, nor by infusion of galls; but it is copiously precipitated by the subacetate of lead.

4. We proceed now to describe the *yelk* of the egg. By carefully removing a portion of the shell, you bring into view the *cicatricula*, a circular appearance on the

yelk. On whichever side you open the egg, you always observe this spot upwards; this is effected by the chalazæ, which are not attached in the axis, but a little on one side of the yelk, so that the latter revolving in the white by means of the chalazæ, the smaller, and, therefore, lighter portion always remains uppermost. Observe at each extremity of the yelk the twisted flocculent *chalazæ*. The yelk is inclosed in a sac called the *yelk bag*.

The yelk is regarded as a kind of emulsion, consisting of oil suspended in water by means of albumen. Its composition is as follows:—

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carbonate of do.	Lime, Magnesia, and Carbonate of do.
Experiment 1 ..	0·29	0·45	0·94	2·92	0·30
Experiment 2 ..	0·15	0·16	0·93	2·93	0·25
Experiment 3 ..	0·18	0·48	0·87	2·72	0·32
Average	0·206	0·463	0·923	2·84	0·29

Lecanu obtained a small portion of cholesterine.

The *oleum ovi* is thus obtained: The yelk of an egg, which has been boiled hard, is to be dried, and then digested in alcohol, which dissolves the oil; and by distilling off the alcoholic solution, we obtain the oil in a separate state. The oil may also be obtained simply by pressure from the yelks.

The saline principles found in the yelk are, according to Prout, as follows:

	Sulphuric Acid.	Phosphoric Acid.	Chlorine.	Potash, Soda, and Carb. of ditto.	Lime, Magnesia, and Carb. of ditto.
First Experiment	0·21	3·56	0·39	0·50	0·68
Second Experiment	0·06	3·50	0·28	0·27	0·61
Third Experiment	0·19	4·00	0·44	0·51	0·67
Average ..	0·153	3·68	0·37	0·426	0·65

I may remark here that it is probable the sulphuric acid obtained from albumen is a product of combustion, and exists in it as sulphur; the phosphoric acid in the yelk as phosphorus; the chlorine as chloride of sodium. There are some curious and highly interesting circumstances connected with the saline principles of eggs. Prout has established the fact that the earthy matter found in the skeleton of the chick when it quits the shell, does not pre-exist in the recent egg; so that the only possible sources whence it can be derived are the shell, or transmutation from other principles. Now the former is highly improbable for several reasons. It is tolerably clear that Dr. Prout believes in the capability of the vital energies to effect this transmutation, though he does not assert it.

Effects and uses of the albumen and yelk of eggs.—Both substances are nutritive; their local action is emollient and demulcent. The white, or albumen, is a valuable agent in the treatment of certain cases of poisoning—namely, poisoning by corrosive sublimate, the preparations of copper, and the perchloride of tin. In these instances it acts as a chemical agent. It is also useful in other corrosive or acrid poisons, as a demulcent or sheathing agent. The yelk has been employed for preparing emulsions (especially that with spermaceti), and its oil has been employed as an application to cracked nipples.

The white, or glaire, is used for clarifying liquids and wines. In coagulating, it entangles in its meshes the impurities, and either rises with them to the surface, or precipitates. When the liquid to be clarified does not spontaneously coagulate the albumen, it is necessary to employ heat.

LECTURES ON SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

By Dr. Latham.

ON SYMPTOMS.

Auscultatory Symptoms—Bronchial Respiration and Bronchophony—Dry Sounds; where and how they are produced; incident to several Diseases; peculiar to none—Their analogy to certain Sounds of the Heart in the mode of their production—Estimate of their value as Auscultatory Symptoms— Their value relative, not absolute; and little or great, according to circumstances—as seen in Phthisis, in Pneumonia, in Pleurisy.

Upon the subject of Auscultation, hitherto we have only considered certain sounds occurring during the act of respiration, and have endeavoured to estimate the amount of the information they furnish concerning diseases of the lungs, both by themselves and by their interference with the respiratory murmur. And we have found that, arising, as they do, from certain pathological conditions of the mucous membrane which lines the air-passages, they become *direct* symptoms of all those pulmonary diseases into which such conditions enter as ingredients.

The sounds which I have called Rhonchus and Sibilus, and large and small Crepitation, in the sense the terms bear at this hospital, and which others have called by other names bearing an equivalent sense, these sounds cannot carry us further in the diagnosis of pulmonary diseases than I have pointed out. Other sounds are required to detect pulmonary diseases, consisting of other pathological conditions, or occupying other structures.

Of these other sounds, some still respect the respiration, some the voice, and some the act of coughing. There are what are call the Bronchial Respiration and the Bronchial Voice, or Bronchophony; and as Bronchial Respiration and Bronchophony will always be found to denote the same thing, I will consider them together.

When there is Bronchial Respiration you hear the breathing, and when there is Bronchophony you hear the voice, as you never hear them when all is healthy. In health, the respiration reaches the ear through the chest, in a clear, smooth, uniform murmur. In health, the voice does not reach the ear at all through the chest, except when it is applied just opposite the first divisions of the tracheæ—viz. on either side the second and the third dorsal vertebræ: but in certain conditions of disease you perceive, at particular situations of the chest, while the patient breathes, audible *gusts* of air puffed in and puffed out of the lungs, instead of the smooth respiratory murmur; and at the same situations, while the patient speaks, a sort of humming or muttering, but no articulate word. Such is Bronchial Respiration, and such Bronchophony.

Now these sounds, accompanying the respiration and accompanying the voice, are called Bronchial, because they are formed in those first divisions of the air-tubes which are popularly called Bronchi. But the Bronchi themselves are not in fault; they need not themselves have undergone any disease or a change of structure whatever, in order to the production of these sounds.

Bronchial Respiration, or Bronchophony, arise when the lungs have undergone such changes of condition as are calculated to render them better conductors of sound than they are in their natural and healthy state.

Now there are so many diseases and so many morbid processes involved in, or contingent upon, those diseases, which have the common effect of rendering the lungs more solid, and thus augmenting their capacity of conducting sound, that Bronchial Respiration and Bronchophony cannot be diagnostic of any one in particular. The lungs are consolidated in phthisis by tubercles; in pneumonia, by effused lymph; in pulmonary apoplexy, by effused blood; in hydrothorax and empyema, by compression, from without, of accumulated serum and of pus;—and in each of these affections, Bronchial Respiration and Bronchophony are apt to occur. But they are not properly diagnostic either of phthisis or pneumonia, of pulmonary apoplexy or pleurisy.

In going round the hospital, I have often taken occasion to point out to you cases of phthisis, in which the sounds of the heart were audible over the entire chest, or a considerable part of it. With the sounds themselves, their kind, their rhythms, their succession, no fault was to be found; only they were heard beyond their natural limit, and were perhaps a little louder th n natural. The impulse of the heart, in the meantime, has not been remarkably strong; perhaps it has been remarkably feeble, and the general state of the circulation has not indicated disease.

In such cases, where, during life, the sounds of the heart have been heard thus constantly and habitually beyond the precordial region, I have found, upon examination after death, that the organ it-

self has not exceeded, and often that it has fallen short of, the natural and average dimension.

When the heart is perfectly healthy, it must depend upon conditions exterior to itself if the sounds which accompany its contraction be heard not only in the precordial region, but both there and beyond it. In the cases I have mentioned, it depended upon the condition of the lungs; which being rendered more solid by the tuberculous matter within them, and becoming better conductors of sound, conveyed the sounds of the heart extensively through the chest.

In particular instances of phthisis, where the patient has been long under my observation, I have sometimes remarked that the audible limit of the heart's sounds has varied from time to time, according to circumstances, which seemed to me not difficult to explain. During the first stages of the disease, and in proportion as the lungs have been more and more beset with *crude* tubercles, they have reached further and further beyond the præcordial region; during the later stages, and in proportion as the tubercles have been more and more changed into vomicæ, they have receded more and more within their proper bounds.

And, not in Phthisis only, but in other and curable diseases within the chest, are examples found of the sounds, which accompany the heart's contractions, being conveyed beyond the præcordial region, while the lungs are rendered more solid by the various products of morbid action; and again receding within that region when reparation has taken place, and the lungs have again become pervious and free. Pneumonia, pulmonary hæmorrhage, and pleurisy, will often, during their progress, conduct the sounds of the heart half over the chest; and the cure of pneumonia, pulmonary hæmorrhage, and pleurisy, will often bring them back again within their just limits.

In like manner, and for the same reasons, and under the same conditions of disease, do the *bronchi* acquire a resonance, or a voice, which in no wise belongs to them, when the surrounding structures are perfectly healthy.

Such I believe to be the true account of Bronchial Respiration and Bronchophony. They are dry sounds, according to the explanation already given, being not produced by the mingling of air with fluid.

It now remains to determine the value that belongs to Bronchial Respiration and Bronchophony, as Auscultatory Signs. If they be taken *absolutely* and *alone*, their value cannot be rated very high; for, inasmuch as they do not attach themselves to any single morbid process, but result from conditions that are common to many, they can never be *exclusively* trusted for the diagnosis of disease; yet they may be trusted very often for lending important aid towards it. In truth, their value consists rather in what they contribute as auxiliaries to other signs than as standing alone.

As auxiliaries their value varies very much, according to the circumstances of the particular case; and as other auscultatory signs present are more or less precise, and stand less or more in need of that confirmation which these are capable of contributing.

Bronchial Respiration and Bronchophony are worth very little in Phthisis, when Gurgling Respiration, and Gurgling Cough, and Pectoriloquy, have already put their authentic stamp upon the disease.

They are worth very little in Pneumonia, when the Respiratory Murmur has been gradually overcome by small Crepitation, and small Crepitation been succeeded by dulness, and no sound is now elicited by percussion, or yielded to the listening of the ear.

They are worth very little in Pleurisy, when already, over one half the chest, the ear detects no Respiratory Murmur, and percussion can produce no Resonance; when the heart is pushed from its proper seat, and the patient is fixed on this side or that, by a dread of suffocation if he move to the other.

Yet it may happen in these several diseases, that the Bronchial Respiration, the Bronchial Voice, may throw just that light which is needed upon a number of doubtful symptoms, and give just that guidance which is required to a right diagnosis.

Phthisis may exist; but you cannot certainly pronounce that it exists, though the flesh may waste and the strength decline, and the pulse be habitually more frequent than natural, and the breathing be habitually a little hurried, and some cough be habitually present without expectoration; from all these circumstances you cannot *certainly* pronounce the disease to be phthisis; you still need the help of some auscultatory sign to decide your diagnosis. But a little help is *now* enough. If the Bronchial Respiration or the Bronchial voice be *now* added, and be always present, and always found in the same place, you may securely trust either one or the other as the sure index of phthisis.

Yet bronchial respiration and bronchophony are not *absolutely* diagnostic of tubercles or vomicæ, or of any morbid process essentially phthisical. But it is enough that in this case they are *circumstantially* diagnostic; for as such they are infallible.

Pneumonia may exist; but if you can have no sure evidence of its existence,

though fever be present, and Small Crepitation be more or less diffused through the lung. But if to this Crepitation Bronchial Respiration or Bronchophony be added, these, which are but indirect symptoms of condensation of the lung, are, *under the circumstances*, as certain tokens of such condensation having already begun, as if the chest at some part already yielded no sound to percussion; and none either healthy or morbid to the application of the ear. Small Crepitation is, indeed, the auscultatory sign of inflammation in the smaller bronchial ramifications; yet as long as this is the *only* auscultatory sign, there is a hope that the inflammation may pass away without involving structures beyond them. But Bronchial Respiration and Bronchophony *are* enough to shew that it has involved other structures, and that air is admitted less freely into the pulmonary vesicles.

In cases of Pneumonia, Bronchial Respiration and Bronchophony will sometimes precede, by no inconsiderable period, those auscultatory signs which more directly declare an impervious state of some portion of the lungs. And in cases of Pneumonia, Bronchial Respiration and Bronchophony will often exist at one part of the lungs, while Crepitation still exists at another: whereas, dulness to percussion and to auscultation do not arise until the Crepitation has ceased, and then are found exactly in those situations where the Crepitation was before. Bronchial Respiration and Bronchophony seem to denote the growing incapacity of the general pulmonary vesicles to admit as much air as they ought: dulness to percussion and auscultation shew the exact portions of the lungs that are absolutely impervious.

Pleurisy may exist. Fever, dyspnœa, and pain in the side, are enough to create a strong suspicion of it; a *suspicion*, however, and no more. But only let the least auscultatory sign be added, and the suspicion becomes a certainty. Bronchial Respiratory, or Bronchophony, is as much much as you want. One or the other, found in any part of the affected side, and known not to have been there before, will at once fix the character of the disease. They show *absolutely* that the lung is beginning to suffer compression; and they show *circumstantially*, i. e. by their union with fever, dyspnœa, and pain in the side, that the compression in the present case is from fluid effused into the cavity of the pleura, which is the first effect of its inflammation.

But wait a little in this case: and, if the inflammation be not arrested, and if the effusion within the pleura increase, the Bronchophony will be attended by a peculiar echo. And this echo (if echo it be) is the very pathognomonic sign of Hydrothorax. It has been well likened to the bleating of a goat, and therefore called Œgophony. But is a sound that admits of varieties, and as been denoted with equal propriety by sveral similitudes.

Wait yet a little longer: and if the fluid still increase, the Bronchial Respiration, the Bronchophony, and the echo, and every the least perceptible sound, will cease.

Finally, concerning Bronchial Respiration and Bronchophony, if it be thought that they are things far too trivial to bear out such vast conclusions as, in one case, that the lungs are beset with tuberculous matter, and the disease is Consumption; in another, that inflammation, either in itself or in its products, has travelled beyond the brochial ramifications, and the disease is Pneumonia; and in another, that inflammation has fallen upon the pleura, and Hydrothorax is already begun, I repeat, that this mere Resonance of the Breathing and the Voice within the larger bronchi, is diagnostic of these diseases, not in itself, but *circumstantially*, and in the relation it bears to other symptoms.

And thus we see, in the daily practice of medicine, that things in themselves mean and of no account, do often, by their place and by their relations, gain a just preponderance over things more prominent and striking, and have the largest share in guiding our decision upon the most important questions.

Cavernous Respiration and Pectoriloquy—Dry Sounds—Where and how produced—Cavernous Respiration has many modifications—Whence these are derived—They cannot and need not be characterized by names—Conditions most favourable to Pectoriloquy—Gurgling Respiration and Gurgling Cough—Moist Sounds—How and where produced. These four Auscultatory Signs, Caveruous Respiration and Pectriloquy, Gurgling Respiration and Gurgling Cough, are chiefly concerned in the Diagnosis of Phthisis—Estimate of their value in each stage and form of this disease.

But there are other Auscultatory signs which respect the respiration and the voice—those, namely, which are called Cavernous Respiration, and Pectoriloquy. I will consider them together, as I did Bronchial Respiration and Bronchophony, because they too will be found to signify the same things.

We call it Cavernous Respiration when, during the act of breathing, a hollow sound reaches the ear through the walls of the chest from some circumscribed space within. And we call it Pectoriloquy,

when, during the act of speaking, the articulate words that are uttered reach the ear through the walls of the chest from a circumscribed space within.

Cavernous Respiration and Pectoriloquy both result from a cavity formed in the lungs and communicating with the bronchi. They are, in the sense already explained, Dry Sounds—*i.e.* not requiring the presence of fluid for their production.

In Cavernous Respiration the hollow sound probably does not begin to be formed until the air enters the cavity, and results altogether from reverberation against its sides; whereas in Pectoriloquy the articulate sound is first formed in the larynx, and then conducted down the trachæa, and entering the cavity, is merely augmented by reverberation against its sides.

I am convinced that the whole subject of Auscultation would have been better understood if a little less artifice had been used in the methods of teaching it—a little less industry to find a name for every sound that is heard. This very sound of "Cavernous Respiration" has been puzzled and perplexed to the student by the fancy of giving an express name to each of its modifications.

There may be, perhaps, now in the hospital, a dozen patients who have Cavernous Respiration; and in each one of them the sound, besides being cavernous, has some distinct peculiarity: it is large or it is small; it is a click, or a hum, or a squeak. It is like blowing into a bottle with a narrow neck, or into one with no neck at all; it is like the flapping of a valve; it is metallic; it is as if air was puffed *into* your ear, or as if air was drawn *from* it.

It would be easy enough to agree upon a dozen terms to designate the dozen different sorts of Cavernous Breathing in the patients now in the hospital; but then the next dozen patients would require as many new terms for as many sorts of Cavernous Breathing, which would still be different.

The varieties of Cavernous Breathing are doubtless owing to the different size and form and situation of cavities, and the different conditions of the surrounding lung.

A cavity may be very large or very small. Several bronchi may open into it, or only one. It may be a simple cavity, or it may have many chambers. Its sides may be condensed and equal, or rough and ragged. The lung around it may be solid and indurated, or pervious and vesicular. It may be near the ribs, or far from them; adherent to, or separate from, the pleura.

It is quite obvious that these different circumstances are calculated to modify the sound, which will, nevertheless, be always such as indicates a cavity.

Would you know what Pectoriloquy is, put the tube upon the larynx or trachea of a healthy man, and, when he speaks, his voice will seem to come through his throat, and pass directly up the instrument into your ear. Just in this manner does the voice reach the ear through the chest, when the conditions within are favourable to Pectoriloquy.

The conditions *most* favourable to Pectoriloquy are that the cavity should have dense walls, that it should be near the surface of the lung, and that, by mutual adhesion of the two pleura, the walls of the chest should themselves contribute to form it, and that the cavity itself should be empty. Thus a cavity that produces Cavernous Breathing may still not produce Pectoriloquy. It must not be too small, or it will not allow sufficient reverberation to the voice; it must not be too large, or the voice will be lost in an indefinite hum. Thus there may be a cavity without Pectoriloquy, but a cavity can scarcely exist without Cavernous Respiration.

There is yet another auscultatory sign, which belongs to the respiration. It results from the mingling of air with fluid in the act of breathing; but it is heard in a circumscribed space, and evidently proceeds from a much larger quantity of fluid than is capable of being accumulated in the mere bronchi within such a space.

The French call it Gargouillement; we call it Gurgling. It is essential to this sound that there be a cavity, and that the cavity contain fluid.

The Gurgling sound of the Respiration is exactly like that which a boy makes when he blows up soap suds with a tobacco-pipe.

Allied with the Gurgling Respiration, and requiring the same conditions to produce it, is the Gurgling Cough. I wish, however, I had another term for the sound which attends the act of coughing: for it is certainly different from this gargouillement of breathing. If you place your ear upon the chest, immediately over a considerable cavity containing pus, or any fluid that admits air to pass through it, and tell the patient to breathe rather deeply and rapidly, you will hear the sound of a hundred great bubbles agitating the fluid and bursting in continual succession. But if you place your ear upon the chest, and tell him to cough, the sound is as if the whole contents of the cavity struck against your ear at once. The one sound is a *gurgling*, the other a *plash*. I wish Plash was not such an awkward word; for it expresses the very thing.

In the effort of coughing, more air penetrates the cavity. It goes in and out of it with a rush. Moreover, it dwells longer in it, and while it is there, the whole chest suffers a succussion. Thus, by the act of coughing, not only are the air and the fluid mingled and agitated together within the cavity, but the cavity itself is shaken, like a bottle, against the ear.

This audible Plash of fluid is certainly produced during coughing, as well by the general succussion of the chest which then takes place, as by the forced impulse and agitation of air within the cavity itself. Indeed, mere succussion alone will produce it; and succussion occasioned by other means than by coughing. Very often, when the cavity has been close to the walls of the chest, and the patient has been much emaciated, after I have heard its contents Bubbling as he breathed, and Plashing as he coughed, I have desired him to hold his breath for a few seconds, and abstain from coughing if he could; and still I have heard distinctly a smaller sound of the same kind, a Plashing synchronous with the pulse. This sound is doubtless owing to the motion derived to the same cavity from the impulse of the heart.

And now for the practical application of the several Auscultatory signs; Cavernous Respiration and Pectoriloquy, Gurgling Respiration, and Gurgling Cough. To illustrate their diagnostic use I will take only one disease, but one which includes many and various morbid changes in the structure of the lungs, and see how far these signs, either by themselves or as auxiliaries to others, enable our knowledge to keep pace with such changes, as they arise. That disease shall be Pulmonary Consumption.

Let us consider Pulmonary Consumption in the stage of its first development, its most uncertain, but its most fearfully interesting stage. An individual is suspected to be Phthisical: he has some fever, some acceleration of pulse, some emaciation, and some cough; all inconsiderable in degree, yet all abiding; but no expectoration.

In a patient thus supected to be phthisical, auscultation may discover no more than this, that beneath the clavicle and about the scapula the Respiratory Murmur is less clear on one side than on the other, and that, where the Murmur is defective, there, too, the chest is less resonant to percussion.

Now if, after repeated examinations, auscultation comes always to this result, no doubt can remain that Tubercles are already formed in the upper lobe of one lung.

But here is no unnatural sound, only the natural sound is in part defective; and this must arise from some impediment to the passage of air through that portion of the lungs. Now impediment may arise from the deposition of lymph, or any of the common products of inflammation, as well as from Tuberculous matter. But inflammation is very unapt to take place, and its products to be effused into the apex of an upper lobe, while every other part of the lungs remains unaffected by it. It may ultimately reach this situation, but seldom, very seldom, begins in it. On the other hand, it belongs to Phthisical disease to deposit Tubercles in the upper lobes first, and thence gradually to scatter them over the rest of the lungs.

Thus by help of Auscultation, but still rather by what we do *not* hear, than by what we *do*, we arrive as surely at the persuasion that there are Tubercles in a certain case, as if there were sounds properly denoting their existence, which there certainly are not.

Always bear in mind that there are no Auscultatory signs which expressly bespeak tubercles. You are left to get at the knowledge of their existence by that sort of evidence which has been called circumstantial; Auscultation, however, having an important share in the result. As thus, Auscultation finds the Respiratory Murmur defective at a certain part of the lungs; and hence we infer its obstruction by the deposition of some kind of matter or other. But the part is that which nature chooses, above all others, for the deposition of Tuberculous matter; and hence we further infer that the matter is Tuberculous in this particular instance. But, moreover, the constitutional symptoms are such as are wont to accompany Phthisical disease; and hence we finally infer almost a certainty that Tubercles are deposited at the upper part of one lung. We conclude that the thing must be, because it *can* be nothing else. Circumstantial evidence, it is acknowledged, may be as infallible as the evidence which bears direct attestation to the simple fact.

I have been speaking of the Auscultatory Signs that we possess of Consumptive disease, when it has proceeded no further than the deposition of Tubercles (crude Tubercles) at the apex of one lung.

In proportion as Tubercles are more largely deposited in one lung, or extend to both, the Auscultatory Signs are the same in kind, but more definite; and the portions of lung that are healthy are more strikingly contrasted with those that are diseased. The healthy part, being called upon to compensate by a more energetic Respiration for the absence of Respiration in the diseased, gives out a louder and more Puerile Murmur, while the diseased gives out no Murmur, or almost none at all.

Recollect, I introduced the subject of Auscultation for the sake of illustrating to you the best means we have of knowing organic disease by direct symptoms. But, if there be any signs, not Auscultatory, which either give direct intimations of disease in the same organ, or tend to give more force and precision to the Auscultatory, it is fit that I should introduce them as I go along.

At no period of Consumptive disease is hæmoptysis more apt to take place than at this which we are now considering, when the lungs are beset by crude Tubercles. And the hæmoptysis illustrates the Auscultatory Signs, and the Auscultatory Signs illustrate the hæmoptysis.

The cases are numerous in the course of a twelvemonth which I see in this hospital and elsewhere, of hæmoptysis occurring in every quantity, from a tea-spoonful to a pint, in individuals whose general health has been previously declining, but who have had no particular complaint except (what they have called) a little hacking cough.

The great majority of such cases, as far as the mere hæmoptysis is concerned, do well. In a few days the hæmoptysis entirely ceases, and very often the patients tell you that they are better since they spat blood than they were before. It is my habit, however, to keep such patients still in the hospital for a week or a fortnight, to satisfy myself respecting the actual condition of their lungs, as far as I can learn it by Auscultation.

And Auscultation gives the same results which have been described. Respiration is unequally performed in different portions of ths lungs. There is less Respiratory Murmur perceptible about the scapula, or beneath the clavicle, on one or both sides, and less Resonance upon Percussion, than elsewhere.

I would invite your attention to all such cases as these, whenever you meet with them in the hospital. I recommend you particularly to exercise yourselves, whether by Auscultation or otherwise, in detecting the signs of Tubercles deposited in the lungs at the earliest possible period: because it is *then* especially that a sound knowledge of the real state of things may enable you to do infinite good, by seasonable, and (I will add) practicable advice to postpone the progress of disease, and protract many a valuable life.

I do not wish you to fasten on small points, and swell them into importance, and by refining and sophisticating to make something out of nothing at all, and frighten families, and deceive yourselves into a belief that you have cured Consumption.

The cases I am pointing out are those in which there are well-defined constitutional symptoms—fever, wasting of the flesh, acceleration of pulse. These denote something going on wrong somewhere. The cough fixes suspicion upon the chest; and the chest being examined gives such results as I have mentioned.

Let us now consider Pulmonary Consumption in other stages. In all stages, subsequent to the Tubercular, the Auscultatory sounds are paramount and unerring, and you may implicitly trust to them for your entire knowledge of each and every other morbid change and process incident to the structure of the lungs from Pulmonary Consumption.

The earliest and the least, but still a very authentic sign of Vomica, derived from Auscultation, is a mere Click, or slight ringing sound, heard in Breathing, at some point beneath the clavicle or about the scapula, in a patient in whom all the surrounding parts have been for some time dull. This Click, to remove all doubt of its being owing to the accidental lodgement of a piece of tough phlegm in one of the Bronchi, must always be found at the same point at several examinations of the chest. This is one modification of Cavernous Respiration. It results from a cavity or vomica in its first formation, when the Tuberculous matter is softening, and when it is just beginning to admit air.

Those who have been accustomed to attend me for any length of time in my visits round the hospital, must know how often I have pointed out this particular sign, this little Click in the Breathing, and desired them to listen to it day after day, until they gradually found it change into a Cavernous Respiration of a more certain character.

Where there is a Cavity, in the progress of its enlargement and of the changes it undergoes, its Auscultatory Signs are to be sought in Breathing and Coughing and Talking. Breathing will give them in one case, Coughing in a second, Talking in a third. Or often in the same case, whether the patient breathe, or cough, or talk, the evidence of a Cavity within the lungs is equally authentic.

Thus the Respiration, the Cough, and the Voice, may all give equal assurance that vomicæ exist; one confirming the other, and all agreeing in the same result.

But in particular cases, from the situation of the Vomica, from its size, from the kind and quantity of its contents, or from the state of the surrounding lung, Auscultation gives sometimes less and sometimes more notice of the disease by the sound conveyed in one of these actions than another. One is needed to supply the symptom which another does not give.

Some time ago I was desired to pro-

nounce upon the nature of the disease, in a gentleman who was affected in this manner. He had suffered a long and abiding Hectic, and had reached a state of extreme Emaciation, but had a very slight Cough, and expectorated only one large globule of yellow heavy matter once a day, immediately after he woke in the morning. His little Cough, his little Expectoration, and his ability to inflate his lungs freely and deeply, encouraged a hope that he still might not have Consumption, his abiding Hectic and his extreme Emaciation notwithstanding.

I examined the chest, and found the Respiratory Murmur clear and loud, and vesicular. In the act of Breathing there was no unnatural sound, either Cavernous or Gurgling, any where.

Having learnt thus much, or rather, having puzzled myself thus far, I was interrupted in my further examination by some accident, and I postponed it until the next day,

The next day I could get no more information from the mere Breathing, except that, upon the whole, the air entered more freely into one lung than the other, the other, how ever, not wanting the Vesicular Murmur in any part. Neither from the Voice could I get more information; it was neither Cavernous nor Pectoriloquous. Percussion elicited a somewhat different sound from the space between the clavicle and mamma on one side and the other. But the sound was dull on neither side.

What, however, neither the Respiration nor the Voice could declare by any authentic sign, was made clear and manifest by the act of Coughing; viz. that there was a large Cavity full of fluid occupying a space in one lung between the clavicle and the mamma. For when I desired the patient to make as deep an inspiration as he could, and then to Cough with all the force he was able, instantly there came Plash after Plash against my ear from the whole of this space, a sound which could only result from the agitation of fluid in a large Cavity.

But why was there Vesicular Breathing at this space? Probably because the Cavity, large as it was, had a considerable stratum of healthy lung interposed between it and the walls of the chest. Why was there no Pectoriloquy? These same conditions, the size of the Cavity, and the intervention of healthy lung between it and the walls of the chest, were enough to prevent it. Besides, the Cavity was full, and thus was unfavourable to Pectoriloquy.

And why, above all, was there no Gargouillement, no Gurgling sound in the Respiration, and little or no Expectoration? The air during ordinary Respiration might not have free access to the Cavity. The Cavity was there, but there might be no considerable bronchus entering it. Or, what is most probable, a considerable bronchus or bronchi entered it, but were obstructed by some obstacle, from within or from without, before they reached it. Either hypothesis will furnish the explanation, how a large Cavity full of pus can exist in the lungs, and yet not enough of air find its way *into* it, in ordinary Breathing, to produce an Audible Agitation of its contents, and not enough of matter find its way *out* of it to furnish more than a scanty Expectoration. In this case it took the whole night, and the continual oozing of pus by some narrow passage from the cavity into the Bronchi, to accumulate half an ounce ready to be expectorated in the morning.

So much for the present. But you must inquire further into the Forms and Stages of Phthisical Disease, if you would know the real value that belongs to the Auscultory Signs in question.

OBSERVATIONS AND RESEARCHES

ON

A NEW METHOD OF CURING CANCER.

By Alexander Ure, M.D., M.R.C.S.

Late House-Surgeon to the Royal Infirmary at Glasgow.

[Concluded from p. 394.]

The four following formulæ are given by Dr. Canquoin for preparing the phagedenic paste. The first three possess strength in proportion to the numbers 3, 2, 1; and the fourth, which is endowed with a peculiar property, will form hereafter the subject of special notice.

I. Chloride of zinc one part, wheat-flour two parts.

II. Chloride of zinc one part, wheat-flour three parts.

III. Chloride of zinc one part, wheat-flour four parts.

IV. Chloride of zinc one part, chloride of antimony half a part, wheat-flour two parts and a half.

Twenty-four to thirty drops of water are to be added for each ounce of chloride.

The preparation of the phagedenic paste requires the utmost care and attention; hence, to procure it properly the following instructions must be scrupulously followed. The chloride of zinc, reduced to powder, is to be mixed, as quickly as possible, on a slab, with the

given quantity of flour. One half of the mixture is immediately to receive its proportion of water, and to be worked up progressively with a spatula, until it forms a homogeneous paste like honey. This paste is to be brought to the desired stiffness by trituration with the remainder of the dry ingredients, well beat for a few seconds, and then rolled out into cakes or wafers, of from half a line to four lines in thickness.

The quantity of water must be proportionally augmented, according to the increased amount of flour in the second and third formulæ.

The antimonial paste, No. IV., is to be moulded into a crayon shape; because, as it preserves constantly the consistence of soft wax, a suitable thickness can always be given to it, so as to adapt it to the form of certain cancerous tumors, presenting inequalities of surface.

As, however, the flour employed in the above formulæ consists of starch, gluten, and vegetable albumen, it will produce a complex combination with the chloride in the phagedenic paste, which may blunt or interfere with its erosive action; or possibly undergo some fermentative change when applied to an ill-conditioned ulcer, and the viscidity of the compound give rise to more or less difficulty in the manipulation. It therefore occurred to me, that if some inert inorganic powder were substituted for the vegetable matter, capable of absorbing and retaining a sufficient quantity of moisture to form a paste, which should be a simple mechanical admixture, it would then be possible to turn to account the full escharotic powers of the chloride. The anhydrous sulphate of lime, in impalpable powder, will be found to realize the above conditions. Mixed with the chloride of zinc, in the proportions already indicated, a paste like putty may be obtained, after these have been well incorporated together with a few drops of water*.

The paste so prepared is perfectly plastic. In its composition the calcareous sulphate seems to perform the part of a porous medium, which allows the escharotic gradually to exude into the morbid texture. In proportion as it is abandoned by its deliquescent ingredient it acquires a firmer consistence, until at length it becomes concrete, and constitutes an impervious case for the eschar. I have already employed it in practice, and have found its corrosive properties to act effectually and beneficially in this form.

Manner of using the Phagedenic Paste.

Where the integuments are sound, the epidermis should be removed by means of a blister; and on the following day one or other of the above preparations, corresponding to the thickness of tissue to be destroyed, is to be applied to the cutis of the diseased part. The sensibility of the surface must also be considered; for should it possess but a feeble degree of vitality, the most powerful form is to be preferred.

The paste No. I. four lines thick, applied during four days, is capable of producing an eschar of from one and a half to two inches in depth. The same paste, three lines thick, applied during three days, will furnish an eschar of one inch at least in depth; the same compound, two lines thick, will in two days determine an eschar of not less than half an inch. The paste No. I., of one line, will yield, in twenty-four hours, an eschar of three lines. Finally, the paste No. I., of half a line, will produce, in the same time, an eschar of at least one line.

These changes will manifest themselves with the above precision only on tissues endowed with a considerable share of sensibility, and of which the consistence is nearly normal. In the gristly (lardacé), almost fibro-cartilaginous degeneration, about one-third is to be deducted from the thickness of the eschar above mentioned.

No. II. is employed in cases of cancerous ulceration and superficial carcinoma, which are attended with much pain.

No. III. is eligible in every species of cancerous affection, occurring in nervous subjects who are incapable of supporting the violent pain which the preceding more concentrated escharotics might occasion. It is so much less productive of suffering, as it is slower in its action.

Lastly, the antimonial paste is best adapted to nodulated cancerous tumors, for which a more decided escharotic action is required.

These preparations, applied over a denuded surface, excite, in a few mi-

* Special care must be taken to prepare a pure sulphate, by calcining the crystallized gypsum at a gentle heat in an oven; for the Paris-plaster of the shops is often sophisticated with chalk or whitening, which would immediately decompose the metallic chloride, and impair its qualities in a greater or less degree.

nutes, a feeling of heat, which, ere long, rises to a burning heat; which unpleasant symptoms may be relieved by an opiate enema.

When the operation of the paste is complete, it may be gently taken off, and the eschar covered with an emollient poultice until its separation, which usually happens, as formerly stated, from the eighth to the twelfth day, according to the thickness of the layer employed. The application is to be repeated again and again, till the whole morbid structure is removed; after which the surface is to be treated with simple digestive ointment; or, in case of acute cancer, with cataplasms, until the cure is finished.

In certain modifications of carcinematous tumors that are voluminous and prominent, Canquoin, instead of applying the caustic on the anterior segment, surrounds the base with a ring of paste two lines broad and four deep.

Should the carcinoma present a central depression, its destruction may be accomplished by using the phagedenic paste in a spiral form.

The chloride of zinc will probably form a valuable means of curing incipient cancerous ulcers of the uterus. These, in their early stage, may be looked upon as purely local affections, since Bayle has demonstrated, by numerous dissections, that the tissues of the organ are perfectly sound at the distance of two or three lines above the solution of continuity. In some instances it is reported that they have been healed by the topical use of the acidulated nitrate of mercury, as also by the caustic potash: but as both have been objected to in practice, the former from its tendency to spread over the healthy parts and excite inflammation, the latter because it is too uncertain in its effects, corroding too profoundly, and giving rise to fatal inflammation, amputation of the cervix has come to be regarded, by many, as the sole alternative for eradicating the disease. Now, the chloride of zinc, liable to none of the inconveniences that have been imputed to these other caustics, employed in its most concentrated form, will furnish its characteristic dry, easily definable eschar, destroying the morbid parts, and leaving behind a healthy surface, which will rapidly cicatrize: thus saving the patient the necessity of submitting to an operation which, although unaccompanied by much pain, is yet most repugnant to her feelings.

Cauterization with the chloride must likewise prove advantageous in carcinomatous affections of the tongue, lips, and of the rectum, where arsenic, from its powerful poisonous qualities, is totally precluded.

The following ointment is recommended for effecting the destruction of extensive schirrhous surfaces, previously flattened by long-continued methodical compression, as also of certain dense scirrhi. This has been technically named, the resolvent pomade.

Take of oxygenated pomade, prepared by triturating eight parts of boiling axunge with one of nitric acid, one ounce; melt this by a gentle heat, and add to it three drachms of the acid subdeuto-nitrate of mercury. Increase the heat a little, till the nitric acid become decomposed, so as to peroxygenate the pomade, and bring the salt into intimate union with it. The ointment, when well prepared, is very hard, and of an orange-yellow colour.

Dr. Canquoin describes the case of a woman affected with a schirrhus of one half of the face, who had been previously treated for it, but without benefit; and she seemed doomed to become, ere long, a victim to the frightful malady. However, thirty-five days' application of the above ointment sufficed to work a cure, without the slightest trace being left.

In two or three months time, and even less, a softening and suppuration have been procured of indolent schirrhous tumors, of a violet-red hue, by the following topical application, called the maturative ointment:—

> Take of the acetic infusion of the Bark of Spurge Laurel, ʒiss.; of Molasses, ʒiss.; Olive Oil, ℥j.; Ox Bile, ʒij. Mix together, and reduce, by the aid of heat, to the consistence of an ointment. Withdraw the heat, and add,
>
> Unguent. Basilic. ʒiss.; Cerati fusci*, ʒiss. Mix the whole well together, and incorporate with each ounce a drachm of the subdeuto-nitrate of mercury.

Inflamed cancerous tubercles, superficially situated, have been destroyed by applying to them, for a few days, a solution containing five or six grains of

* Or Onguent de la Mère, prepared with litharge, axunge, butter, and mutton suet, of each 25 parts, yellow wax 18, and black pitch 8.

cyanide of potassium to one ounce of distilled water.

With the external means above enumerated a happy result may be anticipated in the generality of cases. When there prevails a cachectic state, or confirmed cancerous diathesis, or when the carcinoma is voluminous, and has formed a multitude of firm adhesions, or is of long standing, and has occasioned extensive ravages, no reasonable hopes of recovery can be looked for from any mode of treatment.

In every instance of voluminous tumor, removal by the knife must still continue to maintain the preference, as offering a more speedy, and consequently less painful method of extirpation. Indeed, in thick, firmly-attached swellings, the scalpel ought to commence the operation, and the caustic be made to complete the cure. In order to eradicate fungous growths, it is indispensable that they be first of all snipped away, so as to bring the escharotic into direct contact with the roots of the disease.

The secondary indurations, that make their appearance in the vicinity of parts that have been cured, depend on diathesis, and may be attributed to the circumstance of the phagedenic application having been confined to too circumscribed a space.

Without seeking to draw a minute parallel between the comparative merits of the extirpation of cancer by surgical operation and by the caustics in question, suffice it to say, that the latter never create any serious inconvenience, while an amputation of the breast may not always have the most favourable issue. Besides, the knife can in nowise modify the subjacent tissues, it can only eradicate the evil when not deep seated; and hence disease frequently returns after the most dexterously-performed operations.

The internal treatment is of essential importance. That proposed by Dr. Canquoin consists in the administration of small doses of dilute sulphuric and nitric acids, conjoined with some drops of Pearson's Solution*. Of the latter six drops are given at first; the dose is gradually increased up to seventy-two in the course of twenty-four hours.

* Pearson's Solution is made by dissolving a grain of arseniate of soda in an ounce of distilled water.

Where the menstruation is at fault, a syrup containing iron or iodine, or perhaps both combined, is to be substituted for the above.

Much benefit has been derived in cancerous affections from the use of the mineral waters of Hellbrun in Bavaria. the active ingredients of which are iodide and bromide of sodium. It is probable that the mineral spring of Ashby-de-la-Zouch, which abounds in bromide, might prove an excellent remedy, used either alone or conjoined with an iodic preparation.

Dr. Canquoin has annexed several cases in corroboration of the efficacy of his plan of treatment, from which we subjoin the following abstract:—

I.—A female, aged 50, was affected for three years with a cancer of the right breast, five inches in diameter, and an inch in thickness, accompanied with acute lancinating pain and great emaciation. A single application of the paste effected a cure of the local disease, and also of the general ailments, in the short space of seven weeks.

II.—A lady, aged 42, had been formerly affected with an enormous cancer of the right breast, which had been amputated by Dupuytren. A few months afterwards the cancer relapsed. Three isolated tumors appeared in the cicatrix. They were completely obliterated in seven weeks by two applications of the paste.

III.—Madame L. had a large cancer of the left breast, two inches thick, and three and a half in diameter; the axillary glands were enlarged. Two applications were followed by a cure, accomplished in about two months.

IV.—M. le Comte A., aged 86, afflicted for fourteen years with a cancerous affection occupying the left eyeball and its appendage throughout their whole extent, as well as the right temporal region, which had resisted the most energetic treatment. Several successive applications of the paste proved efficacious in curing this horrid affection in sixty-nine days.

V.—Benoit Ordel, aged 76, had a cancer for fifteen years the size of a hen's egg, on the middle of the left cheek, over the duct of Steno. He had

been subjected to a variety of treatment in the Hôpital St. Louis and elsewhere, without any advantage. He was restored to perfect health in twenty days.

VI.—M. D., aged 77, since his nineteenth year had been affected with a cancer the size of a goose's egg, covering the whole temporal region, a part of the eyelids, and of the left cheek. As in the preceding case, various means had been tried in vain; but two applications of the paste proved successful in the course of two months and a half.

VII.—Madame L. had a cancerous affection of the face, of twenty-two years' duration, which had resisted an operation by M. Dubois, and afterwards a variety of caustics. It gave way in the course of a month to the phagedenic paste.

VIII.—Mons. Valerien Vendeuil, affected in the face with a cancer of seven years' standing, which had destroyed the nose altogether, and a great part of the left lower eyelid. After having been treated for several years without benefit at St. Louis, he was finally cured by Dr. Canquoin's method in four months.

IX.—Demoiselle Laurence Prévost, aged 16½ years, for the eight preceding years had been affected with a cancerous affection of the face, which had destroyed the nose in whole, and the upper lip in part. The tongue was perforated throughout its entire thickness, and the pharyngeal region had greatly suffered. This hideous malady had for years baffled the most skilful practitioners, and this young woman seemed devoted to a speedy death; yet she was cured in about five months.

Two cases are related of grave and deep-seated fungus, in which the method failed, as might have been expected.

A case is adduced in proof of the proposition above laid down, that the cure of external cancer will not prolong the existence of individuals who labour under a confirmed cancerous diathesis.

CONCERNING THE DEFECTS AND RESULTS OF ENGLISH PARISH REGISTERS;

AND THE COMPARATIVE INFLUENCE OF DOMESTIC MANUFACTURE, OR FACTORY LABOUR, ON FEMALE MORTALITY.

To the Editor of the Medical Gazette.

Sir,

Having discussed at some length, in my last letter, the degree of accuracy which may be fairly attributed to the several enumerations of population in Great Britain, I have now to treat of the other great branch of our successive population inquiries, which consists of a collection of the baptisms, burials, and marriages, recorded in the parish registers of England. Historically, I may be permitted to say, that, in the year 1801, the number of baptisms and burials in each decennary year (1700, 1710, 1720, 1730, 1740, 1750, 1760, and 1770), was called for by that part of the population inquiry which sought to ascertain "the increase or diminution thereof;" also the baptisms and burials of every year from 1780 to 1800 (both inclusive); and the marriages from 1754, when the marriage act came into force, since which time the accuracy of that part of parish registers is guarded by such precautions as render it unimpeachable. It is scarcely requisite to add, that from the year 1800, the successive population inquiries have obtained parish register returns to the year 1830 inclusive; and on the last occasion, the date at which the register books of each parish commence, was ascertained, and appears in the Population volumes.

From the acknowledged defect in the registers of baptisms and burials, I must be content to circumscribe the inaccuracy within such limit as can be reasonably established; and in such attempts I have always obtained the best illustration by making use of the established division of England into counties, and taking advantage of the particular county which appears best applicable to the subject under discussion. Thus, in shewing the effect of a greater or less increase of population on the classified ages of the existing population*, I

* See Medical Gazette, vol. xvi. p. 270.

contrasted Herefordshire and North Yorkshire with Lancashire and West Yorkshire; and, on the present occasion, I prefer Cornwall to any other county, not only because its parish registers are nearly perfect, but because it consists of an extensive promontory, and partakes of the character of an island, in its separation from the rest of the kingdom. It is a very healthy county, and contains no large towns, which last circumstance is very favourable to the accuracy of parish registers.

The number of children born in England, as compared to the number of marriages, is 441 to 100*; so that 440 to 100 (or, as stated in the Edinburgh Review, $4\frac{2}{5}$ to 1†) may be deemed an unexceptionable estimate. Herein I avoid consideration of second marriages, of still-born infants, and even of illegitimate children; of the latter, not only because Cornwall produces nearly the same proportion as the rest of England, but because the custom in France and elsewhere, of assuming all children to be illegitimate who are deposited in Foundling Hospitals, precludes any just reference or comparison in adverting to legitimate children exclusively; the poverty and misery of married parents too often driving them to the resource of a Foundling Hospital, where such institutions exist.

The power of being able to assume a certain number of children to each marriage, is particularly important in a nation where the marriage register, and no other, is accurate and complete; and hitherto there is no reason for thinking that the proportion is decreasing in England (nor, according to M. Quetelet, in Belgium), although in France it has fallen from 431 to 100 in the year 1818, to 361 in 1830; 16 or 17 per cent.‡ Assuming, in Cornwall, 440 children to 100 marriages, the number of births during ten years (1821-1830) would have been 90,728 from 20,620 marriages; and the registered *baptisms* (with those known to have been omitted) were, in fact, 89,243*—a small deficiency, unworthy of notice; and in proof that the register of *burials* in Cornwall is not materially defective, it appears that the actual increase of inhabitants between 1801 and 1811, was 3684 less than indicated by comparison of the registered baptisms and burials; the increase was greater, 1811 and 1821, by 1603; less again, between 1821 and 1831, by 1401†; all the three variations such as are within the range of emigration and immigration in a maritime county.

The importance of approximating (by such inference as has been described) to the number of actual births and deaths in Cornwall, will not be undervalued, when it is considered that, throughout England and Wales, in the aggregate, one-fourth of the births (24 per cent.) do not appear in the baptismal register, and that nearly 20,000 burials in a year are not regularly inserted‡. The total number of the deceased during eighteen years (1813-1830), whose age is recorded, was 3,938,496§; an addition of one-tenth to which fulfils the presumed number of deaths during the same eighteen years. These deaths, classed according to the ages of the population of 1821 (nearly a middle term in the said eighteen years), afford a competent exemplification of mortality, showing how far it varies in the several counties at various periods of human life, and in what degree the mortality of males and females differs; this last-mentioned comparison being entirely undisturbed by considerations of the increase of population, or of error or defect in the registry, because these sources of inaccuracy must have been equally incidental to both sexes in the same county.

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* See Medical Gazette, vol. xvi. p. 589.

† See Edin. Review, March 1829, p. 31.

‡ These proportions result from the Table of Mr. Corbaux, p. 144 of his book on *Population, Vitality, and Mortality:* and M. D'Ivernois has investigated this subject with his usual acuteness, in a letter addressed to M. Villermé (October, 1834), on the Movement of Population in France. He has divided the fifteen years (1817-1831) into three quinquennial periods, in the first of which appear 212,814 marriages, 953,638 births; in the last five years (1827-1831), 253,742 marriages, 975,160 births: as 448 to 100 marriages in the first period; as 384 to 100 marriages in the last period—a decrease of 14 per cent., or one-seventh part. The solution of this otherwise increbible decrease of fecundity, is thus given by M. D'Ivernois:—"Mais ce nouvel ordre de choses, fondé sur la convenance et la possibilité de mettre des limites volontaires à la fecondité des mariages, souleve une question d'une si grande delicatesse, que jusqu'ici les economistes les plus hardis n'osent guère y toucher que par des insinuationes detournées!" M. D'Ivernois elsewhere says, that in Geneva (his own residence) the decrease had arrived at 275 births to 100 marriages, or 37 per cent. less than in England, in Belgium, and (probably) in the northern departments of France.

* These numbers are obtained from p. 48 of the Parish Register volume of 1831, by adding seven annually to the number of marriages (for Jews and Quakers), and 3420 to the registered baptisms, at the rate of 342 annually.

† For particulars, see p. 50 of the same Parish Register volume of 1831.

‡ See Population Preface, p. xlv.

§ See Parish Register volume of 1831, p. 487.

Having arrived at this point, I am enabled to offer to your notice what may not be unacceptable in a Medical Gazette, although I confess myself somewhat at a loss as to the best manner of presenting such knowledge to your readers in a striking form, the entire detail of the several counties being too large for insertion; and I must be permitted to preface what I shall produce with a few explanations.

In contrasting the least with the greatest mortality in the several grades of life in the several counties of England, I also insert as a middle term the mortality of all England, which will not appear to be quite the same as that which is stated in a former letter of mine*; and for the following reasons:—That table is calculated on an average of *seven* years, three of them preceding, three succeeding the year 1821, when the ages of the population were ascertained. The statement of English mortality now inserted is calculated on the mortality of the entire *eighteen* years of recorded ages of the deceased, and being nine years on this side of 1821, only eight years anterior to that date, exhibits a higher average of mortality from the increase of population preponderating in the latter period, beyond that in the former and shorter period of eight years. The amount of difference may be seen in the Population Preface of 1831, (pp. xxxviii. to xlii.) being, on an average of eighteen years, nearly as 110,900 males to 108,000; 107,900 females to 105,900 annually during the respective periods: wherefore the mortality, as calculated on the population of 1821, is in that proportion greater, by making use of the higher average of deaths. A further variation of the same tendency arises from the omission of Wales on the present occasion, the Parish Registers there being more defective than those in England; yet the county of Monmouth (sometimes said to be more Welsh than Wales itself) is retained, although so remarkably defective in registry, that the marriages sometimes equal the number of baptisms in its most populous districts. The results of the Burial Register of this county (also very defective) having been inserted without explanation in the Population Prefaces of 1811 and 1821, gave rise to a disquisition unsupported by fact, and which therefore inflicted on me some pain, although such minute explanations are not quite admissible in a demi-official document. Let it now be understood that on the present occasion the results of this county *individually* have not been calculated, although it has been included as part of England, being merged in the mass as a kind of balance (somewhat inadequate) against the unfavourable considerations before described.

In comparing the other counties with each other, it is also to be understood that, for the sake of fair comparison, districts notoriously unhealthy, and several of the largest towns, are excluded from the bulk of some of the counties; and the detail of these exclusions must now be tolerated by those who examine with interest the various rates of mortality in the several counties.

The county of Cambridge is given exclusive of the Isle of Ely, a well-known marshy district, one-third of the entire county. Devon is given exclusive of Plymouth and its suburbs; not so much because Plymouth is a large town, as from the great proportion of migratory male population, constituting the garrison, the seamen of the royal navy there stationed, and mercantile seamen, in common with other ports. None of these can be peculiarly ascribed to Plymouth locally: whence the seeming mortality of males is greatly exaggerated from 10 years of age to 50 or 60; and from 20 to 30 years of age appears twice the rate of female mortality in the same town. The county of Southampton (Hampshire) is given exclusive of Portsmouth and its neighbourhood, for the same reason. Gloucester is given exclusive of Bristol. The county of Lancaster is given exclusive of Liverpool, the mortality of which is exceptionable, as the resort of many seamen, foreign as well as native; but Manchester, and the other large towns of this populous county, are not excluded, lest unfairness should be imputed to the calculation by those who maintain the unhealthiness of our manufacturing population. The West-Riding of Yorkshire is given exclusive of Leeds, but including its other populous towns, so as to make a fair comparison with Lancashire. The county of Lincoln is given exclusive of the parts of Holland, and the county of Norfolk exclusive of Lynn and its neighbourhood, both these districts adjoining the Isle of Ely (before mentioned), and liable to the same deleterious influence in a minor degree.

* See Medical Gazette, vol. xvi. p. 271.

The city of Norwich is also excluded from the county of Norfolk, so as to leave only an agricultural population for comparison with the adjacent counties. The county of Northumberland is given exclusive of Newcastle-on-Tyne, a port and also a large town. The county of Nottingham is given exclusive of the manufacturing town of Nottingham. The county of Surrey is given exclusive of Southwark, which (as regards mortality) is the worst part of the metropolis; yet the populous parishes adjoining the Borough of Southwark operate unfavourably in estimating the mortality of Surrey. The county of Middlesex is omitted as a county, being almost absorbed in the metropolis. The county of Warwick is given exclusive of Birmingham. The East-Riding of Yorkshire does not include the City of York, nor the port and populous town of Hull.

TABLE *exhibiting the Lowest and Highest Rate of Annual Mortality, in twelve specified grades of human life, in the several Counties of England; exclusive of Wales and of the County of Monmouth.*

I.—Annual Mortality under Five Years.

Males.	one dies in	Females.	one dies in
Hampshire	29	Hampshire	34
Cornwall	28	Cornwall	32
Wilts	28	Hereford	32
ENGLAND	19	ENGLAND	23
Stafford	17	Surrey	20
Worcester	17	Worcester	20
Surrey	16	Stafford	19

II.—Annual Mortality (5—9) during Five Years.

Males.	one dies in	Females.	one dies in
Suffolk	205	Suffolk	213
Hampshire	200	Hampshire	209
Rutland	198	ENGLAND	158
ENGLAND	145	Huntingdon	146
Chester	136	Cumberland	143
Durham	106	Durham	120

III.—Annual Mortality (10—14) during Five Years.

Males.	one dies in	Females.	one dies in
Cornwall	273	Cornwall	255
Devon	269	Devon	244
Rutland	265	ENGLAND	201
ENGLAND	211	Bedford	147
Chester	163	Northampton	142
Durham	139		

IV.—Annual Mortality (15—19) during Five Years.

Males.	one dies in	Females.	one dies in
Rutland	199	Cornwall	191
Devon	197	Devon	185
Hertford	181	ENGLAND	138
ENGLAND	147	Buckingham	106
Stafford	119	Northampton	103
Kent	115	Bedford	97
Durham	106		

V.—Annual Mortality (20—29) during Ten Years.

Males.	one dies in	Females.	one dies in
Rutland	118	Cornwall	132
Devon	117	Devon	132
Lincoln	117	ENGLAND	102
ENGLAND	96	Bedford	86
Durham	77	Sussex	85
Kent	64	Northampton	85

VI.—Annual Mortality (30—39) during Ten Years.

Males.	one dies in	Females.	one dies in
East York	124	Cornwall	111
Norfolk	111	Devon	110
Suffolk	111	England	85
England	87	Oxford	77
Chester	83	Stafford	75
Huntingdon	83	Chester	71
Kent	64		

VII.—Annual Mortality (40—49) during Ten Years.

Males.	one dies in	Females.	one dies in
East York	102	Devon	91
Norfolk	99	Rutland	91
England	68	Norfolk	89
Durham	64	England	71
Kent	56	Kent	66
		Chester	62

VIII.—Annual Mortality (50—59) during Ten Years.

Males.	one dies in	Females.	one dies in
Norfolk	61	Cornwall	67
Suffolk	58	Norfolk	60
East York	58	England	49
England	44	Huntingdon	45
Kent	41	Chester	43
Surrey	39		

IX.—Annual Mortality (60—69) during Ten Years.

Males.	one dies in	Females.	one dies in
Norfolk	32	Norfolk	32
Dorset	29	Northumberland	31
England	23	England	25
Hertford	21	Kent	24
Huntingdon	21	Chester	23
Surrey	19	Buckingham	23

X.—Annual Mortality (70—79) during Ten Years.

Males.	one dies in	Females.	one dies in
Northumberland	12·5	Northumberland	13·1
Norfolk	12·1	Gloucester	12·9
England	10·1	Norfolk	12·4
Essex	9·3	England	10·8
Surrey	9·3	Huntingdon	9·5
Hertford	9·1	Oxford	9·4

XI.—Annual Mortality (80—89) during Ten Years.

Males.	one dies in	Females.	one dies in
Lancaster	5·1	Gloucester	5·5
Gloucester	5·1	North York	5·4
East York	5·1	England	4·9
England	4·6	Rutland	3·9
Bedford	3·9	Cambridge	3·9
Northampton	3·8		

XII.—Annual Mortality (90—99) during Ten Years.

Males.	one dies in	Females.	one dies in
Dorset	4·1	Dorset	4·4
Lancaster	3·7	Huntingdon	4·0
England	2·8	England	2·9
Hereford	1·8	Hereford	1·5
Westmorland	1·2	Cambridge	1·3

The foregoing table cannot be carefully inspected without perceiving that the mortality of England, in the aggregate, approaches unfavourably to the greatest mortality attributed to any of the counties,—an appearance which would invalidate the average ascribed to England, unless it be considered that the omission or withdrawal of the metropolis and many other large towns (as previously stated) cannot but produce an effect of this kind; so that if the mortality of the metropolis and of such towns had been stated, this seeming inconsistency of the table would disappear; but I have avoided this kind of justification, from a natural repugnance to produce local anxiety or discontent, which would do more harm than good. I will only say, that the inhabitants of the metropolis may rest assured that they have no reason to be dissatisfied with the degree of longevity (that is, of health) which they enjoy.

It has already been been shown (page 585 of your last volume) that in the several grades of age, the expectation of female life is preferable to that of male life; yet on dividing the mass into its component parts (England into its counties) the advantage in five instances rests with the male sex, and in as many others is nearly balanced. The time of life at which female mortality greatly predominates in four of the five unfavourable counties, is from 10 years of age to 20; and this so remarkably, that perhaps you will admit a tabular illustration, such as may enable your readers to contrast these four counties with England in the aggregate, and with certain other counties.

Counties.	Age 10 — 14.		Age 15 — 19.	
	MALES.	FEMALES.	MALES.	FEMALES.
—	One dies annually in	One dies annually in	One dies annually in	One dies annually in
Bedford	195	147	157	97
Bucks	241	155	160	106
Northampton	226	142	168	103
Rutland	265	188	199	132
—	—	—	—	—
Average of Four Counties	232	158	171	109
ENGLAND	211	201	147	138
Lancashire	195	210	135	134
West York	208	213	148	142

Thus the mortality of females in the two grades III. and IV., from ten to twenty years of age, is as 100 to 66 (three to two), on the average of the four first counties; as 100 to 95 (five per cent.) in all England; as 100 to 104 in Lancashire, where the scale is thus turned in favour of the females to the extent of four per cent.; in West Yorkshire the mortality of the sexes in these grades of age appears to be equal.

I never yet could discover any fact which was likely to place the health of the manufacturing population below that of other occupations; nor have I ever met with any alleged fact to that effect which stood the test of strict examination; so that in the conflict of opinion I was bound to adhere to equality of health in the grades of female life [10-14 and 15-19] which chiefly constitute our manufacturing population. Moreover, I was the less prepared to discover disadvantage to young females in the counties of Bedford and Bucks than elsewhere, because in my youth I had traversed those counties oftener than once in pedestrian excursions, and was then much struck by the happy appearance of young girls and other females sitting at cottage-doors or with open windows, busied in lace-making; especially as constant shelter from bad weather had preserved their beauty, so as to equal that of highly-educated females. Since that time lace-making

has been seized by the inventors of machinery; but straw-platting for female hats has taken its place in those counties, where the tuition of young children in reading and writing, and straw-platting, is often mixed together at evening schools. I was greatly surprised, therefore, at discovering the stubborn fact of disproportionate mortality in the females of these and the two other counties; the more so, as Hertfordshire, Oxfordshire, and Berkshire, adjoining counties which partake of sedentary domestic occupation, exhibit similar mortality in a less degree *.

It is impossible to investigate retrospectively, whether in earlier times, in the days of Queen Elizabeth for instance, the sedentary occupation of the *spinster* (which included all unmarried females, and is still their legal designation) had the same deleterious effect as in the four selected counties: if so, females are positively benefited, not injured by the introduction of machinery, as well-meaning philanthropists too readily suppose. For I cannot imagine or believe that regular hours of labour, plenty of fuel, good clothing, and the many other comforts which spring from high wages, are injurious to the health of any human being. We all know but too well, from the incessant clamour of hand-loom weavers, that there are many industrious men, who during a series of years have carried on a domestic manufacture, in small rooms crowded by looms and weaving apparatus, breathing air loaded with dust, their hours of labour extending into the night, payment for such weaving very moderate,—preferring all these inconveniences to factory labour, because they cannot endure stated hours, and the regular behaviour indispensable in every factory: nor do they send their children thither, because they are retained at home to prepare hand-loom work. When power-looms were first invented, the consequent hardship on existing hand-loom weavers justly created general commiseration; but plenty of time has now intervened for equalizing the reward of similar labour. Hand-loom weavers, however, are not yet convinced that in preferring liberty to subordination they secure a reward which cannot be expressed in money value, and that they cannot reasonably expect independence, and at the same time money payment equal to that of regular workmen in a factory.

The fifth county in which female mortality exceeds that of males is Westmorland, of which I shall only say, that the difference is nothing between 10 and 15, and not striking from 15 to 20 years of age. The four other counties, Bedford, Bucks, Northampton, and Rutland, adjoin each other from south to north, and afford a mass of 300,000 or 400,000 human beings producing a similar and remarkable result in those two grades of life. Those who possess the Population volumes of 1831 may readily satisfy themselves of the general truth of the fact, by adding the male and female deaths (10 to 20 years of age) at the end of those counties: for exact calculation the corresponding enumeration of ages in 1821 must also be taken into account.

A question will naturally arise in the minds of those who are unaccustomed to considerations of the expectancy of human life, Why is the annual mortality of each county withheld; some of the component parts thereof having been adduced? To this I must answer, that the error produced by the increase of population in each component portion or grade of life, is not unmanageable by such approximation as is fairly allowable, correction and regulation being always at hand, in the annual mortality of the next grade; and this, assisted by the actual number of recorded deaths in each *year* of life, is so effectual in calculating the expectancy of life, that no valid objection can be urged against the result obtained. But the proportion of annual mortality as compared to the existing population, is governed much more by the increase of population than by air or climate, or any other consideration. Such a calculation, however, must take into account the average duration of life in each county (*Vie Moyenne*), which in all England is about 33 years, and would be 42 years, (40·5 the males, 43·7 the females,) had no increase of

* I learn from a book recently published (Southey's Life of Cowper) that the unhealthy effect of lace-making in the northern part of Buckinghamshire (and consequently in Northamptonshire) had been observed by others long since. "Cowper has made Olney and its neighbourhood poetical ground. The town, which is the most northerly in Buckinghamshire, consisted of one long street. Lace-making was the business of the place, a sedentary and unwholesome employment." And in a note it is said, "Mr. Lysons observes, in his Magna Britannia, that persons travelling through the counties where this manufacture prevails, have been struck with the sickly appearance of the women and children employed in it."

population occurred during the life of man retrospectively; in which case the annual rate of mortality, the *vie moyenne*, and the expectation of life at birth, would all have coincided.

I endeavoured to illustrate the effect of the increase of population on the *vie moyenne* at the end of the Popul. Pref. p. liv.; but in an official document circumstantial explanation was not allowable; and I may perhaps hereafter endeavour to elucidate in detail this seemingly novel principle, on which the comparative mortality of nations, as well as of counties, mainly depends.—I remain, sir,

Your obedient servant,

JOHN RICKMAN.

Dec. 14, 1835.

P.S.—I stated with due diffidence, page 587 of your last volume, certain facts which seemed to indicate a remarkable degree of mortality at the age of 84. It has since been observed to me, that the printed formula distributed for insertion of the ages of the deceased, ceases to be continuous at 84 (see page xxvi. of the Population Preface), which may have been the mechanical cause of this seeming intensity of mortality. The question will hereafter be solved by altering the printed formula for the next decennial Return of Parish Registers.

ON THE

SOURCES OF THE VITAL POWERS,

AND THE

RELATIONS THEY BEAR TO EACH OTHER.

By DR. PHILIP*.

To the Editor of the Medical Gazette.

SIR,

IN a former paper I pointed out what appears, from direct experiments, to be the nature of the powers of the more perfect animals. In the present paper it is my intention to consider the sources of these powers, and the relations they bear to each other. These two papers will comprehend a summary of all the general deductions from my experiments; and may, in future, prevent opinions being ascribed to me which I not only never entertained, but took much pains to refute.

THE sensorial organs, as appears from what has already been said, are confined to the central parts of the nervous system; the functions of which, having nothing in common with the properties of inanimate nature, cannot directly cooperate with it; but by the intervention of three sets of organs they are enabled to do so indirectly.

The nerves of sensation and the nerves of voluntary motion, both of which, being vital organs, are, on the one hand, enabled to co-operate, the one directly, the other through organs belonging to the central parts of the nervous system properly so called, with the immediate organs of the sensorial powers; and on the other hand, with the external world, in consequence of the relation they bear to that world; the one being capable of receiving and propagating to the immediate organs of the sensorial powers the impressions made by agents belonging to inanimate nature; and the other, through the medium of the central parts of the nervous system properly so called, enabling the organs of the sensorial powers to impress those agents.

Distinct from all the foregoing functions are those by which their organs are maintained, thence termed the vital functions; the immediate agents in which are the ganglionic nerves (each conveying the influence of all the central parts of the nervous system properly so called, which are distributed throughout every part of the brain and spinal marrow, the organs of the sensorial powers being confined to certain parts of these organs), and the blood, equally with the nerves themselves, endowed with vital properties. The former derive their power from the central parts of the brain and spinal marrow, with which they are associated, namely, the only active parts of the nervous system pro-

* Mr. J. W. Earle has been so polite as to send me a copy of a letter which he had addressed to the Editor of the Medical Gazette, for insertion in the next number of that journal. I am at a loss to understand how Mr. Earle has fallen into the mistakes he has committed. The paragraph, part of which he quotes from my last paper, was sufficient to have prevented them. When I first made the experiment, the apparatus for applying the voltaic electricity was arranged after the nervous influence was withdrawn. Even the time required for this I found slightly impaired the structure of the lungs. When the apparatus was previously arranged, and the electricity applied as soon as the nervous influence was withdrawn, their structure was found as entire as in the healthy animal, and they continue perfectly healthy till the inflammation comes on, which requires many hours. What does Mr. J. W. Earle mean by his comment on the quotation above referred to. Can he be so little acquainted with the subject as to suppose that all degrees of impaired structure of the lungs implies the death of the animal? With regard to the cause of death when voltaic electricity is employed, I have both in my last paper and elsewhere stated, that it is the consequence of inflammation of the lungs excited by the electricity, which proves fatal more quickly than the partial abstraction of the nervous influence.

perly so called; the latter from its own composition; that composition, like the structure of the solid organs of our bodies, depending, more or less immediately, on all the general powers of our frame,—its own powers, the sensorial, the nervous, and muscular powers, and the relations these powers bear to each other.

The vital functions, properly so called, are all, we have seen, of a chemical nature; their immediate agent is supplied by the central parts of the nervous system properly so called, the same which operates in all other chemical changes, its effects being modified by the properties of life.

Thus there are, in the more perfect animals, two systems, in a great degree distinct from each other, to the maintenance of both of which the sensorial, nervous, and muscular powers, and the powers of the living blood, although in the two systems employed in very different ways, are equally essential; namely, the sensitive system, by which we are rendered capable of enjoyment, and on which our intercourse with the external world depends; and the vital system, by which the due structure of the organs of all our functions, whether sensorial, nervous, or muscular, and the due composition of the living blood, are maintained.

The only connecting link between the functions of these systems is the function of respiration, because it is the only vital function to which the sensorial powers are necessary; respiration ceasing, provided the due supply of air be not withdrawn, only when sensation ceases; or the effect of which, in producing, through the function of volition, the necessary expansion of the chest, is by some cause prevented. Thus it is that the failure of respiration is, *in all instances*, the immediate cause of death: for the sensorial functions are *always* the first which wholly fail; although, in natural death, that is, the death of old age, and in most other forms of death, the failure begins in the nervous and muscular functions. I speak of what, in common language, is called death, which is the consequence of the removal of the sensorial powers alone; for the vital powers, and all their functions, with the exception of respiration in consequence of its partaking of the sensorial powers, it has been found by direct experiment, survive the removal of those powers.

The cause of the vital powers being the first which begin to fail is, that their organs are immediately exposed to the operation of the inanimate agents on which all the phenomena of life more or less directly depend; and which, by their continued operation, tend constantly to exhaust the excitability of the organs to which they are directly applied. And the cause of their being the last which wholly cease is, that a certain vigour of the vital being necessary for the maintenance of the sensitive powers, the latter necessarily cease before the total extinction of those which maintain them; which are still maintained by their usual stimulants, the removal of the sensorial powers not immediately preventing their continued application.

The sensorial powers are subject to imperfect and temporary exhaustion, the effect of the various excitements of the day, which constitutes sleep; of which state the vital powers in no degree partake, although they, to a certain extent, feel its effects. The latter are subject to complete and permanent exhaustion, the effect of the stimulants of life, and the cause of the death of old age.

The death of old age is a rare occurrence; for in old age we become so liable to disease, that extremely few die, alone, in consequence of the continued operation of the functions of health.

The last of the chain of the remote causes of the death of old age is sleep; because, during sleep, the frequency of respiration being, in consequence of the impaired sensibility, lessened, and all the vital functions, on this account, less powerfully excited than in our waking hours, respiration is most inclined to fail, the excitement to respire being proportioned to the degree of sensibility and the excitement of the vital functions.

It appears, from all I have had occasion to say, that in the maintenance of all the organs, both of the sensitive and vital system, there are four distinct species of powers employed, the sensorial, the nervous, and the muscular powers, and the powers of the living blood. These powers, it appears, from a great variety of experiments, have no direct dependence on each other; yet each an indirect dependence on the other three, the healthy structure or composition of the organs of all depending immediately on the nervous and muscular powers, and the powers of the living blood, and more remotely on the sensorial powers.

The muscular power remains for a certain time after the removal of the sensorial and nervous powers, and the powers of the living blood; the vital powers of which, in part at least, also remain after it is deprived of the influence of all the other powers, because they are found to exist for a short time after the blood is removed from the body. The sensorial and nervous powers survive each other; but neither can survive, except momentarily, the loss of the blood, or of the muscular power by which its supply is effected*. We can have no doubt, I think, from the facts stated in a paper published in the Phil. Trans. for 1831, and republished in my Treatise on the Nature of Sleep and Death, that the active power of the vessels, like that of the heart, is a muscular power.

The muscular power not only survives the sensorial and nervous powers, and the powers of the living body, but is capable of its functions after they are withdrawn, as appears from the effects of artificial stimulants†.

All the functions of the nervous remain after the removal of the sensorial power, with the exception of those in which the latter power co-operates with it.

The sensorial functions also remain after the removal of the nervous power, for the short time that life can continue after the loss of this power, with the exception of those in which these powers co-operate.

It appears from the whole of the foregoing facts, that the source of all the vital powers is in the mechanism of the organs to which they belong; they are therefore inseparable from those organs.

While all the powers of the more perfect animals are entire, the muscular, notwithstanding its independence of the nervous power, is immediately subjected to its influence, even to the extent of its total extinction when the cause impressing the former is both powerful and sudden. And the same, it would appear, from the circumstance of the blood not coagulating when an animal is suddenly killed, by a cause capable of powerfully affecting the nervous system, is true of the vital powers of that fluid. And the nervous, notwithstanding its independence of the sensorial power, is in like manner, and to the same extent, subjected to this power; which, through the medium of the nervous power, has also the same influence over the muscular power: and we have reason to believe, from the general laws of the animal economy, (although, as far as I know, the fact has not been ascertained either by direct experiment or observation,) also over the vital powers of the blood.

Thus the powers on which the maintenance of the sensorial, as well as all the other organs of the living animal depends, are subjected to the sensorial power, on which depends the great influence of mental affections both on the functions of health and the progress of disease.

In the foregoing statements nothing was left to conjecture. In my Inquiry into the Laws of the vital Functions, and papers published in the Philosophical Transactions, the proofs of each, resting either on undisputed experiments or observations open to all, will be found.

I am, sir,
Your obedient servant,
A. P. W. Philip.

Cavendish-Square, Dec. 14, 1835.

* I speak here only of the manifestation of the sensorial powers in the animal body, disclaiming all intention of entering on any question respecting either the nature of these powers, farther than relates to their influence on the functions of the animal frame, or their mode of connexion with that frame; questions the discussion of which their nature, independently of the ample experience we have had, might have told us, is neither consistent with the sources of our knowledge or the nature of our minds.

† The heart and vessels are excited by inanimate agents, the stimulating ingredients of the blood. We know that it is the stimulant ingredients, not the vital properties of this fluid, which it possesses for other purposes which I have elsewhere had occasion to point out, which excite the heart and vessels; because, when these ingredients are separated from the blood they have the same effect on the muscular fibre.

QUESTION WHETHER VITALITY IS EITHER IMPARTED OR SUSTAINED BY GALVANISM.

To the Editor of the Medical Gazette.

Sir,

I take advantage of the opportunity afforded me by the present discussion between Drs. Philip and Williams respecting the identity of voltaic electricity and nervous influence, to propose a

question to the former of these gentlemen, arising out of a circumstance mentioned in the experiments which are most remarkable among those which he adduces in support of his opinions on this subject. The circumstance here alluded to is the fact, that in all the comparative experiments (that is, in all those in which two animals were taken, the nerves being divided in each, but one only subjected to the action of galvanism, the other being left undisturbed, for the purpose of comparing the appearances in each together), death always took place several hours sooner in those animals which were treated with galvanism than in those which were not subjected to the influence of this agent. It seems fair to conclude from this fact, that if voltaic electricity and nervous influence be identical, the application of galvanism ought to restore that which is lost by the division of nerves, and, consequently, that the galvanised animal, instead of dying several hours sooner, ought to live considerably longer than one which has not this advantage.

How, then (and this is the question I would propose for Dr. Philip's consideration), is this fact compatible with the conclusion respecting the identity of these two powers? It is the more necessary that our attention should be directed to and fixed upon this circumstance, and the question connected with it, until a satisfactory explanation shall have been rendered; since not only does it appear to have been forgotten by Dr. Philip himself, but in his last communication he assumes directly the contrary to be the fact.

He says (page 397), "when electricity is substituted for nervous influence, as we cannot apply it in the way in which it is applied by nature, it at length (that is, *long after their structure would have been impaired, had electricity not been employed*) excites inflammation, and the animal dies of inflammation of the lungs," &c.

In this passage Dr. Philip assumes it as a fact, that in a comparative experiment, the galvanised animal lives much longer than one not so acted upon; and if electricity and nervous influence be identical, such ought unquestionably to be the case; because it would be immaterial, except so far as it would be impossible to make amends completely by art for the loss of the natural supply, by which of these two powers life was maintained. It is the very essence of a mistake that the person committing it does it unknowingly, and consequently it is necessary that it should be pointed out to him. I beg, therefore, to refer Dr. Philip to his own experiments, particularly to Experiments 77 and 75. In the former of these the "galvanised dog lived only *two hours and a quarter*, while the other was alive at the end of *four hours* after the nerves had been divided, and *then was killed*." In the latter, "when the *galvanised rabbit died*, the other rabbit, which had been *left undisturbed*, *was killed*." In these experiments, which agree with those of other inquirers, we thus find the fact, as actually stated by Dr. Philip himself, to be directly the reverse of that which he has assumed in the preceding passage, which I have extracted from his last communication to the Gazette.

The circumstance to which I have here alluded—viz. the speedy death of animals which are galvanised after the division of the eighth pair of nerves, has certainly not yet been explained, nor even alluded to as a matter of moment, in any part of Dr. Philip's various writings, nor, so far as I know at least, has the question arising from it ever been raised by any one else. In the forthcoming statement of his opinions, however, which he has promised to the Gazette, Dr. Philip will have an opportunity of shewing, if it be possible, how this circumstance can be reconciled with his conclusion respecting the identity of galvanism with nervous influence. Any farther observations, therefore, at present, would be superfluous.

I am, sir,
Your obedient servant,
J. W. Earle.

64, Welbeck-Street,
Dec. 14, 1835.

OBSERVATIONS

ON

ARTERIAL POWER IN HEALTH AND INFLAMMATION.

To the Editor of the Medical Gazette.

Sir,

I should feel obliged by your giving the enclosed paper insertion in the

Medical Gazette, if it meet your approbation.—I am, sir,

Your obedient servant,

J. Slade, M.D.

Barnstaple,
Nov. 26, 1835.

The opinion current among physiologists, that irritability and contractility are properties of muscle *only*, is not, I think, one of the best proofs of their having given the subject the consideration it deserves. To fibrin, that supposed essential ingredient of muscle, I, in opposition to many, attach but little importance, believing it to have no more connexion with these properties than have other component parts. The true source of irritability and contractility is life, which is quite different from the structures of our bodies, or any qualities that may be supposed to belong to any part of the material kingdom; and whether an artery has or has not such constituent parts as are common to the muscle, there are certainly properties which enable it to contract, to be elastic, irritable, and sensitive; but the impulse given to an artery by the action of the blood is almost or quite sufficient to give it motion, even if there were no vitality, or if it were nothing more than an inert yielding tube. The arteries have many coats or tunics, and no particular one or two, which may be supposed to be muscular, can reasonably be considered to assist the motion of the blood independently of the others. That one coat may be better constructed for this purpose than another, and one be more elastic than another, is possible; but to the division of the coats into irritable, muscular, and elastic, some degree of vagueness applies. The whole tube is elastic, contractile, sensible, irritable, and capable of adapting itself to existing circumstances, the contractile disposition being comparatively greater in the smaller branches. That arteries are muscular, is a subject upon which much difference of opinion exists; but that they obey laws, possess nerves, and are influenced by similar stimuli to the muscle, is too evident to admit of dispute; yet the instance of their being acted upon by stimuli is a weak argument in favour of their muscularity; and nothing more fully exemplifies the truth of these nerves being unnecessary to the existence of such properties, than that, when all communication is cut off between them (the nerves) and the arteries and muscle, uninterrupted action is still continued; at the same time it has been shown by experiment that on stimulating the nerve, or even the spinal marrow, in a recently-dead subject, the muscle or artery which is supplied with branches from either of these sources is thrown into motion; this, however, is no evidence that the capability of an organ to act in a living subject is not derived from the principle of vitality. Haller may talk of his vis insita, but he could never prove the existence of an inherent power of muscle, or of any other animal organ, capable of carrying on the functions independently of a pervading principle different in nature from them; nor is the fact of brainless beings a proof of any such inherent power, for in these beings is life. It must be acknowledged that the brain can neither vitalize nor think; and it would be as absurd to talk of any function accruing from that organ as possessing an *inherent* power capable of producing such function, as from any other material body. "The motion of the blood," says Dr. W. Philip, "is carried on independently of the nervous system;" and that it is so of the will is more evident. Involuntary acts result more particularly from life, and voluntary acts from the mind, which wills to action: the former has been said to take place independently of the nervous system, the latter to be the entire result of that system. That there is greater irritability belonging to the organ which acts involuntarily, that it is likewise more readily influenced by stimuli, and that some unusual cause must at all times be in operation to excite to an unnatural degree the organs of involuntary motion, cannot be doubted; all of which must be ascribed more or less to the strong connexion subsisting between these organs and the nerves, which nerves appear to be peculiarly designed to conduct or possess some certain mental and vital qualities. Hence we would infer, that although the natural motions of an artery are independent of the will, they are liable to be influenced by the nerves. How the influence is communicated—how joy, anger, grief, or any inordinate passion of the mind, should expedite the motion and impair the functions of any organ which, in health, is not subject to voluntary power, or requires not the interference of the will, it

is impossible to describe; and in reference to what I have said respecting involuntary acts being more particularly from life, and voluntary acts from mind, many arguments may be adduced to prove that the general physiological principles on this point are liable to some objections.

Galen, J. Hunter, Philip, Hales, Hastings, Thompson, Bell, and Munro, have written much in favour of the muscularity of arteries; while, on the other hand, White, Portal, Richerand, Scarpa, Soëmmering, Bichat, Macfayden, Parry, Magendie, Johnson, and Bostock, have declared the reverse to be the case, many of whom describe the pulse to result entirely from the impulse given to the artery by the current of blood, and others deny the existence of a contractile power. Dr. Johnson considers an artery to be a contractile body, and capable of acting on the blood without muscularity; and the definition Mr. J. Hunter gives of elasticity is, that "it is a continual action, and its immediate effects are produced whenever the resistance (contractility) is removed, by which it may be distinguished from other powers." The elastic power is always at rest in the event of the contractile power being in exercise, and the artery being collapsed; but death may be said to destroy the tonicity of an artery without affecting its elasticity, which would imply that the contractile power is derived immediately from life. In the time of dilatation, or the elasticity being in exercise, there is a supply of blood that appears to stimulate the artery to offer resistance, and contraction upon the contents is a necessary result; which task being completed, the artery seems at rest, or suffers momentary relaxation, or a suspension of contractile power; and this being directly followed by a fresh supply of blood, the calibre increases, the artery becomes more fully injected, and contraction again takes place.

The powers which belong to the arteries are unquestionably given to assist the passage of the blood; and considering the comparatively small force with which that fluid is thrown from the heart at every systole into the aorta, and also the circuitous route of the arterial system, it is impossible to be otherwise. Borelli estimated the power of the heart in propelling the blood at one hundred and eighty thousand pounds weight; but Dr. Black, an able experimentalist, has said, with much accuracy, that this force is equal only to three pounds on the square inch; and it is supposed that double this force, or six pounds on the square inch, would be required to produce a continued stream of water through a tube one-hundredth part of an inch in diameter; which capacity hardly exists in arteries, and therefore, as Dr. Black observes, "the comparative calibre of the small arteries or capillaries must be ten times short of that extent; and the viscidity or comparative glutinous state of the blood requires four times the power water would to pass through a tube of the same calibre and extent." According to Dr. Parry, arteries undergo neither dilatation, nor contraction, nor locomotion of any sort, during the systole and diastole of the heart; but his argument against the muscularity of arteries is no support to the premises from which he draws this conclusion. I agree with him, however, that a pulsation is not indicative of an artery being composed of any constituent qualities of muscle.

That an artery pulsates, that is, alternately dilates and contracts, is not only proved by the finger, but by the eye; and therefore Dr. Parry and Magendie are incorrect in affirming that the pulse is distinguished only when pressure is made, the act of which diminishes the calibre of the artery, and tends to retard the course of the blood. The arteries always contain this fluid, and receive an additional supply at every contraction of the heart; but we have no direct evidence to prove that the pulsations of the heart and those of the extreme arteries are *synchronous*. It may not be possible to detect any difference, so rapid is the circulation, and so continued is the impulse; but upon reflection, it will be found impossible for the contraction of an extreme artery to take place at the same instant as the contraction of the heart, unless the former results from a previous contraction of the latter, the same blood not causing both heart and artery to beat at the same period. Every contraction of the heart is succeeded by dilatation of an artery, and by the time, or perhaps at the instant this dilatation occurs, the heart has acquired another supply, and becomes itself dilated. Here are *synchronous* beats, or, at least, a concurrence

of events; yet this is not what physiologists in general mean by the true synchronous. There is a continued systole and diastole of the whole arterial system, the circulation decreasing somewhat in force, if not in velocity, as it reaches the most remote vessels, and capable of exciting them to act without nerves; and the circumstance of their being independent of the nervous system, may arise, in some respects, from this circumstance. It is further evident that no impulse can be given by the force of the heart's action to an artery which, after being tied, receives blood only through anastomosing vessels, although it is evident they receive a larger share of influence in this instance; and therefore other means than what may be ascribed to the heart exclusively are necessary to carry on the circulation.

Ossification of arteries is adduced as a proof against their muscularity, in consequence of its not causing any particular impediment to the circulation; but this is scarcely worth observation, when we know that the same disease happens to the heart itself, without producing very great derangement in the sanguiferous system. The arteries may be partially deprived of their contractile power by ossification; but nature in such a case accommodates herself to the change, and gives additional impelling force to the heart; and, on the contrary, if this organ be the part affected, an additional power is given to the arteries. Nature is always ready to make up for a deficiency; and yet if the disease existed to a very great extent, either in the heart or in the arteries, it would cause fatal symptoms. Whenever the heart is the seat of such a disease, the pulse at the wrist is generally found to be irregular, cord-like, and weak; there are also strong palpitations, cold perspirations, occasional difficulty of breathing, besides other symptoms of general disorder of the system.

Mr. J. Hunter has said that the heart cannot be an essential organ, as many animals which have an uninterrupted circulation are destitute of it. I conceive that comparative anatomy is very apt to cause erroneous conclusions; and although it may not be the nature of some animals to require a heart in the sanguiferous system, it is evident that without this organ no creature in the higher scale of existence could exist. Preternatural pulsation in arteries is, as Laennec affirms, the most certain test that the arteries have an action of their own, independent of the heart; and Dr. Johnson says, "when any one will show us a pulsation in an artery where there is no corresponding ventricular contraction, then, and not till then, will we believe that the arteries can pulsate independently of any impulse from the heart." To reconcile these opinions is difficult; but it is most likely that preternatural pulsation never existed but with some disease of the arteries. That arteries have a contractile power is further proved by their being entirely emptied of blood after death. It has, however, been considered absurd to suppose this an effect of contractility, when the right ventricle and pulmonary arteries, which have still greater contractile power, are entirely full. Mr. Hunter has shown that the smaller arteries contract more on this occasion than the larger; and although it has been demonstrated by experiment that the lungs have no power over the arteries, some persons still persist in ascribing this vacuum to the lungs acting as a suction-pump to the arterial blood.

It is the opinion of most pathologists, that, in cases of inflammation, there is always increased action; that is, greater velocity and impetus in the circulation, and greater rapidity in the beats of an artery, than are necessary to the health of the part affected; but, after taking into consideration the nature of the arterial system, including the capillary vessels, and the state of the circulation, both in health and in inflammation, it will be found that such an inference is very objectionable.

To the capillaries belong two particular powers or properties—one of a contracting, the other of a dilating character; and the question is, what is the state of those powers in the time of inflammation? That an equilibrium can be kept up between them, in disease as well as in health, is certain, and there is no doubt that the former may be greatly weakened; but whether we are, in this case, to expect an increased accumulation, weight, or momentum of blood, is a point upon which many differ. But in commenting upon this

subject, it is necessary to be aware that an accumulation may exist without the vessels being increased in action at the same time, and that whenever the blood is increased in velocity, so as to excite inflammation, the vessels which previously contained colourless, become distended with red blood. It is evident, however, that all augmented supplies of blood must first be furnished by the heart; the small arteries cannot derive it from any other source; and it is very doubtful if, during inflammation, they have any power to attract any quantity from those vessels which do not partake of the disease, and which, in health, contain more than themselves. Increase of tonicity, or contractile power, would indicate increase of action in the circulation, and any loss of that power must necessarily be followed by a comparatively greater tendency to dilatation, and consequently a weakened or passive condition of the artery; in which state, in contradistinction to the former, we look for a larger proportion of blood being collected in the disordered vessels. When an equilibrium, therefore, is not preserved between these two powers, disorder or disease must inevitably ensue; and it is from these opposite states, for they cause an essential difference in the circulation, that we may derive much information, and be able to form a more favourable diagnosis of the nature of inflammation.

Increased velocity and increased momentum have very different significations; the former exists when there is quicker motion in the blood, and the latter is seldom found but when the contractility of the vessels is lessened, and the calibre increased. If there be by any means a greater portion of blood sent to a part than is natural, and is still pressed on without interruption, we conclude that the powers of the vessels are augmented in proportion to effect that object.

Here is no loss either of tonicity or elasticity; in the meantime it is very doubtful whether the heart can propel an unusual quantity of its fluid to one part more than to another.

Dr. Parry observes, that "the phenomena of inflammation are always increased momentum, or a diminished velocity with increased quantity; an increased velocity with increased quantity; an increased velocity with the same quantity; and lastly, an equal velocity with increased quantity." But he considers the proximate cause of inflammation in general to be an increased momentum of blood; and, as regards the power of the heart, he believes it capable of carrying on the circulation independently of any assistance from the arteries; which would lead us to infer that no such powers as those mentioned belong to the arterial system.

The cause of any increased action in the minute arteries may be either in the vessels themselves or in the heart. Conceiving the latter to be affected, as in fever, there is an acceleration of the blood's motion, which, if unattended by a proportionable active state of the vessels, is likely to produce either inflammation or congestion. In the time of fever, we have no evidence to prove (although the diastole and systole of the heart are more frequent) that the blood is invariably propelled with greater force; and that there should be a quantity sent from the heart at each contraction equal to what there would be in health, is very doubtful. As the velocity is increased, the momentum is, I conceive, generally or always diminished, and *vice versâ*. Dr. Cullen says, "the phenomena of inflammation all prove that there is an increased impetus of the blood in the part affected; and as, at the same time, the action of the heart is not always evidently increased, there is reason to presume that the increased impetus of the blood in the particular part is owing essentially to the increased action of the vessels of that part itself." We here find power given to the vessels; and it would be unreasonable to suppose that tubes so distant from the heart, so tortuously and intricately distributed, were merely passive and inert; and whenever the action of a vessel becomes increased, we reasonably calculate upon an increased velocity in the motion of the blood: one cannot exist without the other; but in this case there is no more than an ordinary supply of blood, and perhaps not so much as usual, sent to the part, and the calibre of the vessels, owing to their action being increased, is generally less than in health.

It has been proved by experiment, that when the blood circulates with unusual velocity (a circumstance easily known by the frequency of the pulse),

the vessels become paler and smaller; but, with an increased momentum of blood, they would be more charged, and consequently redder and larger; which implies that inflammation does not depend on an increased velocity, but on an increased quantity, and a diminished action in the vessels. The application of stimuli to an artery produces contraction, which may or may not be followed by exhaustion, inflammation, and a loss of tonicity. A preternatural contraction of certain arteries, be it occasioned by what it may, is a very common cause of inflammation; yet it does not follow that when this disease is actually developed — when we discover redness, swelling, pain, and extreme sensibility— that the vessels are in a preternaturally contracted state. It is further to be supposed that no contraction of arteries, whatever the cause may be, is of a spasmodic nature; a collapsed or impervious state of them, however, seldom continues any great length of time; the proper functions of nutrition being disordered, the textures lose their tone and power of resistance, and become exposed to the impulse of the blood behind. Some suppose an increased action is established in the vessels leading to the diseased parts, to relieve the morbid condition of that part; and supposing them to be so affected, it cannot be absurd to consider that some undue influence is communicated to them by means of the nervous intercourse subsisting between them and the disease; and I very much doubt if any such action is the effect of any primary design of nature to restore the part affected to health. When constitutional irritation ensues, the heart sympathises, becomes more frequent in its action, and the blood is sent with greater force through the whole system; and, conceiving that the blood could not be distributed through the body independently of any power of, or assistance from, the arteries, it is to be concluded that any effort to restore a diseased part, remote from the heart, must be attributed, in a great measure, to the small arteries.

The sanguiferous vessels are so constructed as to be able to accommodate themselves either to a large or a small proportion of blood. In the former instance, their diameter being increased, may terminate, if too long continued, in their debility; in the latter case, there is a comparatively small calibre, which, whether owing to a deficiency of blood, or any other cause, is no proof whatever of an actual loss of elasticity, or at least capability in the artery to dilate. The circulation in this state is usually more rapid, as is discovered by the pulse, which in such a case we never find full. In an unusually dilated vessel, a greater quantity of blood, and generally a loss of tonicity, are found; and this is the invariable condition of all passive, if not of acute inflammations; yet the dilatation may exist without any such disease being present, as may also an increased velocity. The throbbing experienced about an inflamed part, and so evident in that circumscribed tumor called whitloe, is a mere shock communicated to the nerves by the blood which is propelled against the obstructed vessels,—a compressed state of the nerves by effusion of serum causing greater pain than would otherwise occur. I do not conceive that the vessels preceding those that are inflamed need in consequence suffer from increased action, although it may be the case when the heart and arteries become affected. Nor is the pulsation any proof of increase in the momentum and velocity of blood in the diseased vessels. Added to this, the sensation is confined more particularly to that side of the inflammation nearest the heart, and the smaller the vessels, and the fewer the nerves, the less perceptible is this sensation, which is at all times synchronous with the beats of the heart, becoming more severe upon those beats being increased in force and frequency, which leads us to infer that it does not depend exclusively upon a preternatural action in the arteries behind. The sensation is also greatly influenced by the effort made to overcome the resistance offered by the congested vessels, and may hence be supposed to arise, in some measure, from the obstruction given to the due course of blood through the inflamed part, from a preternatural sensibility of the nerves of the inflamed part, and from an increased action of the heart and of the arteries. When depending upon the heart and arteries, the pulsation is evident to the bystander on application of the finger; when not, it is evident only to the afflicted person.

OBSERVATIONS

ON THE

CURE OF WOUNDS WITHOUT INFLAMMATION *.

By J. Macartney, M.D. F.R.S.

Professor of Anatomy and Surgery in the University of Dublin.

For above thirty years I have been in the habit of teaching that inflammation, so far from being necessary for the generation of tissues, retards that process when moderate, and prevents it when existing to a high degree.

This opinion I formed as the result of numerous observations and experiments, made on fishes and other of the lower animals, in which I found that even the most considerable solution of continuity was promptly remedied without the presence of any symptoms of inflammation, and that such symptoms never shewed themselves except where there were parts removed to be replaced.

My attention having been thus awakened, I was for some time in search of the best means of diminishing, or preventing, the development of inflammation; and the conclusion I arrived at was, that if the parts susceptible of being inflamed could be put in a condition of feeling only agreeable sensations, inflammation would not ensue, or, if present, would be soon dissipated.

It may be presumed that human beings, and animals of the higher orders, have the power of reproducing their tissues in a high degree—in fact, in almost as high a degree as the lower animals —if the parts be only affected with agreeable sensations. Daily experience tends to prove this. Do not emollient fomentations produce their good effects by soothing (*flattant*) the part affected? And do we not see the comfortable feeling experienced by the patient when his wounds are gently dressed and cleansed, and all bandages removed which hurt him by their pressure? Yet these views are by no means generally adopted: there are even practitioners who, far from agreeing in them, constantly employ local irritants, without reflecting that by these means they oppose the salutary operation of nature, the sole agent of every cure.

The most suitable mode of putting a wound in a state proper for cicatrization, is by enjoining repose, abstaining carefully from all sorts of irritation, dispensing with every kind of compress or bandage, unless there be absolute necessity for it: finally, to place it in such a position as that the blood and other humors, obeying the laws of gravity, may not rest on the parts affected.

There is no topical application better suited to our purpose than pure water, either in its liquid state or in the form of steam. We never employ any other. As to the temperature, it is to be regulated by the discretion of the surgeon, having regard to the nature of the injury and the sensibility of the parts. The steam I use is obtained by a very simple apparatus—a tin vessel opening funnel-wise, and thus at once diffusing the vapour and cooling it. A spirit-lamp sets it boiling; the steam is then received in a large tube of woollen stuff, kept in shape by a number of circles of bamboo. By regulating the lamp the temperature of the steam may be brought to any required degree.

We are thus enabled to apply the steam to the affected part. Another method is by laying on a soft pledget of lint, or cotton, previously steeped in water; and over this, in order to prevent suppuration, a piece of oiled silk. With this precaution the dressings will not have to be changed above three or four times in the twenty-four hours.

Much care and attention is requisite to adapt the temperature of the water to the purposes of nature. Sometimes it should be warm; at other times it ought to be cold. But, as a general rule, I should say that the most favourable temperature, or that best fitted for preventing inflammation, is the one which mitigates the pain the best. Thus, steam of a moderate degree of heat produces almost instant relief from pain, in the case of lacerated wounds, or where bones or ligaments are contused; but the more sensible tissues, which are more vascular, and more disposed of themselves to evolve heat, require a temperature absolutely cold.

By merely attending to the sensations of the parts injured, and abstaining from irritants, it is proved both by my own experience and that of surgeons

* Mémoires de l'Acad. Royale de Médecine, tome v. 1836. *Just published.*

who have followed the same course, that even the severest wounds are cured by repose alone. We also find that the *plastic force*, that is to say, the power of regenerating tissues, is far superior to what it is commonly supposed to be. I must be understood all along as supposing the constitution of the patient not tainted with congenital or acquired disease, such as scrofula, syphilis, or the like. With these exceptions, I have no hesitation in saying that in every instance we may cure, without pain, suppuration, or fever, penetrating wounds of the joints, wounds from fire-arms, with comminutive fracture of bones, and numerous other accidents equally severe.

Under the common mode of treatment the lips of a wound become tumid, diverge from each other, and are covered with granulations. When inflammation is prevented, on the other hand, they approach one another without any swelling; and so tenacious is nature of this approach, that it would require even a considerable mechanical force to prevent it. Thus the wound is soon obliterated; but re-union does not take place by any deposition of plastic lymph, as is the case in what is usually called union by the first intention. This formation of lymph is doubtless the effect of inflammation. In short, when no inflammation interferes, the margins of the wound gradually unite, and become combined; and what induces me to believe that there is no intermediate tissue is, that the cicatrix, under such circumstances, is much smaller than ordinary, and more uniformly of the colour of the surrounding parts: finally, also, it is more elastic and more natural.

It were superfluous to dwell upon all the consequences and advantages arising from a theory so simple, and a process so beneficial. The learned body to which I have the honour of addressing these remarks will readily supply what may be defective in them.

[The preceding paper, we presume, was presented to the Academy in acknowledgment of the honour, recently conferred on the author, of being elected a corresponding member.—ED GAZ.]

MEDICAL GAZETTE.

Saturday, December 19, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri: potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

LOSSES OF LIFE IN THE MINES.

THE extreme reluctance with which any measures, however essential, having for their object the protection of life, are adopted by our Government, must be allowed to be sufficiently extraordinary. If the fact were not repeatedly pressed upon our notice, we could scarcely believe it: foreigners cannot comprehend it; they see no provision made by our legislature for interfering, particularly in civil arrangements, where the life of the subject is manifestly threatened: when the mischief is done, indeed, investigation takes place, and probably a forfeit in God's name—a *deodand*—is imposed on the brute animal, the senseless machinery, or the goods and chattels of the party through whom the fatality has occurred. But even this is omitted, unless culpability, within the meaning of the statute, attaches to the party. If a hundred lives be lost at a time, in pursuing some reckless system of employing human labour, a coroner's jury may probably return a verdict of "accidental death," and there the matter rests till the next catastrophe of the like kind take place, when a similar inquiry, with a similar result, is enacted—nothing meantime having been done to avert the recurrence of the evil.

The late explosion at Wallsend colliery was of this nature; *one hundred and one* lives of men and boys were lost in a moment; and not many years had passed previously when above fifty individuals perished in the same manner: it was all set down to *accident*, and so

it will be again when the next destruction happens.

When noticing, not long since, the Wallsend calamity just mentioned, it struck us that there was much blame to be attached to those in power who took no thought of passing events of this kind, but permitted them to recur again and again with all their lamentable circumstances and consequences, nothing whatever being interposed in the shape of a preventive. Tardily was the formation of the Committee to inquire into accidents in mines pressed on our legislatorial authorities; and sorry are we to find that that body have terminated their labours without proposing any definite plan to obviate the mischiefs of which they obtained such ample cognizance. They recommend, indeed, that the public be fully apprized of the nature of the case; and they *hope* much from the diffusion of the information they have gathered: but as to pointing out any positive or decided step that ought to be taken, —nothing whatever has been done.

What appeared to us, in our late consideration of the matter, as most likely to be beneficial, was the appointment of a number of inspectors—a higher order of police—men of intelligence and activity, whose special duty it would be to prevent, to the utmost in their power, the occurrence of those accidents. We have inspectors and overseers in various other departments of the body politic: prisons, lunatic asylums, factories, &c. have their commissioners or visitors, authorised to see that no detriment take place in these establishments. Why not extend the system to the inspection of mines and collieries, those fruitful sources of fatal mischief? We think we can gather from the tone of the Committee that they deem a measure of the kind advisable, although they venture not explicitly to say so: they let "I dare not," wait upon "I would," and seem to set their chief hope, their "most sanguine expectation," on the progress of public opinion. We cannot approve of these half measures; they may be more practicable, no doubt, than those we advocate, but they are chiefly practicable because they propose to do but little.

Let us see what is the emergency that calls for a new system of relief, and then we shall be the better able to judge whether the one suggested, or rather hinted at, by the Parliamentary Committee be adequate to the object proposed.

Of all the employments in which large bodies of men are occupied—with the exception, perhaps, of actual warfare—there is none to compare with Mining. Danger is always at hand; destruction and death are frequently present. The Parliamentary inquirers, it would seem, are not ignorant of this.

"Your Committee have had ample opportunity of multiplying proofs of the calamities which have occurred in the mines of this kingdem, by sudden explosions of fire-damps, foul air, or sulphur; all which terms are locally applied to carburetted-hydrogen gas, so copiously evolved in many of those mines. Few collieries are entirely free from fire-damp, but *in many the quantity emitted is so large that, in spite of skill and unremitting attention, the risk is constant and imminent*."—Report, Accidents in Mines, p. iii.

In proof of this assertion, the Committee insert in their Report all the coroners' returns they could procure, of inquests held in the mining districts during the last twenty-five years. The sum total is appalling. In the collieries along the rivers Tyne and Wear alone, the number of lives lost during the period just mentioned is 1125: in the other mining counties, exclusive of Durham and Northumberland, the incomplete, but, so far as they go, authentic records,

state the number at 954; giving, altogether, about 2070 as the number of those who perished by various *accidents* in the labour of the mines during twenty-five years! And this the Committee believe to be "vastly short of the actual number" of the victims.

We confess we are disappointed in the practical deductions of the authors of the Report; we doubt the efficiency of what they suggest, and we are disposed to think that their proposed measures cannot lead to much good. They state it to be their impression that "great benefit might be fairly and sanguinely anticipated from men of known ability being *encouraged* to visit the mines, whether in the character of distinguished chemists, mechanists, or philanthropists." "They are assured," they say, "that these visits would be received *with pleasure* by the mine-owners, and that every assistance in the way of experimenting would be promptly afforded." To us all this sounds flat and trumpery. Does it not almost border on the ridiculous to find legislators suggesting pleasure visits, and experimenting tours, to be *encouraged* by the countenance of government? Is this a way in which to meet the emergency? Is it thus an occupation which sweeps away hundreds of lives annually is to be regulated? Where are those men of known ability, distinguished chemists and mechanists, who are expected to play the *dilettanti*, and every now and then take a trip, for the sake of experiment, to the Wallsend colliery, and dive into the Bensham seam? We know of none such; and if the Committee think that an affair of this kind may safely be left to the voluntary services of amateur men of science, we can only warn them that they reckon without their host, and rest their sanguine expectations upon men of straw.

We repeat, that functions of the nature of those pointed out by the Committee, can only be safely calculated upon when they are performed as *duties*. Let the scientific and philanthropic visitors be endowed with official powers—let them be authorized to investigate frequently, and at what times they think fit, the state of the mines, with reference to the risk of fire and choke damp—let nothing depend upon mere encouragement and the pleasure of the mine-owners, and we answer for it, fewer accidents will be heard of — fewer will take place. Vigorous measures, in short, ought to be adopted. A heavy responsibility ought to be legally attached to the conduct of employers who commit to mere children the charge of trap-doors of vital consequence to the safety of the workmen. Only think of putting, in this way, into the hands of a child of 8 or 10 years old, the lives of hundreds of operatives; yet such is the constant practice in some collieries—persisted in even after the most woeful and repeated experience of the terrible effects that may result from it. There is no accounting for this sort of recklessness in defiance of common discretion; it amounts to infatuation.

There are three points in the Report which deserve a more than ordinary attention. These are the observations—1, on the system of ventilation followed in the mines, and the means suggested by intelligent witnesses for improving it; 2, on the use of the safety-lamp, and the consideration of the means by which it may be rendered perfectly secure; and, 3, the advantage of having exact maps and plans of the workings, pointing out also the position of adjacent abandoned mines, which may have become reservoirs of gas or of water.

The second point is the one in which we suspect the reader will take most interest. In their introduction of the

subject the Committee use these words—"The invention claimed by the late Sir Humphry Davy, on principles demonstrated by that able philosopher, may be considered as having essentially served the mining interests of this kingdom, and through them contributed largely to the sources of national as well as individual wealth. Many invaluable seams of coal never could have been worked without the aid of such an instrument, and its long use throughout an extensive district, with the comparatively limited number of accidents, proves its claim to be considered, under ordinary circumstances, a safety-lamp."

In connexion with this high and well-merited eulogium, we are tempted to lay before the reader "a striking fact," as mentioned by the reporters:—

"One striking fact requires to be particularly pointed out. If the year 1816 is assumed as the period when Sir H. Davy's lamp came into use, a term of eighteen years since 1816, and a similar term prior to 1816 being taken, it will be seen that in the eighteen years previous to the introduction of the lamp 447 persons lost their lives in the counties of Durham and Northumberland, whilst in the latter term of eighteen years the fatal accidents amounted to 538. To account for this increase, it may be sufficient to observe, that the quantity of coal raised in the said counties has greatly increased; seams of coal so fiery as to have lain unwrought, have been approached and worked by the aid of the safety-lamp. Many dangerous mines were successfully carried on, though in a most inflammable state, and without injury to the general health of the people employed in them. Add to this, the idea entertained, that on the introduction of that lamp, the necessity for former precautions and vigilance in great measure ceased."—*Report*, p. v.

With this quotation we discontinue our remarks for the present; for we find we must postpone our further observations on the subject till another opportunity.

CHAIR OF SURGERY IN EDINBURGH.

Sir Charles Bell has been unanimously elected to the professorship of surgery in Edinburgh, rendered vacant by the death of Dr. Turner; and we understand that he has accepted the chair which has been offered him in so gratifying a manner. That the electors have made a selection which reflects honour on their discernment, will be doubted by none, except perhaps the unsuccessful aspirants, and we believe that most even of them will feel that there is no dishonour in giving place to one of their brethren so justly distinguished.

As a lecturer Sir Charles Bell has long been pre-eminent, and he has consistently through life insisted on the great importance and honourable nature of the duties which devolve upon the medical teacher. Circumstances had latterly in some degree removed him from the sphere of his usefulness in this respect; and convinced as we are, from many of his recorded sentiments, that the lecture-room must ever be the scene of his happiest exertions, we rejoice for his own sake that an opportunity is again presented him, and under such flattering auspices, of resuming his useful and honourable calling in the field of his early labours.

Sir Charles Bell will next session lecture in Edinburgh, and ere then will of course have vacated his place among us; but we are sure that he will carry with him to the northern metropolis the respect and good wishes of all his professional brethren here. His departure will create vacancies in the offices of surgeon, and probably of assistant-surgeon, at Middlesex Hospital, and also in the Council of the College in Lincoln's Inn Fields.

As might be expected where there were disappointed candidates, some

petty manœuvring took place prior to the election, and it appears that some letters disparaging to Sir Charles were industriously circulated. We shall not stop to inquire into their origin; but it is gratifying to us to observe that one of the electors, in answer to these pasquinades, read various extracts from the MEDICAL GAZETTE, to prove the estimation in which Sir Charles Bell was held as a man of science, and we are pleased to find our humble testimony to his merits converted to so honourable a use.

Since the above was written, we have seen in the *Courier* some very absurd remarks on this subject, evidently supplied by some injudicious friend of Mr. Liston. The public are therein given to understand, that the Chair of Surgery was in the first instance offered to that gentleman, by whom it was declined; and the *Courier* is pleased further to inform us, that Mr. Liston will not give up his prospects in London "for any of the medical chairs in Edinburgh." We feel as much assured of this as the *Courier* can be. As to the rest, to suppose that Mr. Liston, notwithstanding his sudden journey to Edinburgh on the death of Dr. Turner, ever had it in his power to obtain the professorship just conferred on Sir Charles Bell, is—a mistake. The real state of the question, as regards all parties, will be seen from the speech of the Lord Provost, which we subjoin *, and which, from the unquestionable nature of the authority, renders any farther remarks of ours altogether superfluous.

"The Lord Provost rose and said, that the Council had now arrived at that stage of the proceedings when it became their duty to take into consideration the filling up of the vacant chair of Surgery in the University of Edinburgh—one of the most important duties, he believed, that had fallen to their lot to perform, since they (the Reformed Council) had the honour of being appointed to the management of the city's affairs, and he might safely say, that the eyes of Europe were fixed upon the Council in anxiety respecting the result of their deliberations on the subject. It therefore became the Council most seriously to consider well the step they were about to take. He confessed, that for his own part, he had at first, when the vacancy took place, entertained the idea that there was none between that chair and Mr. Liston; but, *upon further consideration, and hearing the opinions of others, he had seen reason to abandon that idea. He had all along endeavoured to avoid proposing any thing at that board but what was agreeable not only to the Council but to the public; and he had accordingly kept his ears open to public opinion. He had hence discovered that the eyes of the greater part of the public were concentrated on another individual than the one he had anticipated.* He should not hurt the feelings of any one, by drawing comparisons between one candidate and another; it was not called for, and would, therefore, be highly improper. The minds of the Council being made up as to their choice, it was desirable that the matter should not be allowed any longer to be hung up, creating hopes and desires, as it would unnecessarily do, in the minds of other candidates; but that they should proceed at once to nominate the person whom they conceived best fitted for the situation. He should therefore propose, that an offer of the vacant chair should be made to Sir Charles Bell. (Cheers.) In making this proposition it would not be expected that he should dilate upon that gentleman's professional merits, a task for which he was utterly unable; he should do more justice to the merits of so distinguished a physiologist by merely mentioning his name. (Renewed cheers.)"

* From the "Scotsman."

LECTURES ON THE DISEASES OF THE NERVOUS SYSTEM.

BY M. ANDRAL.

As published in the *Gazette des Hôpitaux*, with the approval of the learned Professor.

INFLAMMATION OF THE BRAIN.

Anatomical characters of Encephalitis—Causes—Symptoms—Affections of Motion (Convulsions, Cramps, Paralysis)—Affections of Sensation—Special Senses—Affections of the Natural Functions (Digestion, Circulation, Respiration)—Treatment.

INFLAMMATION of the nervous centres does not manifest itself by symptoms so well

marked as those which indicate the inflammation of other organs—as of the lungs, for instance; that of the brain, in particular, derives a part of its obscurity from the circumstance of its often being difficult to distinguish what belongs to it and what to inflammation of the meninges. Besides, the disease is not so common as some would have us to suppose; unless, indeed, we call every disorder of these organs, with anatomical change, by the name of inflammation, as appears sometimes to have been done. To characterize inflammation of the nervous centres, an assemblage of phenomena is required, into such details concerning which I shall enter by and by, as will, I trust, render the subject complete.

We must not, for example, mistake for inflammation of the brain the nervous symptoms which accompany follicular ulceration of the bowels. There are few children who sink under an acute disease without exhibiting nervous symptoms, where, nevertheless, there has been no encephalitis; the disturbance having been entirely the effect of sympathetic reaction. Towards the close of many chronic diseases, we observe a variety of symptoms referable to the brain, without, however, our having any right to assume that there is encephalitis: for instance, we see this in phthisical patients, in whom the brain is rather in a state of anemia; nor is this fact of light importance in practice. In typhus fever, too, the nervous system acts an engrossing part; there is disturbance in the brain as well as in the alimentary canal, but inflammation is no more necessary for the one than the other.

The brain may be disturbed in its functions without inflammation; thus nervous delirium—delirium tremens—is not the result of inflammation, for it resists the abstraction of blood, and yields, as if by magic, to large doses of opium; a medicine which, as we know, is very injurious in inflammation of the brain.

Encephalitis is divided into inflammation—first, of the brain—secondly, of the cerebellum—thirdly, of the central white substance. Acute inflammation of the brain may be general, which is very rare, or partial, which is much more common. One hemisphere may be attacked while the other remains free, or a portion only of a hemisphere may be inflamed; in short, I have here to repeat what I have already said with regard to congestions. Inflammation may have its seat on the external surface, in the internal white substance, or in the cerebellum.

Anatomical characters.—These are invariable, whatever may be their seat. They consist of injection, more or less deep, of the grey or white medullary matter, just as in simple congestion. Are there any characters by which it may be determined with certainty whether the injection result from inflammation, or only from hyperemia? There are not; and, indeed, simple congestion often passes insensibly into inflammation; a circumstance which is not peculiar to the brain, for it is not always possible to distinguish positively between mere engorgement of the lungs and recent pneumonia. But we do not find injection alone in all such cases. Thus sometimes the cerebral tissue shows manifest tumefaction, resulting from an increased determination of blood, so considerable as to augment the bulk of the hemispheres. The brain being rendered more voluminous, and being contained in a cavity which admits not of distention, is compressed; and various symptoms are developed, corresponding to the degree of this compression. When the skull-cap is removed, we frequently see the diseased hemisphere project above that which is sound; the convolutions, pressed and indented into each other, no longer exhibit their enfractuosities. The inflamed hemisphere, being more bulky, tends to encroach upon the other, and disturbs it by its mechanical pressure.

Inflamed nervous texture also changes in its consistence; and we have here a general law of pathological anatomy in regard to inflamed organs: thus softening of the brain may be a consequence of inflammation; and with inflammatory softening, when acute, there is always injection. This does not hold good, however, in chronic inflammation. One of the terminations of inflammation, in all organs, is ulceration: this is, indeed, rare in parenchymatous structures, and also in the brain; but it has been observed in this organ.—(Scoutteten and Abercrombie.)

Every inflamed tissue tends to the formation of pus; the brain does not escape the general law, and the presence of purulent matter is a satisfactory proof that there has been inflammation, except in those cases where this is the result of abscess formed by metastasis. The pus may exist at first in the state of diffusion, but by degrees, and in proportion as the disease advances, if the patients are not carried off too soon, the molecules of pus are collected into masses, and abscesses are formed: this is a kind of abscess, however, which is much more common as a consequence of chronic than of acute inflammation. Sometimes there is but one such deposition, sometimes there are several.

It is doubtful whether inflammation of the brain ever terminates in gangrene, although some examples of this kind are re-

corded in the Memoirs of the Old Academy of Surgery, and M. Lallemand has related another in his excellent work.

But these lesions are not the only ones that may be met with; thus the meninges become inflamed here as the pleura does in pneumonia, and it is the affection of the meninges which gives origin to the effusions found between the membranes, or in the ventricles, and which occasion great modifications in the symptoms.

Causes.—These are often the same as what we have seen to produce congestion; but there are some also which act more especially in causing encephalitis. In the external world, insolation plays an important part, and the same holds good with spirituous liquors taken to excess. External violence, which has little part in the production of congestion, is all-powerful in exciting inflammation; among these are blows, falls, &c. with or without solution of continuity. The blow, indeed, may not have been upon the head itself, but have communicated a shock to the whole body, and produced a commotion which only becomes completely dissipated to give place, sooner or later, to genuine inflammation. If there has been solution of continuity, it may either have affected the bone or only the soft parts which cover it; and it is by no means very rare to see inflammation of the brain resulting from a simple wound of the hairy scalp. Again, there are cases wherein the bone externally is sound, but the inner table being fractured, its periosteum becomes the seat of inflammation, which thence extends to the adjacent parts.

Sometimes the causes of encephalitis are foreign bodies, which wound the nervous pulp. A projectile may cause inflammation, with or without remaining in the part. But the action of all these causes is far from being instantaneous, and it is not uncommon for it not to become manifest until after the lapse of a very long period. Thus in certain cases a bullet has been known to remain several months in the midst of the brain, without producing any accident; shortly after, violent symptoms have suddenly become developed, death has taken place, and, on post-mortem examination, the projectile has been found to be surrounded by a layer of inflammation.

We may also discover causes of encephalitis in certain chronic affections of the brain; thus apoplectic cysts, acting as foreign bodies, excite genuine inflammation around them. Numerous accidental productions do the same, such as malignant tumors or tubercles, particularly in children, who have them at the same time developed in various other organs. It is around these that inflammations form, generally chronic, but sometimes acute. Now it is not the tubercles which prove fatal in such cases, for they may have existed long without producing serious mischief; but it is the inflammation excited around those productions which is the cause of death.

Among the causes we ought also to mention diseases, whether acute or chronic, of the meninges; and also caries, exostosis, &c. of the bones of the head, particularly the pars petrosa of the temporal.

The organs of sense give us the following causes:—In the eye, internal ophthalmia, such as violent iritis; in the ear, inflammation affecting the deep-seated parts; in the nose, inflammation of the olfactory surface, particularly if it extend into the frontal sinuses. It is not uncommon to see intense, and even fatal, inflammation of the brain, result from the extraction of simple or malignant polypi from the nose.

With regard to the skin, inflammation of this on the face or scalp, in the different forms of erysipelas, may cause encephalitis; we must not, however, suppose that the delirium of a patient labouring under erysipelas always depends on inflammation of the brain, for it may be the result of a mere sympathetic re-action, or nervous disturbance; but it is requisite, when there is any doubt, to act as if we were certain that inflammation was present.

Nerves which communicate with the brain may be the points whence encephalitis originates. M. Bouillaud has seen an example of this kind, in which, after the application of ligatures on the nerves of the arm, violent pains were excited, which, spreading progressively along their course, soon arrived at the brain.

There is no organ the inflammation of which may not prove the occasional cause of encephalitis; but it is necessary to keep in mind that every disorder which accompanies such inflammation does not prove that this condition has also extended to the brain. Teething may also be enumerated among the causes, although it is congestion rather than inflammation which is produced.

Again, the brain may become irritated and inflamed simply by its own increased action, such as results from long-continued intellectual exertion, violent emotions, &c.

Symptoms.—These, as with regard to congestion, vary in proportion to the intensity, the extent, the nature, and the seat, of the lesion. Sometimes they become blended with the symptoms of acute meningitis. They may be divided into two series, according as they depend upon—1st, disorder of the functions of the brain; 2d, disorder of the functions of nutritive life.

Cerebral functions.— In the first series

are ranged the disturbances of intellect, motion, and sensation. The disturbance of intellect may be the predominant symptom, as in the phrensy of the ancients, and delirium in all its forms, furious or tranquil, taciturn or loquacious. The study of these numerous varieties contributes nothing to our knowledge of encephalitis; and, indeed, they are often idiosynchronous. Delirium may show itself alone without appreciable lesion of sensation or motion, or be attended by disturbance of both these; and what is remarkable, — in the most unequivocal inflammation of the brain, the intellect may regain its clearness from time to time. The same remission may also be observed in the derangements of other organs.

The delirium is increased by noise, and sometimes by the slightest degree of light. It continues during a period which varies, and is followed by coma more or less profound. There may be alternations of delirium and coma; and if the disease tends to a fatal termination, the coma shews a disposition to become permanent; nevertheless, death may take place during the delirium. The delirium becoming permanent, or succeeded by coma, or alternating with this, is very frequent in encephalitis. Sometimes, however, the intellect may remain entirely undisturbed during the whole continuance of this disease, particularly when it affects those parts only which are at a distance from the convolutions. Sometimes the intellect is disturbed only at the commencement; sometimes, entire at first, it becomes affected afterwards, its derangement being then added to those of sensation and motion. The intellect may regain its integrity after the recovery of the patient, or it may remain weakened for a longer or shorter time. Alienation, or mental debility, so as to prevent the individual from any continued exertion, may also take place. In some the memory is permanently impaired.

Affections of motion.—In some patients we observe as an unique phenomenon great agitation of the limbs, which are affected as with a kind of chorea. To this is often added starting of the tendons, similar to what occurs in follicular enteritis, even when there is no inflammation of the brain; but in encephalitis much more certain indications are afforded by convulsions, spasms, and paralysis, which are rarely found in any marked degree in merely sympathetic disturbances.

Convulsions.—The convulsions may implicate a great number of muscles at the same time, and produce singular distortions of the eye, grimaces of the face, or inability to speak. Sometimes we have preternatural movements of the limbs, which must be carefully distinguished from simple agitation. However, these convulsions alone do not suffice for the diagnosis of encephalitis, for we may also have them, though less violent, in typhus fever, where, indeed, they differ only in degree from the convulsions which attend inflammation of the brain.

The convulsions may be either general or partial, and may affect one or both sides of the body: if they be general, there may be some doubt in the case, but if they be limited to one side of the body, we may infer that there is something amiss in the brain.

Cramps.—This symptom consists, as is known, in permanent contraction of the limbs, with inability to produce extension by the most violent efforts; it is characteristic of the disease before us. These cramps, but slightly marked, and not constant in the first degree of inflammation, are strong and constant when there is softening. They are not met with in the enteritis of typhus fever; but in inflammation they may be seen at times in all the limbs.

Paralysis.—This may have a variety of seats: thus, it may attack the face, the limbs, tongue, pharynx, the muscles of respiration, &c. The paralysis may come on suddenly, and at the very commencement of the disease, but this is rare. In the great majority of cases the paralysis does not show itself till a more advanced period; it is usually preceded by convulsions, and more particularly by cramps; and then it is a symptom of the greatest importance; whilst, if it declare itself at the onset, it is just as likely to depend upon apoplexy. If, however, it be accompanied by delirium at the same time, we have more reason to suspect the presence of inflammation. In certain cases there is paralysis of one side and cramps of the other; or the same limb may be the seat of paralysis, convulsion, and cramp, alternately.

Affections of sensation.—We often observe pain of the head to manifest itself at the very commencement, and before the appearance of any other symptom. These pains are not an absolute sign of encephalitis, as they may also occur in meningitis. The pain is more severe in inflammation about the brain, than that which accompanies the onset of certain other diseases, such as typhus fever. The patients experience sensations which are quite peculiar; some think they hear a constant whistling, others are alarmed by loud and repeated detonations, and yet others are tormented by noises resembling the constant rushing of a torrent. When, in the course of any disease, the symptoms just enumerated come on, it behoves us to direct our attention to the brain.

Sometimes the general sensibility is exalted in an extraordinary degree; in other instances a paralysis of sensation accompanies the loss of motion.

Special senses.—In the first period, which is that of excitement or exaltation, we may observe in the eye that the pupils are contracted, and that there is intolerance of light; in the ear that there is extreme sensibility to sound, ringing, buzzing, &c. In the second period the senses are blunted, weakened, and sometimes even abolished. The sight is lost, the pupils are dilated and immoveable; the patients are affected with deafness, but the lesions of the sense of hearing are not always dependent on encephalitis alone; for it occurs also in typhus fever; but it is very rare to see it so completely abolished in this as it is in inflammation. It is the same with regard to vision. When we meet with complete blindness, it is a reason for suspecting that there is mischief in the brain. And further, the conjunctiva is never completely insensible to the touch when the fingers are applied to it in typhus; but this frequently happens in inflammation of the brain.

The disturbances of the nutritive functions come next.

Digestion.—This is often considerably affected; thus vomiting frequently marks the onset of the disease. In many persons, too, who are attacked with acute encephalitis, very obstinate constipation is often observed. With these positive signs there are others of a nature purely negative.—Thus, the tongue is natural, the abdomen soft and without distention; signs which are of importance when we have to establish a diagnosis between this disease and the enteritis of fever.

Circulation.—At the access of inflammation of the brain, if it be violent and general, the pulse is accelerated; but often, also, particularly when there is effusion, the circulation becomes slower; and lastly, if the inflammation, though acute, be partial, the pulse is, in a majority of cases, quite natural.

Respiration.—If the disease be intense, the breathing is embarrassed, becomes stertorous, and shows the same derangements as in those cases in which there is hæmorrhage into the brain.

In other respects the symptoms of acute encephalitis differ much, according to various circumstances: 1st, according to the nature of the lesion. If there be simple irritation of the brain, it is manifested by an exaltation of function and by delirium; if, on the contrary, there be compression of the cerebral substance, there is weakness, and tendency to collapse. 2nd, According to the seat of the lesion. Thus, inflammation of the cerebral lobes of the central parts of the ventricles, of the white or grey substances of the cerebellum, have each their special signs. I should here have but to repeat the observations which I dwelt upon so long in speaking of congestion.

To recapitulate, I may say, that in acute inflammation of the brain, the symptoms supervene and succeed each other in a manner which forms two distinct periods. The former is characterized by excitement: it is then that we have delirium in all its varieties, and that we often have, besides, violent agitation, convulsions, cramps, and similar phenomena. The latter period belongs to the state of coma, and the different forms of paralysis, with various symptoms characteristic of a diminution of the organic functions. But it is to be constantly kept in mind, that the phenomena which characterize these periods may be mixed up with one another, and are not always successive: they may alternate in their manifestation; and the disease, in certain cases, as it were begins with the second period, without there having been any thing preceding it which could be regarded as constituting the first stage; thus we may have paralysis without any antecedent convulsions. In others, the disease is limited to the phenomena of the first period, delirium, convulsions, and spasms; in the midst of which scene of excitement death takes place, before any of the signs of prostration indicative of the second period have arrived; so that the disease presents three different aspects, according as, 1st, it goes through all its stages; or 2nd, the first period be wanting; or 3rd, it be limited to the first period.

If now we turn our attention to the mode of attack, we shall find it marked by important phenomena, but which are not always the same. Thus, in certain individuals, before the development of the assemblage of symptoms which mark acute inflammation of the brain, we observe an intense degree of fever, with indications of cerebral congestion; in a word, we have the inflammatory fever of the ancients, and gradually only do the characteristic symptoms of encephalitis display themselves. In others, whilst they are in the midst of health, and without any warning, delirium, with all its accompaniments, comes on, without any lesion whatever of motion. Are we, then, to conclude from this, that we have to deal with acute inflammation of the brain every time we observe fever with delirium? Certainly not; for I have seen more than one case in which typhus fever has begun with nervous symptoms of this kind: the circumstance is rare, indeed, but it is sufficiently established. Other persons, at the onset of acute encephalitis experience some sudden disturbance of motion, as convulsions, cramp, or paralysis, more or less general; and this mode of onset enables us to form

our diagnosis much more surely and more easily than in the two other forms previously described.

The cases to which we have alluded are of daily occurrence; but there are others which, being uncommon, ought, when they present themselves, to be noted with the greatest care. It is thus that sometimes the disease does not make itself manifest by any of the capital symptoms already pointed out; but the observer is struck exclusively by the presence of complete inability to articulate; this symptom alone being present for a longer or shorter time, to be followed, however, by the other phenomena. Abercrombie relates the case of a young man, fifteen years of age, who, after having bathed, fell asleep exposed to the rays of a burning sun. On waking, he had lost the power of speech, but there was no other lesion of the motive powers, and the intellect remained entire. After three or four days his mind became disturbed, all the phenomena of encephalitis came on, and he died. The post-mortem examination proved the brain to be inflamed in the most acute degree, with numerous points of suppuration in both hemispheres. It is worthy of remark, however, that there was nothing observed in the anterior lobes. This is, without doubt, a remarkable case, in which the loss of speech continued, for more than three days, to be the only symptom; and certainly, from this solitary fact it was impossible to divine before-hand the intensity and extent of the lesion.

In other cases, it is on the side of the nutritive life that the symptoms commence which are to end in the manifestation of acute cerebral inflammation. Thus, before any other phenomenon occurs, we may have vomiting more or less obstinate, depending upon the sympathy produced on the stomach by the brain.

Duration.—Acute inflammation of the brain is very variable in its duration: sometimes it proves fatal in less than twenty-four hours; at others the disease does not come to a close till after the lapse of a much longer period; it may remain acute for thirty or forty days, or even for two months, and then pass into the chronic state, ending in a return of perfect health, or leaving behind imperfections, more or less severe, of intellect, motion, or sensation. Thus the motions of certain muscles remain imperfect; and particularly those of the eye are occasionally so modified after an attack of encephalitis, that strabismus is produced. In other cases the fingers are affected, more or fewer of them remaining contracted. The sensibility may also be observed to be impaired in various parts of the body.

Treatment.—This does not differ essentially from that of other acute inflammations: there are, however, some particular indications to be laid down. First, we must commence, in every case, by the abstraction of blood; and the rules by which we are, in this respect, to be guided, do not differ from those already mentioned in speaking of cerebral congestion. After the bleeding follows a means of great power, namely, the application of cold; but we must take care not to employ it until we have combatted the re-action by repeated depletions. If the re-action has not become developed, cold may be employed much earlier, but never without the greatest precaution. The application of cold before the re-action has been overcome renders this much more violent, and capable of producing the most fearful mischief. Another inconvenience attached to the employment of this means is, the production of too powerful a collapse, and coma which nothing can remove. It is necessary to be aware of these two dangers, between which it must be our object to steer our course. However, it were better to have too powerful a re-action, which we have the means to combat, rather than a state of collapse, against which all our means are frequently unavailing. The application of cold is to be made by ice to the head permanently, and not temporarily, or for short periods; the intervals, under such circumstances, affording the re-action time to be re-produced. The ice must not lie heavily on the head; it must be pounded, and renewed from time to time. There are some persons who receive a disagreeable impression from the cold, not for a short time only, which is common, but permanently; and when this happens, its use must be discontinued. To others, again, the application of the ice affords the greatest comfort, and often leads to the restoration of the intellect, the cessation of the delirium immediately following the application of the ice, which such patients beg for with importunity.

Cold may also be employed in another form, that of affusion of water at the temperature of 22, 21, 18, or 16 degrees (cent. ther.); very rarely, however, beneath this. These affusions are applied at intervals longer or shorter, according to circumstances; and each may last at first from one to two minutes, but may afterwards be continued much longer. In certain cases it has been contrived to keep a constant current over the head and face, a moderate temperature. In yet other cases cold water is made to fall drop by drop from a certain height on the head; and some physicians attach great importance to this method. I have seen a young woman, having all the signs of a severe encephalitis, perfectly cured after being subjected to this plan during four days; this occurred in the practice of M. Recamier. Cold is certainly a powerful agent,

but one the management of which requires great skill: improperly applied it may be productive of the most formidable mischief. General cold affusion has been used in cases of violent febrile action: the whole surface of the skin may be sponged with water, or vinegar and water, care being taken to keep the head cool at the same time.

Revulsives.—All the medicines belonging to this class have been used—sinapisms, and blisters to the limbs, neck, or even to the head itself; but not until after the reaction has been subdued by depletions or by cold. There are some individuals, however, who do not bear cutaneous irritation, and in whom the febrile symptoms are reproduced as soon as the skin becomes red from a blister or sinapism.

In general it is necessary to employ cold in conjunction with bleeding, rather than revulsives, the premature use of which is particularly to be feared. Nevertheless, revulsives are sometimes of great advantage in cases of profound collapse, or persistent and still increasing coma; a state which must not be confounded with the condition in which mere sinking prevails, and in which the use of revulsives is often dangerous. In the former, which requires the employment of active revulsion, the eye is insensible to light, the pupil motionless, and the general sensibility almost entirely extinguished. In cases of this nature occurring in infants, I have covered the scalp with a large blister, with decided advantage; but I have carefully avoided this plan as long as there was any heat of skin, or the least remains of erethism.

Mercurial preparations have been held up as specifics in acute inflammation of the brain, but I have never seen them of much avail. They have been rubbed in on the back of the head or neck, but the action of such means is not rapid,—a certain time is required, and meanwhile the disease advances. They cannot conquer the disease unless they have an opportunity of encountering it. Calomel has been given internally, particularly in the treatment of infants, where, indeed, it has been regarded as a specific; but it is doubtful whether it acts otherwise than as a purgative. In recurring to the cases which have been published, you will find that the calomel has acted upon the bowels, evidently producing revulsion, as all purgatives do; perhaps, however, it may be more efficient than other medicines of this class, because it exerts little irritation, may be given in very small doses, and has no disagreeable taste, — circumstances which are of great importance in the treatment of infants or of adults affected with delirium.

ABUSE OF THE STOMACH-PUMP.

THE LUNGS INJECTED WITH MUSTARD.

To the Editor of the Medical Gazette.

Sir,

In a clinical lecture by Dr. Watson, contained in your last number, I find some excellent remarks on the abuse of the stomach-pump; and a case is mentioned where a quantity of *chalk mixture* was found in the texture of the lungs after death. Believing that the injection of the lungs is an accident of more common occurrence than is generally supposed, and having several times observed the rude and careless manner in which this valuable instrument is used, I am induced to make the following circumstance public:—A few years ago, on entering one of the wards of the hospital with which I am connected, I found one of the resident medical attendants actively engaged, injecting by means of the stomach-pump, a strong infusion of mustard, in a case of poisoning from opium. The patient, a young man, was evidently in a state of asphyxia, and in a very few minutes before the instrument was withdrawn, expired. Being present at the post-mortem examination, I was surprised on finding the lungs remarkably distended, and that they did not collapse when the chest was opened. On making a section of them, the individual who had introduced the stomach-pump, immediately exclaimed, "How thickly they are filled with *miliary tubercles;*" and really they had a very curious appearance, the texture of both lungs being thickly studded with small yellow spots, which, from the odour, we soon recognized to be caused by the presence of the mustard, which had been forcibly injected into the numerous vesicles of this organ instead of into the stomach. The same substance was found in the bronchial tubes, trachea, and larynx. This is not the only instance of similar culpable negligence which has come to my knowledge, and by directing attention to the subject, you may probably be the means of saving several lives.

Your obedient servant,

Chirurgus.

Dec. 14, 1835.

[The writer has given us his name.—E.G.]

COX'S DISPENSATORY.

NOTE FROM MR. COX IN REPLY TO MR. NIAARP.

To the Editor of the Medical Gazette.

Sir,

Perceiving in your journal of last week an attack on my character by Mr. Niaarp,

I trust you will allow me a small space for reply.

The New London Dispensatory published by me, and designated by Mr. Niaarp as a cheat and imposition, contains a translation of the *last* London Pharmacopœia, and has been considered by competent judges a correct and valuable work. The epithet *new*, was used to distinguish it from the London Dispensatory of Dr. Thomson.

There has been no alteration in the London Pharmacopœia, as is well known, since 1824, at which time the work referred to was published, therefore on appending to it, this year, some hundred pages of new matter, viz. the Formulary of M. Magendie, I was justified in calling it an improved edition. I may also add, that the original price was 10s. 6d.; and although the above important additions have been made, the price has only been raised to 12s. Surely this is no aggression upon the purse of any one except the publisher.

As to my imposing on the public by selling Lawrence's Lectures in such a form that only twenty out of eighty-eight lectures are introduced, I have merely to say that the work was not published by myself, but by Mr. Westley, of the Strand. Mr. Niaarp should have mentioned that he only gave 5s. 6d. for the book, which, if purchased in the numbers of the Medical Gazette, would have cost him more than two guineas.—I am, sir,

Yours respectfully,
E. Cox.

St. Thomas's-Street. Southwark,
Dec. 10, 1835.

BENEVOLENT DISPENSARY.

To the Editor of the Medical Gazette.

Sir,

May I request you will apprise your numerous readers that the practice of the Benevolent Dispensary (the prospectus of which appears as an advertisement in the wrapper of this day) is free to the medical public, and that I shall feel a pleasure in relieving any poor patients my professional brethren may send to the charity.

I remain, sir,
Yours very obediently,
Frederick Salmon.

12, Old Broad-Street,
Dec. 8, 1835.

LITERARY INTELLIGENCE.

A Brief Memoir of the late Sir William Blizard, as read before the Hunterian Society, with Additions, by William Cooke, Esq., will shortly be published.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

December 17, 1835.

Joshua Goodwin Kershaw, Bradford, Yorkshire.
Rudd Lucas.
John Stedman, Godalming.
William Martin, Reigate.
Thomas Haymes, Great Glenn, Leicestershire.
James King, London.
James Bell Metcalfe, London.
Andrew Joseph Doyle, Lewes, Sussex.
John Daniel Vittoria Packman, Puckeridge.
Alfred Henry Vallack, Kingsand, Devon.
Richard Godfrey Lowe, Bristol.
Thomas Kean, Reading.
John Collier Seccombe, Plymouth.

WEEKLY ACCOUNT of BURIALS,

From Bills of Mortality, *Dec.* 15, 1835.

Abscess	38	Heart, diseased	13
Age and Debility	270	Hernia	4
Apoplexy	52	Hooping Cough	79
Asthma	89	Inflammation	153
Cancer	6	Bowels & Stomach	18
Childbirth	36	Brain	13
Consumption	346	Lungs and Pleura	48
Constipation of the Bowels	2	Insanity	24
Convulsions	169	Jaundice	2
Croup	15	Liver, diseased	26
Dentition or Teething	37	Measles	79
Diabetes	3	Miscarriage	2
Diarrhœa	4	Mortification	14
Dropsy	71	Paralysis	12
Dropsy on the Brain	41	Small-pox	59
Dropsy on the Chest	5	Sore Throat and Quinsey	5
Epilepsy	8	Spasms	2
Erysipelas	4	Stricture	1
Fever	46	Thrush	13
Fever, Scarlet	31	Tumor	2
Fever, Typhus	4	Worms	1
Fistula	1	Unknown Causes	55
Gout	16		
Hæmorrhage	1	Stillborn	47

In this week's bill, which closes the series for the present year, are comprised the reports of several Parish Clerks, who have neglected to make their returns in due order, and who have now sent them for insertion in the general or yearly bill: from this circumstance alone has arisen the seeming increase of burials.

METEOROLOGICAL JOURNAL.

Dec.	Thermometer.		Barometer.	
Thursday . 10	from 23	to 33	30 19	to 30·30
Friday . . 11	15	31	30·15	30·21
Saturday . 12	15	34	30·19	30·22
Sunday . . 13	26	39	30·22	30·23
Monday . . 14	28	39	30·23	30·25
Tuesday . . 15	32	40	30·27	30·25
Wednesday 16	32	43	30·24	30·29

Prevailing winds, S.W. and N.W.

Except the 11th. generally cloudy; with rain on the mornings of the 12th and 16th: a little snow fell on the morning of the 10th.

NOTICES.

"Mr. M.," without authenticating his letter, offers us a statement of facts in reply to a communication with a real signature. We must decline inserting it unless we recive the writer's name.

Our correspondent at Newbury is informed that the Inquest has not escaped us.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, DECEMBER 26, 1835.

LECTURES
ON
MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XIII.

REPTILIA.

HAVING finished two of the vertebrated classes—Mammals and Birds—we advance to *Reptiles*. Here, however, we find no pharmacological agent; but I shall very briefly notice, *en passant*, two or three animals interesting either from having once yielded something to medicine, or from their poisonous qualities.

Reptilia.—In the Cuvierian arrangement, the term Reptilia (from *reptilis*, creeping) includes not only the reptiles commonly so called, as turtles, lizards, and snakes, but those denominated Batrachia (from βατραχος, *a frog*), or the amphibious animals. The impropriety of this method is now generally admitted; and by most late writers Reptilia and Amphibia form distinct classes. I follow their example.

Reptiles, properly so called, have the following characters:—Their blood is cold, with elliptical globules; the heart has two auricles; the respiration pulmonary; the generation oviparous (in some cases being ovo-viviparous); and their skin is furnished with scales: hence they are termed by Blainville the *Squamifera*.

We subdivide them into three orders, thus:—

			Orders.
REPTILIA	having extremities	no teeth	Chelonia—as the Turtles.
		teeth	Sauria—as the Lizards.
	no extremities	....	Ophidia—as the Snakes.

Order 1st—*Chelonia.*

The Chelonian reptiles, or tortoises, are recognized by the strong bony or horny shields with which their bodies are defended. The upper shield, called *carapace*, back-plate, or buckler, is formed by the immoveable expanded ribs; while the lower shield, termed *plastron* or breast-plate, is constituted of the pieces representing the sternum. The mouth has no teeth, the upper jaw closing over the edges of the lower. Some are terrestrial, others aquatic: the latter usually have their feet more or less webbed (as in the *Testudo caretta*). Those tortoises that inhabit the sea are denominated turtles.

Three tortoises have been employed in medicine; namely, the *Emys europæa*, the *Testudo lutaria*, and the *Testudo græca*. From these have been obtained a broth or soup (*jus testudinis*), a jelly (*gelatina testudinis*), and a syrup (*syrupus pectoralis testudinum*); all of which were formerly recommended as restoratives and pectorals in phthisical complaints.

The *Chelonia midas* is the green or edible turtle, the "green fat" of which is the delight of the epicure, the glory of the alderman, and an important article of food to the tropical navigator.

Order 2d—*Sauria.*

The Saurian reptiles have an elongated body, a mouth armed with teeth, four legs, and a tail. None are now employed in medicine, though the *Scincus officinalis* was formerly used as an aphrodisiac.

Order 3d—*Ophidia.*

This order has been so called from ὄφὶς, *a serpent.*

Cuvier makes three families of it; namely—

Fam. 1. The *Anguis.*—The animals of this family are similar in several respects to some of the Saurians (lizards), but they are without feet, and have a third eye-lid. The *Anguis fragilis*, usually called the *blind* or *slow worm*, is a native of this country. When handled, it easily breaks, each of the pieces continuing to move for some time afterwards. It is perfectly innocent, though its bite is vulgarly believed to be fatal. The scales of this genus are very small, and have a similar form in all parts of the body.

Fam. 2. *The True Serpents.*—This family contains both venomous and innocent serpents. The *Viper* (*Vipera communis*) may be mentioned as an example of the first, and the *common ringed snake* (*Coluber natrix*) of the second: both are indigenous. The latter is very common: it is ash-coloured, with black marks on its sides, and a white ring (formed of three white spots) on the neck.

Fam. 3. *The Naked Serpents.* — This family includes only one genus—that of *Cæcilia.*

Vipera Communis.

History, &c.—This animal was very anciently known: it is mentioned in the Bible, and by Aristotle. It has received a variety of names: in this country it is usually called the *Adder*, and less frequently *Viper.* By some zoologists it is called *Coluber berus*, by others *Vipera berus.* I have adopted Cuvier's designation.

Description.—The length of the animal is about two feet, varying a little more or less. Its colour is liable to considerable variation, depending on age, sex, climate, locality, &c.: so that mere varieties have been sometimes considered as distinct species. Dr. Leach makes three varieties: —

1. The *black viper*, so called from its colour.

2. The *blue-bellied viper*, in which, while the back has a browner tinge than usual, the belly has a shining blueish-black appearance, somewhat similar to polished steel.

3. The *red viper* is of a bright red colour above, has a heart-shaped mark on the head, and a dark spot near the extremity of the tail.

The head is shorter, broader posteriorly, and flatter than the common snake. Along the back, from the neck to the tail, you observe a black band, usually formed by the junction of rhomboidal figures. This band, although important as distinguishing the animal from the common snake, varies considerably in its appearance, being sometimes continuous, sometimes interrupted. When continuous, it frequently forms a waved or zigzag line; when distinct, the spots may be roundish, oval, rhomboidal, or rectangular. On the top of the head are observed two black lines, which unite anteriorly, but diverge posteriorly, so that we have the initial (V) of the animal's name: these lines are separated by a brown spear-shaped mark. On each side of the body is a stripe of roundish or subtriangular spots. The abdominal scales vary from 142 to 148 (usually they are about 146); those under the tail are from 30 to 40 pairs (generally about 39). The tail is shorter than in snakes.

The *poison apparatus* consists of a gland, (fig. 78, *b*), an excretory tube (*d*), the poison or dental sac (*e*), and the fangs, tusks, or stings (fig. 79). Each of these requires a brief examination.

First, let us notice the *gland.* It is placed behind the orbit, above and in front of the angle of the mouth. It is elongated, somewhat triangular, and slightly flattened, about $3\frac{1}{2}$ lines in length, and at its widest part about 2 lines broad: the apex of the gland is turned forwards, the base backwards. There are two coverings to it, both apparently tendinous: the external one, which seems to be the compressor of the glands (query, should it be regarded as the buccinator muscle?) is attached anteriorly by a tendon to the nostril; posteriorly it is continued to the junction of the two jaws. The internal layer dips down into the gland, so as to divide it into four or five lobes, each of which has a small excretory duct, and is composed of little sacs containing the yellow poisonous matter.

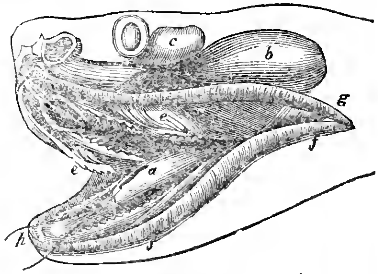

Fig. 78.—*Vipera Communis.*

a, Fissure of the pomum Adami.
b, Poison gland.
c, Lachrymal gland.
d, Excretory duct of the poison gland.
e, Poison sac.
f, Inferior maxillary salivary glands.
g, Superior maxillary salivary glands.
h, Tongue.

The excretory tubes of all the lobes of the gland unite to form one common *excretory duct*, which leaves the gland at its anterior or narrower portion, and passes downwards and forwards to open into the poison bag.

The *dental* or *poison sac* is an oval musculo-cutaneous bag, which projects into the mouth, where it is covered with a callous margin, and is perforated by a slit at the apex. Through this slit passes the poison fangs, which may be regarded as the osseous tubes of the sac.

The *poison fangs*, *tusks*, *teeth*, or *stings*, are sometimes single, sometimes double or treble: they are contained in the poison sac, and are so attached by a ligamentous substance in a hole in the upper jaw, that they are moveable. They are curved, and when not in action, the concavity is directed towards the roof of the mouth; but when the animal is about to bite, the concavity is backwards. From the base to the apex on the convex side of the tooth is a canal formed by a slit.

FIG. 79.—*Fang of Vipera Communis.*

Use of the poison apparatus.—When the animal opens his mouth for the purpose of biting, the elevators of the upper jaw compress the gland; a little of the poison passes along the excretory tube into the dental sac, and from this through the canal of the tooth into the bottom of the wound.

This poison apparatus ought probably to be regarded as part of the salivary organs, the poison gland being analogous to the parotid. In this respect we have an analogy to the poisonous salivary organs of the Arachnida.

Properties of the viper poison.—It has an oily appearance, but in reality is of a gummy nature, and has neither acid nor alkaline properties. It is without taste or odour; is somewhat viscid, and sinks in water, but may be readily mixed with it; it is not coagulable by heat. No chemical analysis of it has been made.

Effects.—The Abbé Fontana made more than 6,000 experiments with this poison. He employed more than 3,000 vipers, and had 4,000 animals bitten. His results, therefore, are worthy of confidence, since they are drawn from such an extended experience. The following is an abstract of some of the most important of them:—

Applied to the tongue, viper poison was neither acrid nor burning: it had, in fact, no determinate taste. When swallowed pure, and in large quantity, it excited a disagreeable, though indescribable, kind of sensation in the mouth, and which continued for five or six hours. On the conjunctival and olfactory membranes it had no irritant or even obvious action. Fontana poured the venom into incisions made with a lancet, but never obtained any evidence that the poison caused pain at the moment of application. It produces no obvious effect on the blood drawn from the body. Injected into the jugular vein of rabbits, the animals screamed violently, and died in convulsions within two minutes: these effects, however, were not uniform, for in some cases the animals survived several hours.

The symptoms produced by the bite of the viper are the following:—Pain in the part, commencing within a few minutes after the bite, augmented by pressure, and extending up the limb. Tumefaction, at first accompanied with paleness, then with redness, and subsequently with lividity and a gangrenous appearance; great hardness of the part. Syncope, or tendency to it; pulse frequent, small, and irregular; respiration difficult; cold sweats; vomiting, with extreme sensibility of stomach; sometimes pain in the umbilical region, and yellowness of the skin; disordered vision; sometimes convulsions, and disturbance of the intellectual faculties. A discharge of dark and subsequently sanious blood takes place from the part; and, when death occurs, gangrene appears. It is not frequently fatal in the human subject.

There are a considerable number of curious circumstances connected with the action of this poison, which I cannot here enter into. Such are, the non-operation of it on certain animals (the cold blooded, as the leech, the viper, &c.); its inactivity when swallowed, even in the human subject; the quantity required to produce death in various animals; the condition of the viper as affecting the result of the bite, &c. &c. These are all interesting topics of inquiry, but I must refer you for an account of them to Fontana's work.

Treatment.—The local treatment consists principally in making pressure around the part. This may be effected by placing a tight ligature around the limb, or by means of a cupping-glass applied to the part. The operation of the latter agent depends simply on the pressure of the edges of the glass, and is independent of the vacuum. Some years since, in conjunction with the late Mr. Ellerby, I performed a number of experiments on the agency of the cupping-glass in pre-

venting the action of certain energetic poisons; and we found that if a wooden ring be substituted for the cupping-glass it was equally efficacious. Where pressure cannot be conveniently made, caustics may be applied, with the view of decomposing the venom. If the parts are already swollen, scarifications may be resorted to.

The general treatment consists in the use of energetic stimulants and sudorifics, especially ammonia. The patient should be placed in bed, and every means taken to promote perspiration, by the use of warm liquids, &c.

AMPHIBIA.

The class *Amphibia* (so called from αμφις, on both sides, and βιος, life) constitutes the fourth order of reptiles, called Batrachia in the Cuvierian arrangement. Their characters are as follows:—The skin is without scales, and hence they have been termed the *Nudipelliferæ*; the heart has only one auricle and one ventricle. In the early period of their existence they respire by branchiæ, like fishes; but subsequently these organs shrink and disappear, while the lungs enlarge and come into action.

None of these animals are employed in medicine. The esculent frog (*Rana esculenta*) is used on the continent as an article of food. Both this and the common frog (*R. temporaria*) were formerly used medicinally. In old pharmacological writers we meet with various preparations of these animals; for example, two kinds of ointment (*Emplastr. de ranis sine et cum mercurio*), and several preparations of the spawn (*sperma ranarum*), such as, a distilled water, oil, plaister, &c.!! The tree frog (*Hyla viridis*, or *Rana arborea*) has also been employed.

PISCES, OR FISHES.

These animals are characterised as follows: the skeleton is for the most part osseous, though in many it always remains cartilaginous or fibro-cartilaginous; and on this is founded the primary division of the class. There is, however, a distinction between their cartilage and that of animals and birds; namely, by boiling in water it cannot be reduced to gelatine. But the most important distinctions between fishes and other vertebrated animals exist in the organs of circulation and respiration. The heart contains only two cavities, and corresponds to the right side of this organ in mammals: thus, the auricle receives the venous blood, and the ventricle propels it through the pulmonary (or rather branchial) artery to the branchiæ. The blood is cold, and its particles are elliptical. The respiratory organs are branchiæ or gills, in which the blood is submitted to the influence of the air, which is dissolved in the water. For the most part fishes are oviparous; but in some (the shark, for example), the eggs are hatched within the body of the mother: these are called ovo-viviparous. Their body is covered with scales; they have no mammæ; and their extremities are converted into fins; whence Blainville designates this class *Pinniferæ*.

Cuvier makes nine orders of fishes, thus:—

1. *Acanthopterygii* (so called from ἄκανθα, a thorn, and πτέρυξ, a fin), of which the perch and mackarel are examples.

2. *Malacopterygii abdominales* (from μαλακος, soft, and πτέρυξ), as salmon, carp, &c.

3. *Malacopterygii subbrachia*, as the flat fish.

4. *Malacopterygii apodes*, containing the eels, &c.

5. *Lophobranchii* (from λόφος, a tuft or crest, and βράγχία, branchia), of which the hippocampus is an example.

6. *Plectognathi* (from πλέκω, to tie or connect, and γνάθος, the jaw), as the porcupine fish.

7. *Sturiones*, or the sturgeons.

8. *Selacii* (from σέλαχος, a kind of cartilaginous fish), as the skate, the torpedo, &c.

9. *Cyclostoma* (from κύκλος, a circle, and στόμα, a mouth), as the lamprey.

These nine orders form two series:—

I. One called *Chondropterygii*, or the *cartilaginous fishes* (from χόνδρος, gristle, and πτερυξ, a fin).

II. The second termed *osseous fishes*.

Here is a tabular view of the arrangement of the orders, taken from Dr. H. Edwards's *Elémens de Zoologie*.

FISHES — ORDERS.

- Skeleton osseous; superior maxilla complete; (*osseous fishes*) ......
  - Superior maxilla moveable ....
    - Branchiæ pectinate ....
      - Anterior dorsal fin with osseous rays; some osseous rays to the anal fin, and ordinarily one to each ventral fin .... *Acanthopterygii.*
      - All the rays soft, except sometimes those of the first dorsal or pectoral fins (*Malacopterygii*)
        - Ventral fins behind the abdomen .... *M. Abdominales.*
        - Ventral fins suspended to the shoulder *M. Subbranchia.*
        - No ventral fins .... *M. Apodes.*
    - Branchiæ disposed in round tufts .... *Lophobranchia.*
  - Superior maxilla united to the cranium .... *Plectognathi.*
- Skeleton cartilaginous; superior maxilla replaced by the palatine bones; (*cartilaginous fishes*, or *Chondropterygii*) ....
  - Branchiæ free at their external edge; one orifice only for each operculum ..... *Sturiones.*
  - Branchiæ adherent at both edges; many branchial openings ....
    - Inferior maxilla moveable .. *Selacia.*
    - Maxillæ united in an immoveable circle......... *Cyclostoma.*

Of these orders the only one I have to notice is *Sturiones*, which contains the *acipenser*, yielding us Isinglass.

Acipenser.

History.—The term Acipenser is applied by modern naturalists to designate a particular genus of cartilaginous fishes, called, in English, sturgeons. But the word is met with in the old writers, and was used by them to indicate a fish much esteemed by the Romans, and which was brought to the tables of the wealthy by decorated servants, with great pomp, adorned with flowers, and accompanied by music. It has not been accurately ascertained what kind of fish was here employed, but it is generally supposed to have been an *Acipenser:* Cuvier thinks the *A. ruthenus*. Macrobius states that this custom was not confined to the Acipenser, but was common to all favourite dishes.

Etymology.—Authors are not agreed as to the etymology, or even the proper mode of spelling the word acipenser, some using one, others two *c*'s. We have, however, the authority of Lucilius and of Martial for using only one. Those who employ two *c*'s adopt the opinion of Sipontin, that the word is derived from *accipiendo*, because this fish is frequently taken at table. I have, however, seen no etymology that carries with it even probability.

Generic characters. — The sturgeons are remarkable for the longitudinal rows of bony tubercles, or plates, with which their bodies are more or less covered. These parts are exceedingly interesting to us in tracing the development of animals, from the lower to the higher tribes. As Dr. Grant has observed, these plates are a remnant of the superficial shells of the invertebrata. The snout of the sturgeon is lengthened and obtuse, and is furnished with four *cirri*, tendrils, or beards. The mouth, which is placed completely beneath, is small, and destitute of teeth.

Species.—The species of acipenser are badly determined. In Brandt and Ratzeburgh's Medizinische Zoologie, they are arranged in three subdivisions, thus:—

A.—*Husones*, including *A. Huso*, and *A. Schypa*.

B.—*Sturiones*, comprehending *A. Sturio*, *A. Lichtensteinii*, *A. Güldenstädtii*, *A. Stellatus*, and *A. Ratzeburgii.*

C.—*Sterleta*, containing *A. Ruthenus.*

Three substances are obtained from this genus; namely, *caviare*, *fish cartilage*, and *isinglass*. The first is prepared from the roe of the *Acipenser Huso*, *A. Güldenstädtii*, *A. Stellatus*, and *A. Ruthenus*. Isinglass is prepared from the swimming bladder of the same species. *Acipenser Sturio* yields a

good caviare, but which is not met with in commerce. I do not think it necessary to enter into a description of the species; those who wish to do so may consult Brandt and Ratzeburgh's work.

The sturgeons are sea fish, but frequent the mouths of large rivers for the purpose of spawning. The great fisheries for them are those of the Caspian. Here is a tabular view of the number of fish caught, with the products, for two years:—

Fisheries of the Caspian.

Year.	Number of Fish taken.			Products.			
	Sturgeon. *A. Sturio (?)*	Sevriouga. *A. Stellatus.*	Bielouga. *A. Ruthenus.*	Caviare.		Isinglass.	
				Pouds.	lbs.	Pouds.	lbs.
1828	43,035	653,164	23,069	34,860	1	1,225	27
1829	68,325	697,714	20,391	28,420	7	1,092	22

This table is taken from a review of the "Official Report on the Fisheries of the Caspian, made to the Minister of Commerce at St. Petersburgh," contained in the "Athenæum" for 1833. I may remark, that it is not improbable the specific names of the fish are not correctly given. The *poud* is equal to about 40 lbs.

Of the swimming bladder of fishes.— As isinglass is prepared from the swimming bladder of the sturgeon, it is necessary I should make a few remarks on this organ in fishes generally.

You are no doubt well aware that the horizontal progression of fishes is effected by the tail, while the processes of turning, stopping, &c. are performed by the fins. But it is necessary that these animals should have the power of rising or sinking in the water; and it is evident that this could only be effected either by enabling the fish to vary his specific gravity, or by means of a great muscular effort. In the great majority of instances nature has adopted the first method; in some cases the second.

In a very considerable number of fishes we find placed immediately under the spine, in the middle of the back, and above the centre of gravity, a membranous sac filled with gaseous matter, and which is denominated the *air bag*, or, from its supposed assistance in the motions of the animal, the *swimming bladder*. In the cod (*Gadus morrhua*) it is denominated the *sound*, and forms an article of food much sought after. In the sturgeons and some other fish, it consists of one bag only; but in carp (*Cyprinus vulgaris*) there are two, one placed anterior to the other, and communicating by a short tube. In most fish this bag communicates with the gullet, or stomach, by a canal or excretory duct, called *ductus pneumaticus*: the length of which is liable to considerable variation; and in some (as the *Salmo thymallus*, or grayling) this canal is altogether wanting. The form, size, and structure, of the swimming bladder, are subject to considerable variation. It is composed of three coats—

1st, An internal, highly vascular coat.

2dly, A tendinous, or, in some, probably a muscular coat.

3dly, It is covered externally, to a greater or less extent, by peritoneum.

In some fish a glandular apparatus exists on the inner side of the bag, which is supposed to be the organ secreting the gaseous matter. Mr. Langstaff has beautifully injected this gland in the conger (*Muræna conger*). In the sword fish (*Xiphias gladius*) the internal structure of the bag is cellular.

The gas contained in the swimming bladder consists for the most part of nitrogen, but mixed with oxygen, and sometimes with carbonic acid. Lacepede asserts he found hydrogen in the air bag of the tench (*Cyprinus tinca*). The relative proportions vary not only in different species, but in animals of the same species, arising probably from the age, size, season, locality, &c. Sometimes there is no oxygen present, or it may be less than one per cent.; in other cases the proportion of this gas is greater than that found in atmospheric air. Thus, in fish residing at considerable depth in the water, there have been found from 69 to 87 per cent. of oxygen. What is the source of these gaseous matters? Evidently they must either be derived from the atmospheric air, or they are the result of secretion. The latter opinion seems to be the more probable, since, in some fish, the air-bag does not communicate with the gullet, and in others

we find a glandular apparatus, supposed to be for the purpose of this secretion.

That one use of this bag is to enable the fish to rise and fall in the water, seems in the highest degree probable. If a few minnows be placed in a glass of water, under the receiver of the air-pump, and the air gradually exhausted, you see the animals rise to the surface of the water, by the expansion of the contents of this bag, and bubbles of air are emitted from the mouth. If the air be then re-admitted into the receiver, the fishes immediately fall to the bottom of the water, and are incapable of rising, except now and then, by a great muscular effort. In the natural state of the animal, therefore, it is presumed that by the compression or dilatation of this sac, effected by the surrounding muscles, the fish is enabled to alter its density, and thus to rise or sink in the water. The flat-fish (as the sole and flounder) are unprovided with an air-bag, and hence they can only rise in the water by a great muscular effort. It has, however, been objected, to this physiology, that eels, which have a swimming bladder, live at the bottom of rivers, while sharks, which roam at all depths, and mackarel, which pursue their prey at the surfaee, have none. Hence some authors have been led to regard this organ as forming part of the respiratory apparatus—that is, as secreting sometimes oxygen, sometimes nitrogen; and, when there is a ductus pneumaticus, of evacuating these gases.

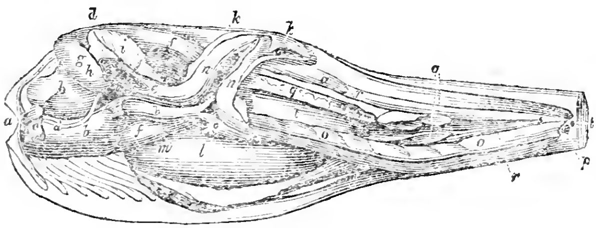

Fig. 80.—*Abdominal Viscera of the A. Ruthenus.*

b, The liver.
c, The gall bladder.
d, The gall duct, opening into
e, The duodenum.
f, The proventriculus.
g, The stomach.
h, The pylorus.
i, The pancreas: the duct of which terminates close to the pylorus.
k, The spleen.
l, The swimming bladder.
m, The ductus pneumaticus.
n and *o*, The intestines.
p, The anus.
q, The ovaries.
r, The oviduct and ureters, forming
s, The common tube, which terminates at *t*.
u, The free opening of the oviduct in the abdominal cavity.

Preparation of isinglass. — The remarks now made on the anatomy and physiology of the swimming bladder, prepare us for the proper comprehension of the nature of *isinglass*; the preparation of which is described, by Mr. Jackson, in the 63d vol. of the Transactions of the Royal Society, and also by Fischer, in a work entitled "Ueber die Schwimmblase der Fische," in 1795. The fish, when taken, are placed on rafts, where they are gutted. The roes are removed, to make caviare; the swimming-bladder, or sounds, are employed to form isinglass, or *ichthyocolla* (so called from ιχθυς, a fish, and κολλα, glue). When cut out, these bladders are slit open, then washed and placed in the air, with the inner lining upwards. The latter is then scraped off, and in this state the sac is dried, and forms the *leaf* isinglass. Sometimes, however, before it is quite dry, it is rolled up and bent like a horse-shoe, or a lyre, or a heart; and is then termed *staple* (of which there are two kinds, *long* and *short*). Sometimes it is folded up into little square packages, forming what is called *book* isinglass. Lastly, there is met with, in commerce, a kind called *purse* isinglass, which seems to be the sac dried unopened. The *pipe* kind is analogous to the purse, but longer. Sometimes it is brought over rolled up like a ribbon. Here are specimens of the different kinds. When brought to London they are picked into shreds. Formerly this was done by women and children; steam power is now used.

Commercial varieties and adulterations. — Independently of the varieties, arising from the mere form which isinglass is made to assume, we meet with various kinds in commerce, differing exceedingly in value; and as the adulteration is principally effected by mixing the inferior (the picked Brazilian leaf, for example) with the finer kinds, it is necessary I should put you on your guard: and here I may remark, that when reduced to the form of shreds, it is impossible to distinguish the finest from the

commonest kinds by the eye merely: this a wholesale dealer admitted to me. The only way of recognizing them is by boiling in water: the best kinds are completely soluble in water, the common kinds only partially so.

The following table will give you some idea of the varieties kept, though I have not inserted all (such as the second and third qualities of some of the sorts). I have thought it best to put the price to each (as given me now by Messrs. Simpson and Humphrey, of Little Britain, London), in order that you may know their relative values.

Form.	Designation.	Price per lb. £.	s.	d.
Staple	Long staple (finest), from Ural	0	13	6
	Short staple (Patriarch), from Astrachan, very scarce and dear.			
	Short staple (finest), ditto	0	14	6
	Samovey short staple	0	6	0
Leaf	Astrachan leaf (finest)	0	13	6
	Beluga leaf	0	11	6
	Brazilian leaf. from Para	0	5	0
	Samovey leaf (finest), from the Caspian	0	4	6
Book	Samovey book (finest) ditto	0	4	6
	Ural book	0	12	0
Purse	Siberian purse	0	9	0
	Hudson's Bay purse	0	7	0
Pipe	Pipe isinglass, from Maranham, Brazils	0	4	6
Ribbon	Ribbon isinglass, New York	0	4	0
Picked	Picked Astrachan leaf ....from 11s. to	0	14	6
	Picked Bazilian leaf	0	6	6
	Pickings (the brown ends)	0	8	6

Chemical properties.—The principal constituent of isinglass is gelatine; and the purity of it is determined by its complete solubility in water. John gives the following as the result of his analysis of isinglass:—

Gelatine	70·0
Osmazome	16·0
Membrane not soluble in water	2·0
Free acid (lactic?) with salts of potash, and soda, and phosphate of lime	4·0
Water	7·0
	100·0

It is probable, however, that this analysis is not quite accurate; for dried flesh, as Berzelius observes, does not contain more than 8 per cent. of osmazome; and if isinglass contained this quantity, we could not keep it dry when exposed to the air, on account of the deliquescent character of osmazome.

Here is a decoction of isinglass: we readily recognize the gelatine in it by the action of infusion of galls on it, and by its non-coagulability by heat and acids. Isinglass jelly is, in fact, the purest form of gelatine we can procure.

Physiological effects.—The local action of gelatine is emollient and demulcent; its remote action nutritive.

Uses.—Medicinally, we employ isinglass jelly dissolved in water or milk, and rendered palatable by a little acid and sugar, as a nutritious substance; or in diseases of the alimentary tube, as an emollient and demulcent.

In pharmacy, a solution of it with a little tincture of benzoin, is employed, brushed over silk, to form court-plaister. It is used also as a clarifying substance for coffee, wines, beer, &c. Some of the constituents (as the colouring matter) of these liquids unite with the gelatine and form insoluble compounds, which precipitate; and, in the act of precipitation, the gelatine incloses within its parts the matters rendering the liquid turbid.

DESCRIPTION OF A MICROSCOPIC ENTOZOON INFESTING THE MUSCLES OF THE HUMAN BODY *.

By Richard Owen, Esq., F.R.S. & Z.S.

Upwards of fifteen distinct kinds of *Entozoa*, or internal parasites, are already known to infest the human body; but none have been found of so minute a size,

* This interesting paper has just been published in the Transactions of the Zoological Society, vol. i. part 4.

or existing in such astonishing numbers, as the species about to be described.

The body of an Italian, æt. 50, who had died in St. Bartholomew's Hospital, was brought into the dissecting-room, and it was observed by Mr. Paget, an intelligent student, that the muscles presented an uncommon appearance, being beset with minute whitish specks. This condition of the muscles had been more than once noticed by my friend Mr. Wormald, the Demonstrator of Anatomy, in subjects dissected at St. Bartholomew's during previous anatomical seasons. His attention had been especially called to it on account of a gritty sensation sometimes perceived in dissection, from which circumstance, and the rapid blunting of the scalpels employed, he was induced to consider the appearance as being caused by a deposition of specks of earthy matter. Mr. Wormald having acquainted me with this fact, I expressed a desire to be furnished with portions of muscle so affected, and through my friend's prompt attention, I soon received ample materials for microscopical examination from the subject above mentioned*.

With a magnifying power of an inch focus the white specks in the muscle are seen to be cysts of an elliptical figure, with the extremities in general attenuated, elongated, and more opaque than the body (or intermediate part) of the cyst, which is, in general, sufficiently transparent to shew that it contains a minute coiled-up worm. On separating the muscular fasciculi the cysts are found to adhere to the surrounding cellular substance by the whole of their external surface, somewhat laxly at the middle dilated part, but more strongly by means of their elongated extremities, so as to render it generally a matter of some difficulty to detach them. When placed upon the micrometer they measure $\frac{1}{50}$ of an inch in their longitudinal, and $\frac{1}{100}$ of an inch in their transverse diameter; a few being somewhat larger, and others diminishing in size to about one half the above dimensions. They are generally placed in single rows, parallel to the muscular fibres, at distances varying from half a line to a line apart from one another; but sometimes a larger and a smaller cyst are seen attached together by one of their extremities, and they are occasionally observed slightly overlapping each other. If a thin portion of muscle be dried and placed in Canada balsam, between a plate of glass and a plate of talc, the cysts become more transparent, and allow of the contained coiled-up worm being more plainly seen.

Under a lens of the focus of half an inch the worm appears to be inclosed within a circumscribed space of a less elongated and more regular elliptical form than the external cyst, as if within a smaller cyst contained in the larger, like the yolk of an egg surrounded by its albumen and shell. The worm does not occupy more than a third part of the inner space. A few of these cysts have been seen to contain two distinct worms; and Dr. A. Farre, who has paid much attention to the subject, has shown me a drawing which he made of one of the cysts containing three distinct worms, all of nearly equal size.

The cysts vary in form as well as size, being more or less elongated, and the opaque extremities being further extended in some than in others: in a few instances only one of the extremities is thus produced. Occasionally the tip of one of the extremities is observed to be dilated and transparent, as though a portion of the larger cyst were about to be separated by a process of gemmation; and these small attached cysts are seen of different sizes, as it were in different stages of growth. This appearance, however, I conceive to be explicable without a reference of a power of independent vitality to either of the adherent cysts.

Besides size and figure, the cysts also differ in structure: in general they are composed of condensed and compacted lamellæ of cellular tissue, but a few are hardened by the deposition of some earthy salts, so as to resist the knife, and to break with a gritty sensation under pressure*.

In order to detach the worm from the cyst, which, from the minuteness of the object is a matter of some difficulty, I have found it best to select a portion of muscle which has been placed for a short time in spirits of wine. After separating the cysts from the surrounding fasciculi of muscle, and placing them, moistened with a little water, on a slip of glass, I have generally succeeded, on cutting off the end of the cyst, or tearing it open with the point of a needle, in ejecting the worm and the surrounding fluid in which it floats, by gently pressing on the cyst.

The little worm is usually disposed in two or two and a half spiral coils: when straightened it measures from $\frac{1}{25}$th to $\frac{1}{30}$th of an inch in length, and from $\frac{1}{700}$th to $\frac{1}{800}$th of an inch in diameter: a high magnifying power is consequently required for its examination. It is cylindrical and filiform, terminating obtusely at both extremities, which are of unequal sizes, tapering towards one end for about a fifth part of its

* The existence of the Entozoon was at the same time satisfactorily determined by Mr. Paget, with the assistance of Mr. Brown and Mr. John Bennett, at the British Museum.

* This change is probably dependent on the death of the inclosed worm, the traces of which are either very obscure, or altogether wanting, in these ossified cysts.

length, but continuing of uniform diameter from that point to the opposite extremity. It is only at the larger extremity that I have been able to distinguish an indication of an orifice; but this indication has been so constant in a number of individuals, examined under every variety of circumstance, that I have no hesitation in ascribing a large transverse linear mouth to the great extremity, which I therefore consider as the head.

A recently extracted worm, seen by a Wollaston's doublet before any evaporation of the surrounding moisture has affected its integument, presents a smooth transparent exterior skin inclosing a fine granular and flaky substance or parenchyma; and after carefully testing various appearances of more complex organization, that have on different examinations presented themselves, I now believe that the only structure that can safely be ascribed to this minute *Entozoon* is the simple one above described. It is not of a rigid texture, but is extremely fragile, and exhibits when uncoiled a tendency to return in some degree to its former state.

It is curious to watch the variety of deceptive appearances that successively present themselves as the worm dries by evaporation. One of the most constant is a succession of minute transverse rugæ, especially at the concave sides of the coils, which give a finely annulated character to the worm, but of which no trace can be perceived in the plump recent specimens when observed by a good doublet. Another appearance, which is more difficult to be accounted for, results from one, and sometimes two, longitudinal lines, extending over a greater or less proportion of the body; but these are not to be perceived in worms examined under circumstances least liable to cause deception. As evaporation proceeds, the wrinkling of the integument produces an appearance of the body being occupied by minute tortuous tubes, and a beautiful microscopical effect is thus obtained; but the fallacy of this appearance and its true cause are easily detected.

The test of coloured food could not be applied to elucidate the form of the digestive organs in the present instance: there was not any indication of the polygastric structure; which, indeed, was hardly to be expected, since it does not exist in those *Entozooid Infusoria* which most nearly resemble the parasitic species in question. There was no appearance of the parietes of an alimentary canal floating in a visceral cavity, and distinct from the integument of the body, as in the higher organized *Nematoid Entozoa*; nor could a trace of an orifice, or anus, be observed at the smaller extremity. I have been equally unable to detect a projecting spiculum, or hook, at either extremity, or any appearance of the worm having been torn from an attached cyst. The natural transparency of this species is such as not to admit of a doubt as to its wanting the ovarian and seminal tubes, and other characteristics of the complicated structure of *Filaria, Ascaris*, and *Nematoid Entozoa* generally.

Three species of small *Nematoid Worms* are described by Zeder as inclosed in cysts or capsules, and hence are termed by him *Capsularia*. Rudolphi, however, whose authority on this subject cannot be lightly disregarded, does not sanction or admit this genus in his "Systema Entozoorum," but refers the three species described by Zeder to the genera *Filaria* and *Ascaris*. The *Capsularia halesis*, or *Filaria capsularia* of Rudolphi, infests the abdominal viscera of the herring, and measures from half an inch to an inch in length: the intestinal canal is distinct, and is dilated at one extremity into a stomach. In the males the intromittent spiculum protrudes from the anal extremity, which is the largest. The *Capsularia salaris* and *Capsularia trinidosa* of Zeder, represent, according to Rudolphi, a single species of *Ascaris* (*Asc. Capsularia*, Rud.). They are about an inch in length, and are inclosed in a spiral form, in cysts attached to the peritoneum of the salmon. The *Capsularia halecis* figured by Zeder* exhibits a straight alimentary canal and longitudinal lines, probably nervous filaments, which resemble those lines observable under certain circumstances in the present microscopic species; but no further correspondence in internal structure can be traced between them.

The circumstance of being inclosed in cysts is common to many very differently organized genera of *Entozoa*. There are few, indeed, with the exception of those which live upon the mucous surfaces of the body, that do not, by exciting the adhesive inflammation, become inclosed within an adventitious cyst of condensed cellular substance analogous to the galls produced by the irritation of larvæ developed in the substance of a living vegetable.

The simple type of structure, which the minute animal here described exhibits, approximates it to the lower organized groups of the *Vers Intestinaux Parenchymateux* of Cuvier: and both from its locality and the constancy of the cyst inclosing it, it manifests a relation of analogy to the order *Cystica* of Rudolphi. From all the genera of this order, however, it differs in the want of the complex armature of the head and of the dilated vesicle of the tail. At first sight it might seem indicative of an annectant group, which would complete the circular arrangement of the *Entozoa*, by

* Naturgeschichte der Eingeweidewürmer, tab. i. figs. 3, 4, 5.

combining the form of the *Filariæ* of the first with some of the characteristics of the *Cysticerci* of the last, of Rudolphi's orders. Unfortunately, however, the class *Entozoa*, as it now stands, is so constituted that an animal may be referred to it without much real or available knowledge of its organization being thereby afforded: it embraces animals with the molecular and animals with the filiform conditions of the nervous system; conditions which are accompanied by different types of the digestive system, and which indicate not merely differences of class, but of primary division in the animal kingdom.

The organic form in the natural system, to which I consider the animal under consideration as being most nearly allied, is that exhibited by the lower organized *Vibriones* of Müller, and of which Ehrenberg has composed his genera *Vibrio*, *Spirillum*, and *Bacterium*: so that the present species may be regarded as affording, with the seminal *Cercariæ*, a second example from the lowest class of the animal kingdom having its *habitat* in the interior of living animal bodies. Referring it, however, provisionally, to the class of *Entozoa* of Rudolphi, in which it would indicate a new order, its generic character may be thus given:

Genus Trichina.

Animal pellucidum, filiforme, teres, posticè attenuatum: os lineare; anus nullus; tubus intestinalis genitaliaque inconspicui. (*In vesicâ externâ, celluloсâ, elasticâ, plerumque solitarium.*)

Trichina spiralis.

Trich. minutissima, spiraliter rarò flexuosè incurva; capite obtuso; collo nullo; caudâ attenuatâ obtusâ. (Vesicâ externâ elliptica, extremitatibus plerumque attenuatis elongatis.)

Hab. in *Hominis* musculis (præter involuntarios) per totum corpus diffusa, creberrima.

With respect to the case in which this singular parasite has been met with, Dr. Roupell, Physician to St. Bartholomew's Hospital, has obligingly forwarded to me the following notes:—

"Paolo Bianchi, an Italian, by trade a barometer-maker, about 50 years of age, of a sallow complexion, with black hair and eyes, was admitted under my care on the 4th of December, 1834. When admitted he was much emaciated and weak, his countenance was haggard, and his look depressed. His legs were œdematous; his urine contained albumen, was sweet, and when evaporated yielded a residue like treacle: he had pain in the back. His appetite was deranged, and his liver was felt beyond its natural limits. He had cough, but without urgent distress or hurry in the breathing or expectoration. On auscultation pectoriloquy was detected in the upper part of the lungs: his bowels were relaxed. The general treatment was to give him strength by tonic and sedative medicines, with a nutritious but not stimulating diet, and leeches were applied to the loins: for a time he appeared to gain ground, the œdema disappeared, and he gained some strength, being able to get out of bed and dress himself. But his appetite rather suddenly failed him; his diarrhœa increased; his abdomen became tense and painful; his stools passed unconsciously, and contained blood; and having received extreme unction from his priest, he died on the 29th of January, 1835, in a state of extreme debility and emaciation. There had not been observed any eruption on the skin, or any greater loss of muscular power than related to the debility caused by the disease of which he died.

"He was examined thirty-six hours after death. Tuberculous cavities were found in the upper lobes of the lungs on both sides, and specks of tubercles in both. The kidneys presented in a marked degree the change described by Dr. Bright. The liver was enlarged and fatty. The mucous membrane of the small intestines was ulcerated to a great extent."

About a fortnight after the dissection of the above subject, a second was brought into the rooms, similarly affected; respecting which Mr. Paget, who first noticed the parasites in the Italian, has favoured me with the following note:—

"The second body was that of a poor Irishwoman, who had been in Mr. Lawrence's ward for six weeks. She had died in a state of extreme emaciation, produced by a large sloughing ulcer just below the knee, by which a considerable portion of the head of the tibia had been exposed. She had had occasional severe diarrhœa, and obstinate vomiting."

As regards the seat of the *Trichinæ*, they occur in all the voluntary muscles, and in those which have been termed semi-voluntary or respiratory, as the diaphragm. My friend Mr. Wormald examined and detected them in the minute muscles of the tympanum; as many as twenty-five were lodged in the tensor tympani. I could perceive no trace of them in portions of the muscular coat of the small or large intestines; neither could I detect any in the detrusor urinæ, or in the substance of the heart.

A portion of the muscle of the first subject which was sent to me being in a state of incipient putrescence, I preserved it in

spirit of wine for three days before examining it; yet after macerating a small portion in water, and separating the cysts, the worms when pressed out continued, to my surprise, to exhibit motions, which, though languid, were sufficiently evident, tightening and dilating their coils. I suppose that, being buried in the flesh and defended by the dense exterior cyst, the spirit had not penetrated so as to act sufficiently upon them to destroy their vitality; for on adding a drop of alcohol to the expressed worms, and afterwards moistening them with water, the motions of coiling and uncoiling ceased. More languid motions than those above described were afterwards noticed by Mr. Wormald and myself in some specimens that were examined a fortnight after the death of the subject infested by them; but it is difficult whether to refer these to hygrometrical influence or to irritability.

The tenacity of life or irritability manifested by these low organized *Invertebrata*, has attracted the attention of almost every entozoologist. Rudolphi especially takes notice of the power which the *Entozoa* possess of resisting the deadly effect of ardent spirits*, and relates many other singular instances of their tenacity of life, of which not the least remarkable is that which is manifested by the *Filaria capsularia* before referred to. When the hard-frozen herrings which are sent packed up in ice to Berlin are thawed for use, these *Filariæ* or *Capsulariæ* revive and exhibit lively motions†. The same remarkable property is occasionally forced upon the notice of individuals not immediately engaged in physiological investigations. It recently happened that two medical gentlemen, having sat down to partake of a cod's head and shoulders, were disagreeably interrupted in their repast by the appearance of a large lively round worm, which on the first cut into the fish escaped therefrom, and began to coil and uncoil itself on the edge of the dish. Now this worm must have been submitted to the temperature of boiling water for at least half an hour, and the *Entozoa* would thus appear to endure with impunity extremes alike of cold and heat

With respect to the cyst of *Trichina spiralis*, I was at first inclined, from the prevailing regularity of its figure, to believe it to be the *Entozoon* itself, or a part of the *Entozoon* analogous to the dilated tail of the *Cysticerci*. Mr. Hilton, Demonstrator of Anatomy at Guy's Hospital, who appears to have first recorded this affection of the human muscles *, ascribed it to the presence of a minute species of *Cysticercus*, not being aware of the existence of the animal to which the presence of the cysts in question is owing. The difference however, between the parasitic animals under consideration and the *Cysticerci*, is at once obvious; the true *Cysticerci* are always inclosed within an adventitious cyst of cellular membrane, in which the hydatid either freely floats, or at most adheres to the inner surface by the mouth only; whereas the present cysts, besides the absence of the peculiar structure and pearly subtransparency which characterize the true hydatid, adhere to the surrounding parts by the whole of their exterior, which is covered by a cellular flocculency.

But admitting the similarity of the outer cyst of *Trichina* to the outer adventitious cyst of *Cysticercus*, it may be contended that the inner cyst is part of the organization of the inclosed worm. Its analogy to the second cyst of the genus *Anthocephalus*, within which the elongated body of that worm is seen, readily occurs, but will not hold good on a close examination. The elongated body of *Anthocephalus* is always found in organical connexion with the second cyst; and Rudolphi observes, that the point of continuity is indicated externally by a depression occasioned by the inversion of the body at that part. In the *Cysticerci* a similar appearance is frequently observed, from the inversion of the head and body within the terminal cyst; and in the *Cœnuri*, where the corresponding bladder is common to many armed heads, some of these are generally found inverted, while others are projecting externally. In all these cases, however, besides the difference of structure between the second and outer cyst, they are always perfectly distinct from each other, and readily separable. But I have never been able to effect a corresponding separation between the outer and supposed inner cyst of *Trichina*, or to demonstrate satisfactorily the existence of the latter as a distinct part: it appears to be a layer only of the external cyst, which, as is often seen in cysts of corresponding structure not formed by hydatids†, is more or less detached from the outer layer.

In almost every instance in which I have succeeded in opening the cyst with-

* Synopsis Entozoorum, p. 595.

† "In Harengas congelatas rigidas et glacie textas frigida affusa reviviscere viderim."—Hist. Entoz., tom. ii. p. 62.

* See Medical Gazette for February 2, 1833, p. 605. In the letter from Mr. Hilton to Thomas Bell, Esq. which the latter distinguished naturalist has kindly communicated to me. It is stated that three subjects, with the muscles similarly affected, have been brought to the dissecting-room at Guy's Hospital during the present season (1834-5).

† This separation of cysts alternating with a secretion of fluid, is the cause of the ***pill-box hydatid*** of Mr. Hunter, which is a distinct animal, or true *Entozoon*.

out injury to the worm within, it has been expelled entire, together with the fluid matter surrounding it, by pressure upon the cyst. Occasionally, however, a part of the worm remains adherent; but this has been accompanied with a glairy adhesive state of the fluid secretion of the cyst, and has been, I believe, dependent upon it; for when the broken pieces have been extracted and examined with a high power, both extremities have presented the same entire surface and uniform rounded appearance as in the worms which are extracted whole.

The structure and relations of the cyst, therefore, and the absence of all organical connexion between it and the contained worm, lead to the conclusion that the cyst is adventitious, foreign to the *Entozoon*, and composed of the cellular substance of the body infested, morbidly altered by the irritation of the worm.

From the tenacity of irritability manifested by the *Trichina* under circumstances so opposite to those under which it was developed, from its small size compared with the cavity of the cyst, and from the quantity of fluid in which it is immersed, it is highly probable that in its natural condition it enjoys active powers of motion. If in such movements the extremities of the worm were repeatedly pressed against the surrounding capsule, this would yield and become elongated in the directions where there was least resistance; viz. where the muscular fasciculi would most readily separate: and observation shows that it is in the direction of the fasciculi that the cysts are elongated. If the germ of a *Trichina*, or a portion of the worm separated by spontaneous fission, be deposited at the end of one of the elongated axes of the cyst, it might, in the process of development, excite the adhesive inflammation, which would then cut off the communication between the smaller cyst and that of the parent worm, while the former would be stimulated to secrete from its inner surface a serous fluid, and so go on enlarging in size, through the influence of the same causes as occasioned the formation of the cyst of the parent. Smaller cysts, of different sizes, are occasionally seen thus attached to one end of larger cysts, and I am inclined to account in this manner for their formation.

Cysts filled with opaque matter are also occasionally seen. In these the worm may have perished, or its germ, after exciting the cyst to secrete, may not have been developed, and the enlargement of the cyst may be occasioned by the accumulation of its own secretion. But these appearances are not sufficient to establish the independent vitality or existence of the cyst, in opposition to those analogies which so plainly point out its true nature and origin.

I have seen, in two pieces of the diseased muscle, groups of minute oblong vesicles, about $\frac{1}{300}$ part of an inch in length; and these may, by possibility, be germs of the *Trichina*: they are pellucid, and without internal spot or other structure.

Although the parasites which have been described are of such minute size, their number is so immense, and their distribution throughout the muscular system so extensive, that they must occasion debility from the quantity of nutriment required for their support. It is satisfactory, however, to believe, and the history of the two cases which have afforded the materials of the present communication encourages the belief, that the *Trichinæ* are productive of no other consequence than debility of the muscular system; and it may be questioned how far they can be considered as a primary cause of debility, since an enfeebled state of the vital powers is the probable condition under which they are originally developed. No painful or inconvenient symptoms were present to lead the medical attendants to suspect the condition of the muscular system which dissection afterwards disclosed; and it is not improbable that in all cases the patient himself will be unconscious of the presence of the microscopic parasites which are enjoying their vitality at his expense.

An inspection of the muscles of recently amputated limbs might afford the opportunity of examining this interesting species under peculiarly favourable circumstances; and the occurrence of two cases in the same dissecting-room within so short a period of each other, with the recollection of similar appearances being not unfrequently present in subjects dissected in the same establishment, render it highly probable that a sufficient number of observations will soon occur to elucidate this curious disease in all its relations.

It is one, and by no means the least important benefit of the present system of providing subjects for anatomical purposes, that the histories of the uncommon appearances which may present themselves can be traced, and the circumstances to which the appearances relate be accurately determined. Many an interesting pathological condition has been wholly lost to science from the want of such regulations as are now in operation; and it is not unreasonable to suppose that the unfavourable condition in which subjects were formerly for the most part obtained, may have contributed to prevent due attention being paid to the appearance which has been described, and which results from so singular and unexpected a cause.—Feb. 24, 1835.

Appendix.—At this early period of the first anatomical season which has commenced since the preceding description of the *Trichina spiralis* was written, another example has occurred in the dissecting-rooms of St. Bartholomew's Hospital of a male subject, with the muscular system similarly infested with this most interesting and remarkable parasite. Its numbers exceed, if possible, those in the cases already mentioned, every part of the voluntary muscles teeming with the minute white cysts in which the worm is contained. These cysts differ from those I have before examined, in being more opaque and gritty, so that the presence of the contained worm would not be suspected from a simple examination of their exterior, and it is probable therefore that the cysts first described in the Medical Gazette were in this state. The examples in which two worms are present in one cyst, are more common in this case than the preceding. The *Trichinæ* when extracted were more lively than I have ever seen them; and in every instance they have presented an opaque or dark-coloured spot, about one-fifth of the length of the body from the anterior extremity. On breaking across the recently extracted specimens, I have observed in several a retraction of the outer skin, leaving the substance which it envelopes protruding.

Dr. A. Farre, who has observed the same appearances in several of the *Trichinæ* of the present subject, is of opinion that the projecting substance is the alimentary canal*. I have not as yet been able to satisfy myself of its tubular structure. In one case the dark-coloured spot formed part of the protruded string. Is this body the ovary? If it be determined that there is an alimentary canal contained within, and distinct from the outer skin, then the *Trichina* would rank higher in the scale than I have placed it, and form a genus of the *Cœlelmintha*. I have not, however, in any instance seen a trace of an excretory or anal orifice.

Nov. 18, 1835.

FUNCTIONS OF TASTE AND COMMON SENSATION IN THE TONGUE.

To the Editor of the Medical Gazette.

Sir,

I should have replied at an earlier period to the polite inquiry of "A constant Reader of the Medical Gazette," relative to the condition of the olfactory function in the case of Davis, but that I was in expectation of being enabled by the delay to afford a satisfactory answer.

I have to charge myself with an undue omission in making no special inquiry, in this instance, as to the precise condition of the sense of smell, having mainly interested myself in the case with the intention of ascertaining the state of the sensibility of the tongue. On the appearance, however, of your correspondent's inquiry, I hoped to have again met with the patient; but as the man is not under medical treatment at this time, and as, from a variety of circumstances, some time may elapse before an opportunity may occur of again seeing him, I will, for the present, simply state, that Mr. Ker, whose patient he was, informs me that, from a general examination, he considered the sense of smell to be materially diminished, but not destroyed; and this on the paralysed side only: but as few trials were made with the express view of determining this point, he does not wish me to rely upon the statement as being of undoubted accuracy. If, however, on some future occasion, I should have the opportunity of determining the matter by a closer scrutiny, and if the result should differ from what has here been stated, it shall, if deemed of sufficient importance, be communicated to the profession through the medium of your excellent journal.

Whilst upon this subject, if I do not unduly trespass, I beg to offer a few remarks on the postcript appended by Mr. Bishop to the account of his very interesting case of paralysis, published in the last week's number of the Medical Gazette. In this case, because there was coincident abolition of taste and common feeling in the tongue, Mr. Bishop considers that there is a non-accordance between this fact and my views upon the same subject. Now, sir, on the two occasions that I have appeared before the profession, in connexion with the subject, I have cautiously abstained from expressing any mere *views*, but have confined myself to the facts, and the suggestion of what appeared to be the legitimate induction; and cases having been observed with most rigid minuteness, manifesting, in one instance, loss of feeling with maintenance of taste, and in the other, loss of taste with maintenance of feeling,

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* See Dr. A. Farre's paper, in our last number but one, p. 382.—E. G.

the inference in my mind seemed to be inevitable that the two functions are distinct; and that, from analogy, we must look for a separate nervous supply. I believe it was from similar pathological facts that the probability of the compound nature of the spinal nerves was first inferred.

With all due deference to Mr. Bishop, I must suggest that because his case is not *corroborative* of mine, it is not therefore *non-accordant* with it. Cases of cutaneous paralysis of two functions in the same structure may occur without at all interfering with the doctrine of their separateness. When in the limbs, for example, we observe simultaneous abolition of sensation and voluntary motion, we do not, on that account, revert to ancient notions, but regard the paralysis as affecting the nervous structure in connexion with each of the functions; and why not adopt the same course in those cases where coincident abolition of taste and common feeling is witnessed?—I am, sir,

Your obedient servant,

DANIEL NOBLE.

Manchester, Dec. 17, 1835.

CASE OF EXTREME CONTRACTION

OF THE

AORTIC AND LEFT AURICULO-VENTRICULAR VALVES;

With its Effect upon the Arrière Circulation.

BY MARSHALL HALL, M.D. F.R.S. &c.

THE following case illustrates the effect of extreme contraction of the aortic and left auriculo-ventricular valves, upon the *arrière* circulation in the lungs, the liver, the intestines, &c., and upon the pulse. I think these have not been so well observed before, and that the case will not prove uninteresting to the readers of the Medical Gazette:—

Mr. C——, aged 63, a barrister, called on me on the 10th of September, 1835. He had returned from the Circuit, during which his friends had observed his altered appearance. I was struck with his *breathlessness*, small, indistinct pulse, pallor, thinness, &c. I appointed to see him at home.

On the next day I saw Mr. C. at his own house. There were breathlessness on the slightest exertion, augmented impulse of the heart, without either distinct second sound, or *bruit de scie*, slight *anasarca*, and slight *icterus*.

The progress of the case was rapid. The breathlessness became urgent; there was a distinct rattle over the posterior right side of the thorax; the left ventricle beat rapidly, with considerable impulse, and without distinct second sound, or bruit; there was some cough, distinct icterus, and augmented anasarca. The jugular veins were turgid; the pulse was small, irregular, indistinct.

To these symptoms hæmoptysis succeeded. The only position which could be sustained was the erect. The cough was troublesome. The breathlessness, the rattle on the right posterior side of the thorax, the rapid forcible beat of the heart without second sound or bruit, the small, indistinct pulse, the icterus, the anasarca, continued, with occasional sickness.

Gradually the cheeks became cool, the beat of the heart less forcible, the pulse less indistinct, the posture less raised, the extremities cold and clammy, and the patient sank very slowly during several days.

Examination.—The organs were examined thirty-six hours after death, on September 29th, 1835.

There was slight icterus and anasarca.

The head was not examined.

The thorax.—The *left* cavity of the pleura contained one pint of sero-sanguineous fluid.

The costal pleura was very vascular; there were no adhesions, except between two contiguous portions of the lung, and of this to the pericardium. The *right* cavity of the pleura was obliterated by adhesions.

The *trachea* and bronchia were filled by frothy bloody mucus. The bronchial tubes were dilated, and their lining membrane redder than natural.

Both lungs, but especially the right, were gorged with bloody fluid, so that only the upper portion gave the healthy crepitus on pressure between the fingers. A portion of the lower lobe of the *right* lung presented a circumscribed apoplexy, of the size of an egg. Similar but smaller apoplexies were found in the middle lobe, and in the upper lobe of the *left* lung.

The two layers of pericardium adhered by means of coagulable lymph, which admitted of being torn, and stripped of the pericardium. This membrane was vascular within; and, on its exterior surface, loaded with adeps and serum. The *heart* was considerably enlarged: the *right auricle* and ventricle were dilated and thickened; the auriculo-ventricular and pulmonary valves free from disease; the pulmonary arteries and their branches appeared enlarged: the *left auricle* was much dilated and hypertrophied; the auriculo-ventricular valve was very much thickened, of the firmness of cartilage, and admitted one finger only; the *left ventricle* was slightly enlarged and hypertrophied. The *aortic* valves were ossified and rigidly immoveable, and their orifice so contracted as not to admit the little finger.

The abdomen.—The peritoneal cavity contained no fluid. The *liver* was small, and its surface granulated. It was shewn to Mr. Kiernan, who stated that it was in the second stage of hepatic-venous congestion. The gall bladder was full of dark-coloured bile; its ducts free.

The peritonæum covering the intestines was deeply congested. The intestines themselves, from the middle of the jejunum to the rectum, were highly congested — the valvulæ conniventes being of a deep purple hue, and presenting numerous small patches of ecchymosis. The spleen, pancreas, kidney, &c. were healthy.

It is impossible that morbid appearances should follow in a more distinct order, or account more lucidly for the symptoms during life. I know of no case on record so illustrative of the effect of obstruction of the circulation, upon the *arrière* part of that circulation.

The breathlessness is accounted for by the condition of the valves of the aorta and left auriculo-ventricular valve. The smallness and indistinctness of the pulse by the former. The turgid jugulars by the impeded circulation, propagated from the lungs to the right side of the heart.

The impeded flow of the blood through the aortic and mitral valves led to *congestion* in the lungs, and this amounted to such a degree as became true "*apoplexie pulmonaire;*" in consequence of this impeded circulation in the lungs, we have congestion of the hepatic vein in its second stage; as a further consequence of hepatic-venous congestion we have congestion of the vena portæ, and of the intestine, so remarkable on the post-mortem examination.

The congested state of the liver led to the icterus, and to the hæmorrhagic state of the intestine. That of the vena cava to the anasarca.

It is impossible to imagine a *series* of phenomena more distinctly connected: the smallness of the pulse with the contracted aortic valve; the congested lung, with impeded circulation through the left side of the heart generally; the impeded flow of blood through the right side of the heart, and the turgid jugulars, with the congested lung; the congested hepatic vein, with the impeded flow of blood through the right side of the heart; the progressive, though *arrière* congestion of the vena portæ, with icterus, and of its *roots* in the intestine, with that of the hepatic vein.

IODINE—SALIVATION.

To the Editor of the Medical Gazette.

Sir,

You will perhaps consider the following note hardly worthy of a place in your columns; yet facts, in practical medicine, are always valuable, especially when they throw light upon the physiological action of remedies. In a postscript to his communication in your last number, Mr. Winslow has recorded an instance of salivation occurring during the exhibition of iodine. The same effect was produced in a patient of mine about a month ago. She had been taking the tincture of iodine for five weeks on account of a structural disease of the liver, and her dose was then about fifteen drops of Coindet's alcoholic solution three times a day. At this period it also excited headache, and slight gastric irritation.—I am, sir,

Your obedient servant,
George E. Ely, M.D.

Chatham, Dec. 16, 1835.

P.S. It is deserving of notice that mercury had been freely exhibited in the above case without producing the slightest effect upon the mouth.

MEDICAL GAZETTE.

Saturday, December 26, 1835.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MODEST DEMANDS OF ASSURANCE COMPANIES.

MEDICAL men are surely either very great simpletons, or among the most disinterested and generous of mankind. The fact is notorious, and has long been acknowledged, that no people on earth work harder for the most inconsiderable remuneration—a remuneration bearing no proportion, (at least, compared with what accrues from other professions), to the expenses of their education and outfit. Here are no high places for ambition to soar to. When a man enters into the medical profession, his future position in life is pretty nearly determined: his path is limited; in one sense it is up-hill; and although the end and aim be not distant, it is but too often destined never to be attained.

But medical men rarely think of these things,—though surely the time is come when considerations so nearly connected with their interests ought not wholly to escape them. They see, or may if they please see, the more worldly-wise in every direction availing themselves of *their* services, and profiting by *their* singularly unwise inattention. No newly founded institution, raised for the purposes of convenient charity, ever now dreams of endowing its medical appointments with salaries; and the few old ones, that happened originally to have made a more liberal arrangement, are gradually adopting the new plan. They can find medical men enough, it appears, to fill the vacant places; and pay being no object with our professional brethren, the charitable people quietly put up their purses and say nothing.

The new Poor Law Commissioners go more boldly to work; they offer us alms; they treat us as paupers who desire to have charge of paupers; they threaten, they bully, and are often successful by these means in procuring the cheap, however ineffective, medical attendance which they require.

This, however, is less provoking after all, than the sly, selfish, conduct of assurance companies towards the profession. They have long been in the habit of worming out of medical men information of the greatest importance for their commercial schemes; and what with forms of courtesy at first, and the few applications made, it was not then worth disputing about. But their business has increased largely of late years; more numerous policies are effected at each office than ever before, notwithstanding the competition of various companies for the public favour. So far, however, is this from prompting them to the exercise of common justice, leaving generosity out of the question, that they even wax haughty; they demand that as a sort of right which they hitherto obtained from our indulgence; menaces, moreover, are plainly hinted; what we shall come to next, we can scarce conjecture. A party wishes to have his life insured—he submits to the freest and fairest examination that can be given him by the medical officers of the company—and he is willing to abide by their decision. But the managers want further security, and procuring the address of some medical acquaintance of the applicant, write to this professional man a string of interrogatories "strictly confidential:" they demand categorical replies—under the sanction of a very intelligible threat that the letter will be filed, or, in other words, that it will be preserved for future use at the respondent's peril.

Now what do we owe these people, that they should take this liberty with us? Luckily nothing; and it is perhaps the more fortunate that it is so, inasmuch as the numerous disclosures made of late regarding the transactions of some of these companies render it desirable that they should have as few official claims upon us as possible. There should be no more *courtesy* with these personages: *business* should be the sole agent to bring us in contact with them.

Some there are who have doubts whether in insurance cases the medical referee ought to be paid by the applicant, or by the office. We think these doubts ought to vanish upon putting a few simple questions. Who makes the application to us? Who has the most immediate interest in our professional testimony? Who cautions us—nay, threatens—implying that without some such strong hint we might certify falsely? Who, in short, employs us *confidentially*—meaning thereby that we should consult the interest of the office, at least as much as that of our patient? Need we append, then, another question—who ought in such circumstances to pay us for our time, trouble, and the responsibility imposed?

As a general position, therefore, we hold that the office should pay. It is idle to tell us that it is unreasonable to expect the directors to pay both their own medical officer and the referee of the person to be insured: if the information they seek be not worth a fee, they ought not to take the pains of applying for it; but we know that they think far otherwise, judging from their importunity; and it can only be attributed to a mean and selfish spirit that they seek in so ungenerous a manner to compass their ends. Nor let them plead their old-established usages: they have played at that game long enough at our expense; and if we only turn the tables on them now, as we ought to do—if we only withhold our certificates from them till we are properly treated, we shall have effected a reform in their usages and customs which in the end may be found to be perhaps the most beneficial for all parties.

It is not true, as some have asserted, that the question of fee is entirely a private affair between the applicant and his medical attendant. We deny it—not only for the reasons already stated, which prove that it is much more like a private affair between the office and the medical referee, but because, in very numerous instances, no such private relation subsists between the party about to insure and the medical man to whom he refers. How many proceed to insure their lives who do not happen to reckon in their list of friends any medical man? How many, also, who have not been visited for months, nay years, by any professional friend? How many of that equivocal class of lives (for whom a much higher premium is demanded at the offices), who cannot conscientiously be certified for without a special examination? And are professional men to act gratuitously—to devote their time and talent for nothing, to such investigations; binding themselves, into the bargain, to abide by the result? We have already said, that as a general rule, the office ought to pay: we will now add, that, in such instances as those just quoted, the applicant should pay also.

The public attention, we are glad to perceive, begins to be awakened on this subject. Some excellent letters have appeared in the newspapers, setting forth the injustice and the unreasonableness of the conductors of assurance directors in this matter; and, so far as we have been able to discover, only one feeble attempt has been made to defend the other side of the question. A writer, signing himself "M. D.," whom we strongly suspect, from the

internal evidence afforded by his letter, to be a medical officer, or employé, of one of the companies, is the solitary advocate of the course pursued by the offices. His arguments (if they deserve that name) are singularly feeble and sophistical. What he says may be reduced to three points—and very weak points they must be allowed to be. 1. In the first place he asks, "Are the directors of assurance offices blindly to send fees to all the medical men to whom they are referred?"—and he supposes the extreme case of their being referred to a St. John Long, or some person of that description. The reply to this is very simple. If the directors proceed *blindly* about their work, it is their own affair, or that of the company to which they belong. But there is no necessity for (nor, indeed, does there seem much likelihood of) this profuse sending of fees. There can be no such great difficulty, as M. D. supposes, in the directors making a selection, if they have a number to refer to. The three medical corporations which license medical practitioners throughout the country, have their printed lists, which the directors could easily procure, and thus make themselves acquainted with those to whom they might safely apply, and send the proper fee.

2. We learn from M. D.'s production, that "on all occasions when a letter of queries is sent to the medical, another is sent to the private friend, of the party. These private friends are sometimes clergymen, solicitors, barristers, bankers, merchants, &c. or other professional persons, whose time is valuable, and whose opinions are important." He adds, that these persons never get fees on such occasions; and asks, why should they not, if the medical man be entitled to payment? This is mere sophistry. In the opening clause of the quotation the writer makes a distinction between the *medical* and the *private* friend: yet in what follows he affects to consider the queries sent to both to be of the same nature. M. D. must know that this is not the case. The medical referee is applied to purely in his professional capacity: the application to him is special, confidential, official, and imperative Is the clergyman applied to *professionally?* Is the solicitor? Is the barrister? If they be—especially the latter parties, we will be bound for it, they must have their fees: they will send in their bills of costs, which *must* be paid. And so should they all be remunerated, if they be applied to in any other capacity than as private friends. The medical man, if referred to merely as such, and not professionally, or under strict injunction, we venture to say will be quite as ready as any other private friend to give all the information he can, without fee or reward.

3. The last point in M. D.'s letter is rather ludicrous. His conscience is so tender in the matter of certifying, that he cannot persuade himself to take a fee either from the applicant or the office. And why? Because, if he take a fee from the applicant, it will seem as if he (M. D.) was bribed to speak more favourably of his patient than he ought: and if he accept a fee from the office, and the patient happen to be rejected, the latter may think that he, the said M. D., was bribed to give an unfavourable report! Surely there must be something morbidly sensitive about such a conscience; perhaps not without a cause. The reputation of M. D. must, we say, be very questionable indeed, if it be so open to the suspicion of bribery. Enviable position, truly, of a medical man in the estimation of his patient, if he be supposed capable of being bribed to a dishonourable act by the paltry temptation of a fee!

If the assurance offices have no better advocate than M. D., we would recommend them to send him a fee to induce

him to hold his peace. Perhaps he would have no scruple to accept one on that score.

CHAIR OF SURGERY IN EDINBURGH.

As might have been expected, neither the Edinburgh press, nor the members of the Town Council, have suffered the ridiculous story of the *Courier*, that Mr. Liston had been offered the Chair of Surgery in "modern Athens," to pass unnoticed. This ingenious piece of puffing, which the editor of the *Lancet* echoed last week, as in duty bound, but which we then ventured to deny, on the strong presumptive evidence which we adduced, has since publicly received a contradiction the most direct, explicit, and unequivocal. Mr. Maclaren, one of the electors, took occasion, at a recent meeting of the Council, to make the observations which will be found below; and the editor of the *Scotsman*, having quoted the passage alluded to from the *Courier*, declares it to be "entirely a misrepresentation of the fact," and calls upon his London contemporary "as a matter of justice to correct his misstatement." To this appeal the *Courier* replies, with the most unblushing effrontery, that it "never assumed nor insinuated that Mr. Liston would necessarily have been elected to the Chair, had he been willing to accept it!" Pretty well this, after telling us last week that the professorship was only offered to another "as soon as it was ascertained by reference to Mr. Liston that he would not forego his prospects in London for any of the medical chairs in the University of Edinburgh." It now appears, however, from the shewing of the *Courier* itself, that the implied offer, when stripped of its editorial embellishments, is simply this:—Some indiscreet person asked Mr. Liston if he would become a candidate for the vacant professorship, and that gentleman having, during his recent visit to the North, ascertained exactly how the land lay, very wisely said he would not. Now this private transaction of two individuals has been converted into an official application to Mr. Liston, and the refusal on his part of an offer—which was never made. One little additional piece of information, of rather an amusing kind, is afforded us by the *Courier*—namely, that the article which it now attempts to back out of, with so bad a grace, was written *in consequence of information received from Mr. Liston himself.*

Proceedings of the Town Council at Edinburgh, Dec. 15, 1835.

* * *

The Lord Provost then proposed that Sir Charles be elected Professor of Surgery in the University of Edinburgh, which was seconded by Mr. Bruce, and unanimously agreed to.

Mr. Maclaren said, he had observed a paragraph in the *London Courier* relating to this subject, which was calculated to give pain to the eminent individual whom they had just elected. It stated that the feelings of the Council were nearly unanimous in favour of Mr. Liston, but that having been applied to, and having refused to leave London, it was then only the Council had thought of applying to Sir Charles Bell. Such a statement being TOTALLY UNFOUNDED, and calculated to hurt the feelings of him whom they designed to honour by their unanimity of choice, he thought it right to bring the circumstance before the Council, that the editorial article alluded to might be publicly contradicted.—*Scotsman.*

Sir Charles Bell's Letter to the Lord Provost of Edinburgh.

Brook Street, 11th Dec.

"MY LORD,—I have had the gratification of receiving your lordship's letter, and I accept the honour which you, the magistrates, and council, have conferred upon me.

"The manner in which this appointment has been conferred, and the assurances with which your lordship has accompanied the communication, lead me to anticipate the utmost harmony in our endeavour to cultivate medical science, and promote the reputation of the University.

"You place me where there is every

motive for personal exertion; but where, most of all, experience and judgment are necessary. As to the first, I trust my habits of life give you sufficient warrant. For the rest, I ground my hope of being useful on the characters of those distinguished men, many of them removed from the stage, with whom I have been united, or with whom I have been in consultation on many trying occasions. By this intercourse my opinions have been matured, and now, through your kindness, I shall use the advantages which time and circumstances have given me, so as to preserve the record of the opinions and practices of the men who have raised the profession of surgery in these kingdoms to its present acknowledged perfection.

"By this, to me, eventful change, I am led to reflect on the advantages I have possessed, in my anxiety that your lordship and the council shall not have occasion to regret the decision to which you have come.

"The unanimity of the council, and the very flattering expressions of your lordship, lessen the regret which every one must acknowledge to be natural to me in leaving a society where I have experienced uniform kindness and liberality, and in breaking the intimate and friendly relations with some of the most estimable men of our day.—I have the honour to be, my Lord, with great respect,

"Your Lordship's obliged and very humble servant,

"CHARLES BELL."

CLINICAL LECTURE

ON

FISTULÆ IN PERINEO,

Delivered at St. George's Hospital, Dec. 8, 1835,

BY SIR B. C. BRODIE.

GENTLEMEN,—You will recollect that in the case to which I drew your attention in the last lecture, one of the effects of a stricture in the urethra under which the patient laboured, was an abscess in the perineum, which had terminated in what we call *fistula in perineo*. We have another patient, labouring under the same disease, lying in the adjoining bed to the patient just referred to.

"Samuel Eldridge, 53 years of age, a coachman by occupation, was admitted on the 14th October, 1835, into Harris Ward. It appears that five months ago he had an attack of fever, attended by shivering, followed by pain, and subsequently by an abscess which burst in the perineum. He then got better, and resumed his work, although suffering much inconvenience. Three weeks before his admission the pain of the perineum became again very severe, attended with a frequent desire to make water, and great difficulty in doing so; and in this state he continued till he came into the hospital. At the time of his admission I examined the perineum, and found a large abscess in it, the skin having been extensively undermined. The abscess was immediately opened, and a great quantity of matter, putrid from the admixture of urine, was evacuated. The constitutional symptoms under which he laboured were immediately relieved, and the abscess gradually contracted. However, the greater part of the urine continued to flow, not by the natural passage, but through the opening in the perineum. On the 4th November I introduced a gum catheter, No. 9, mounted on an iron stilette, into the bladder. It did not pass without some difficulty, an obstruction being discovered in the usual seat of stricture, that is, in the membranous part of the urethra. On the 16th a gum catheter was introduced again, and allowed to remain lodged in the urethra. The retention of the catheter gave the patient no inconvenience. On the 6th December there is the following report of the case:—" He has continued to use the gum catheter; it having been removed, however, two or three times, to be replaced by larger ones. This day the gum catheter was taken away." He now makes water without pain or difficulty; the wound in the perineum is nearly healed, no more than a few drops of urine at this time passing through it when he makes water.

This man describes his case as having began with an attack of fever, attended with shivering; but you will observe that there was an abscess in the perineum, and when I passed the gum catheter there was an obstruction in the usual seat of stricture. I should conclude that there had been a stricture of the urethra going on for some time, that the patient had been making water in a small stream, but that the diminution of the stream having taken place gradually, it had escaped his observation. I conclude, also, that the attack of fever, which he describes as beginning with shivering, was the result of the abscess which this stricture had produced.

I refer to the case which was the subject of the last lecture, and I call your attention to the present case, chiefly on this account, that in each of these cases there was an abscess in the perineum communicating with the urethra, and forming what we call a *fistula in perineo*.

I am glad to have an opportunity of calling your attention to the subject of these fistulæ, because I do not think

that it is very well treated of in books; and because I am, at all events, bound to acknowledge, that in my Lectures on the Diseases of the Urinary Organs, which are published, the account of it is not so explicit as it ought to be. At least, I think I can give a better account of the disease now than I was able to do then.

A *fistula in perineo* is the result of an abscess in the perineum, communicating with the urethra at one extremity, and opening externally at the other. The whole or part of the urine flows through the fistula whenever the patient makes water; the constant irritation of the urine causes the sides of the fistula to become hard and callous; and at last, a sort of button-like projection, with the orifice of the fistula in the centre, is discovered in the perineum. The fistula differs in length and size, and the opening into the urethra differs in size also There is sometimes a single fistula, sometimes there are several; and, accordingly as the fistula is larger or smaller, accordingly as there are more or less sinuses communicating with the urethra, so does a larger or smaller quantity of urine escape through this unnatural passage whenever the patient makes water. Sometimes the urine thus voided amounts to only a few drops; sometimes it comes away in a small stream; and sometimes it comes away in so large a quantity, that that which is voided by the perineum is greater than that voided in the natural way.

A *fistula in perineo* is a very great inconvenience to the patient: he wets his dress when he makes water. The perineum is constantly teazed by the urine dribbling over it, and the passage of the urine through the fistula is often attended with a good deal of irritation and pain. A *fistula in perineo*, however, is not an evil unmixed with good. You will ask, what is the good effect that it produces? If a man has a stricture of the urethra the fistula saves him from the ill consequences of retention of urine. A spasm comes upon the stricture, and he cannot make water if there be no fistula; but if there be a fistula, when there comes a spasm on the urethra, and he cannot make water otherwise, the urine generally escapes by the fistula, and thus prevents the over-distention of the bladder, and all the ill consequences resulting from it.

A *fistula in perineo* occurs, in some cases, as a consequence of gonorrhœa; in other cases as the consequence of stricture, either independently of gonorrhœa or long after it has subsided. These two kinds of *fistulæ in perineo* require each a separate notice.

A man has a severe gonorrhæa; the urine comes away in a very small stream, because the urethra is inflamed, swollen, and contracted. At last he complains of pain in the perineum; a tumor is found there; and this state of things is frequently attended with a complete retention of urine in the bladder: at other times, however, it is attended only with an increased difficulty of making water. By and by the tumor bursts; or perhaps the surgeon feels fluctuation in it, and opens it with a lancet; pus is discharged, then the difficulty of making water subsides, and a day or two afterwards the patient finds that whenever he voids his urine a portion of it comes by the opening in the perineum.

In another case, the patient has, perhaps, never had gonorrhœa; or if he has had one, it has subsided, and left a contraction or stricture of the urethra. On some occasion he has a more than usual difficulty in making water, but not amounting to a complete retention of urine. Under these circumstances he discovers a swelling in the perineum, which increases in size, and becomes painful and tender. The tumor bursts, or the surgeon opens it; matter escapes also in this case, and afterwards a portion of the urine comes away through the opening. Where this disease is connected with gonorrhœa, there is generally only a single abscess; but when it is connected with a stricture of the urethra, there may be many abscesses formed in succession: so that a patient having a stricture of the urethra of long standing, may have *fistulæ* formed in various directions, opening into the perineum, in the middle of the scrotum, nay, even in the groin, or on the nates, or one of them, perhaps, may open into the rectum.

When the matter and urine are freely discharged through one opening, it does not often happen that any fresh abscess is formed; but when the matter and urine are not freely d scharged from the original opening, then it is probable that several abscesses may follow. This is the history, in most cases, of the formation of several *fistulæ*.

How are these *fistulæ* generated? Why should an abscess form in the perineum? Because there is a contraction of the urethra. You will observe, that whether the *fistula* is the consequence of gonorrhœa or of a stricture, some urine always flows through it afterwards, so that the abscess always communicates with the urethra.

In a great number of cases you find that the matter which is discharged is not healthy pus; it is putrid and offensive, and operates as a poison to the system; so that the patient, during the formation of the abscess, not only has repeated shiverings, but a dry brown tongue, a hot skin, a frequent pulse, and other symp-

toms of great constitutional disturbance: he labours, in fact, under all the symptoms of a severe typhus fever. Now all this is the consequence of the matter being putrid. But what is it that makes the matter putrid? It evidently arises from some admixture of urine with it.

Now if you consider all these circumstances, that in many cases the contents of the abscess bear manifest indications of urine having been mixed with them; and that in all cases, in the course of two or three days after the abscess has burst, urine begins to flow through it; I think you will be inclined to believe, with me, that an aperture must have been formed by ulceration in the mucous membrane of the urethra in the first instance. Such ulceration would allow a little urine to dribble into the cellular membrane, and the formation of an abscess would be the necessary consequence. This is the simplest way of explaining the formation of these fistulæ, and it explains every thing about them. The escape of even a single drop of urine into the cellular membrane would be sufficient to do all this mischief. I have no doubt that many abscesses about the rectum are formed in the same manner by ulceration taking place above the sphincter muscle, and a small quantity of feculent matter escapes into the neighbouring cellular membrane.

But you have been often told of the ill consequences which arise from ulceration of the urethra behind the stricture, and the consequent effusion of urine into the cellular membrane in the neighbourhood; that an effusion of urine through such an ulcerated opening produces inflammation, sloughing, and probably the death of the patient; and you will ask, why does it happen that in one case effusion of urine produces merely an abscess, which, unless it be neglected, is attended with no danger, and in other cases that it produces extensive gangrene, and afterwards death.

A *fistula in perineo*, and a general effusion of urine producing gangrene, are both, I apprehend, connected with ulceration of the urethra; but the ulceration occurs under different circumstances. If the patient be making water in a small stream, and a little ulcer forms behind the stricture, the greater part of the urine flows out by the natural passage, and it is not probable that more than a few drops will dribble into the cellular membrane through the ulcerated orifice. But if the patient has complete retention of urine, if the stricture be so contracted that not a drop of water can pass through it, and if then the urethra ulcerates behind the stricture, observe what will happen: the next time the patient tries to make water, he strains with the bladder and the abdominal muscles; the urine cannot pass by the natural passage, and it is driven by the muscles which I have mentioned, as it would be driven by a syringe, into the cellular membrane, of the perineum first, and into that of the scrotum, penis, and nates, afterwards, and a great deal of urine is extravasated. In the one case it *dribbles* into the cellular membrane, in the other it is *driven* into it; and thus I say that you will easily understand why there should be sloughing under certain circumstances, and an abscess under others.

I have known surgeons formerly who supposed that a *fistula in perineo* bore a resemblance to a *fistula in ano*, and that it was to be laid open and treated in the same manner; and I have seen this done in more than one instance. Now suppose that you were to dilate a *fistula in perineo* very extensively, what would happen? The wound would heal up to a certain point, until the *fistula* had regained the size which it had before the dilatation was made, and then the healing process would stop, and the patient be as bad as ever: in short, you gain nothing by such an operation. Consider what it is that prevents the *fistula* healing; the urine dribbling into it. How are you to make it heal up, but by preventing this dribbling of urine? Then how is this to be accomplished? Why does the urine find its way into the *fistula* in ordinary cases? It is because the urethra is contracted in front of the orifice, by which the *fistula* communicate with it. To cure the fistula, you must remove the contraction, restoring the urethra to its natural diameter.

In a case of *fistula* after gonorrhœa, this is easily accomplished. A few introductions of a bougie will probably be sufficient to dilate the urethra and make the *fistula* heal.

In cases of *fistula in perineo* connected with chronic stricture of the urethra, the treatment is just the same. All you have to do is to cure the stricture, and in nineteen cases out of twenty, by the time that it is fully dilated the *fistula* is healed. It is more easy for the urine to pass along the natural passage, if it be of its proper diameter, than it is for it to pass through the oblique passage of the *fistula*. The *fistula* has generally a kind of valvular opening into the urethra, into which the urine does not easily flow; and when you have dilated the stricture in front of the *fistula*, the urine having a free passage in that direction, ceases to flow in the other.

But there are some cases in which, in consequence of some complication—from the opening of the *fistula* being unusually large, or from there being several *fistulæ*, or from some other cause—the cure of the

fistula is not immediately consequent upon the cure of the stricture. When the *fistula* does not immediately heal, you should examine the perineum, and ascertain whether there is any cavity in it in which the urine and matter are likely to lodge. If the orifice of the *fistula* is such, that matter formed at the bottom of it does not readily escape, you must certainly dilate the orifice, or make a fresh opening somewhere else. But supposing that there is no difficulty of this description, that you have dilated the stricture to its proper diameter, and yet that the *fistula* does not heal, you may then introduce an elastic gum catheter into the bladder, and allow it to remain in the urethra, so as to draw off the urine by the catheter, and the cure of the *fistula* may perhaps take place under this mode of treatment. Yet I must acknowledge that in many cases the permanent retention of a gum catheter in the bladder does not answer the intended purpose with respect to the cure of the *fistula*. It would do so if it altogether prevented the urine flowing through it; but the fact is, that it does not in reality produce this effect: after three or four days a little urine always finds its way by the side of the gum catheter, and gets into the *fistula*, although the greater part is drawn off through the catheter. Another circumstance also takes place: the gum catheter acts like a seton, inducing inflammation and suppuration of the mucous membrane of the bladder; and some of the pus which is secreted passes through the *fistula*, and keeps it open just as much as it would be kept open by the urine itself.

In some cases I have adopted the following method:—I have made the patient draw off the water with a catheter three times daily, so that he should never make water except through the catheter, and thus that the urine should be altogether prevented from finding its way into the *fistula*. This is a better mode of treatment than the constant retention of a gum catheter in the bladder; and yet it will sometimes fail. It may be that the patient cannot manage to retain his water for eight hours, and then the introduction of a catheter three times in the twenty-four hours of course is not sufficient; and if he introduces the catheter more frequently than this, the catheter itself becomes a source of irritation, keeps up the inflammation, and produces suppuration of the urethra—in fact, operates much in the same way as a gum catheter when it is constantly retained.

In some cases, formerly, where a *fistula* was slow in healing, I tried the following method of treatment. I applied the nitrate of silver to the orifice of the *fistula* in the perineum. The orifice in general soon begun to contract, and at last healed. But it was only the orifice that healed, not the bottom of the *fistula*, and the matter always collected within, so that a fresh abscess was the consequence, bursting externally, either in the old place or somewhere else. In other cases, again, I have adopted the following plan with better success. I endeavoured to stimulate the bottom of the *fistula*, so as to make that heal, while I took measures to prevent the orifice from healing prematurely. This was effected in the following manner. I melted a little nitrate of silver in a spoon of platina, and dipped the end of a probe into it. This being repeated two or three times, the end of the probe became covered with a varnish, I may say, of the caustic. I introduced the probe, thus prepared, quite to the other extremity of the *fistula*; and when I had done this, I just touched the orifice of the fistula very slightly with the caustic potass. The effect of the nitrate of silver upon a sore surface is not so much to make a slough as it is to stimulate it to contract, granulate, and heal; whereas the effect of caustic potass is to make a slough, and prevent healing. The object which I had in view was that of making the bottom of the *fistula* heal, and preventing the orifice from healing too soon. This plan I found to be sometimes useful, and at other times to be of little benefit. Uncertain as all these modes of treatment seem to be, I have scarcely ever seen a case of *fistula in perineo* in which the patient has not ultimately got well. I do not say there are no such cases, but they are very few indeed. If all more active modes of treatment fail, tell your patient to do this: let him introduce a good-sized instrument—a metallic instrument is better than a common plaister bougie—into the bladder every morning, and not trouble himself further. If the *fistula* does not heal in three months, it may heal in six, and if not in six months, it may in the course of twelve months. I have seen a number of obstinate cases of *fistula in perineo* which, after other means have failed, have got well when the patient has had patience enough to persevere in the very simple plan of treatment which I have mentioned. I have known a patient who has voided a tea cupful of urine through a large *fistula* in the perineum every time he made water, in whom the stricture having been first fully dilated, and an instrument having been daily passed into the bladder afterwards, a perfect cure has at last taken place. Of course it is necessary that the stricture should be kept fully dilated; for if you allow it to contract, then the urine will dribble into the *fistula*, and prevent it closing.

These observations apply to the common cases of *fistula*; but there are some more rare cases, in which there is an abscess in the perineum communicating with the urethra, but not having burst externally. A patient will come to you having such an abscess in the perineum; every time he makes water the abscess swells from the urine passing into it, and when he presses upon the perineum, afterwards he squeezes urine and matter together into the urethra; and this state of things may continue for a long time. How is such an abscess as this to be treated? Will you cure it by drawing off the water? Will you cure it by dilating the stricture? Will you cure it by keeping an elastic gum catheter always in the bladder? Certainly not by any one of these methods: you must dilate the stricture; but that is only a preliminary measure. One of these blind *fistulæ* in the perineum will not be cured by simple dilatation of the stricture; and yet the cure is a very simple one. Make a free incision into the abscess, so that the fistula may open externally as well as internally; then, whenever the patient makes water, the urine, instead of lodging in the cavity, runs out at the aperture which you have made, and you convert this blind fistula into a common one, which is to be treated in one or other of the ways which I have already mentioned.

I shall next describe a case which used to perplex me very much when first I met with it, and which would perplex you when you meet with it hereafter, if I were not to explain its nature. A patient may come to you who has, perhaps, had gonorrhœa formerly, which has been followed by a stricture, perhaps a very slight one, of the urethra; or at any rate there has been a discharge from the urethra, which he calls an obstinate gleet, telling you, at the same time, that nothing will cure it. You examine the perineum, and you find in it a little tumor, not bigger apparently than a horse-bean or filbert. You can just feel it at some distance below the skin, and the patient tells you that he has had it ever since he has been the subject of this obstinate gleet, and that sometimes there is a little pain in it. Now such a little hard tumor is neither more nor less than a blind *fistula*. There is a small orifice in the urethra, and a narrow channel communicating with it, which leads into a cavity in the centre of this hard lump, and every time that the patient makes water a very small portion of urine finds its way into this cavity. In consequence of the smallness of the central cavity, and the great deposit of solid matter on its outside, the fluctuation of fluid in it is not perceptible. Such a case as this is not a very uncommon occurrence, and I have known a patient labour under this sort of hard lump in the perineum for many successive years, suffering a good deal of inconvenience from it, but not suffering excessively.

I have cured several cases of this kind by a very simple process. All that you have to do is to make an opening into the cavity in the centre of the tumor. But the cavity is very small, and how are you to find it? You may run a lancet into it, but it is very probable that it may pass on one side of the cavity; and therefore some management is necessary in performing the operation. You are to introduce the lancet a little obliquely, so that you may, as it were, almost cut the tumor in half. When you have done this, the blood and the deep-seated situation of the tumor prevent you from seeing whether you have made the opening in the central cavity or not. Introduce a piece of lint, so as to prevent the wound uniting by the first intention. Two or three days afterwards, you take out the lint; and then you ascertain whether, when the patient makes water, any comes by the opening made with the lancet. If this be the case, you may be certain that you have penetrated into the cavity, and then you have only to dilate the urethra with a proper instrument, and the patient will get well. But if you find that the urine does not flow through the artificial opening, you may proceed thus:—Introduce a piece of caustic potass through the opening you have made, down to the bottom of it, in the centre of the tumor, so as to make a slough there. A portion of the tumor will slough out, and it is twenty to one but what the cavity in the centre will be exposed, and then a cure follows. You should apply the caustic potass in such a manner that it may act on the part on which you wish it to act; defending, at the same time, the neighbouring textures, the skin especially, by washing it with vinegar.

A patient may have a *fistula* in the perineum, attended with some little contraction of the urethra, not giving him much inconvenience, so that it may not much attract his attention; and I have known individuals who have continued to be in this state for years together. Are you to allow them to remain thus merely because the *fistula* is not very troublesome? Certainly not; for you do not know to what mischief it may ultimately lead. The matter, on some occasion, may not readily escape; it may burrow, and cause sinuses in other directions, and may even do still greater mischief. Mr. Vincent and myself attended a gentleman who had suffered under a *fistula in perineo*, and which he had

neglected for a great many years. At last he observed the callosity round it grew bigger and bigger, so that it ulttimately extended to the scrotum and penis. When we were called in to see him, we found him with a malignant disease, either carcinoma or fungus hæmatodes, which hadclearly had its origin in the *fistula*, and had extended from that to the neighbouring organs. The patient ultimately died, in great distress and misery.

LECTURES ON THE DISEASES OF THE NERVOUS SYSTEM.

BY M. ANDRAL.

As published in the *Gazette des Hôpitaux*, with the approval of the learned Professor.

INFLAMMATION OF THE SPINAL MARROW.

Chronic Encephalitis and Cerebellitis—Inflammation of Spinal Marrow—Anatomic Characters—Symptoms affecting Motion, Sensation, Digestion, Circulation, Respiration, Secretion, and the Genital Organs.

Chronic encephalitis bears a strong resemblance to the acute form of the disease: it presents nearly the same anatomical characters, but with the addition of induration. Abscesses, too, are more frequently met with than in acute encephalitis; and the pus is inclosed in a cyst, which is well formed, and has sometimes fibrous or fibro-cartilaginous parietes.

The causes are the same as those of acute encephalitis; and the disease may occur either primarily, or follow the acute form. The symptoms, also, are the same in both, except that their manifestation in the chronic is more slow and less energetic.

Chronic cerebellitis.—Chronic inflammation, and even abscesses of long standing, are frequently met with in the cerebellum, though acute inflammation there is of rare occurrence. In six or eight cases of chronic cerebellitis you will not, perhaps, find the same symptoms in any two; it is, therefore, impossible to say any thing positive on this head, and before doing so, we must wait for additional facts. It is thus that in some cases the lesion principally affects the movements of the upper or lower extremities, which may be either affected with complete paralysis, or only with a certain degree of weakness, such as to prevent the patient from being able to walk without staggering, or to keep him in perpetual danger of falling forwards. In other cases nothing is observed to be amiss with respect to motion, but there is some conspicuous change of sensation. In two cases, acute and general sensibility of the entire surface was observed; and after death the only lesion discovered was an abscess in the cerebellum. In another there was exquisite pain about the occiput; in two loss of sight was the principal phenomenon. I have already observed, in my introductory lecture, that certain affections of the cerebellum, such as tubercles, might produce blindness in children. In yet other cases, with the lesions of movement, sensation, &c. there have been nausea and vomiting throughout the whole course of the disease.

Thus it will be seen that we may have a variety of symptoms differing very much from each other, and produced separately, by the same morbid condition; and these symptoms do not as yet admit of being generalized. In proportion as the facts become multiplied, it will probably be seen that the phenomena which, indicate chronic inflammation of the cerebellum, have only appeared thus different in consequence of the disease having its seat in different parts of the organ. A diversity of symptoms, which may be readily accounted for from the numerous connections which the cerebellum has with the different points of the cerebro-spinal axis.

The treatment of chronic inflammation of the cerebellum consists in the abstraction of blood, so long as there is any symptom of re-action. We may afterwards have recourse to revulsives applied to the limbs or back of the neck; and we may also advantageously adopt the use of purgatives.

MYELITIS.

After having considered the acute and chronic inflammations of the parts within the cranium, we now proceed to inflammation of that prolongation of the cerebral mass which is contained in the vertebral canal, and which has been called the spinal marrow. *Myelitis* is the name given to this disease. Now myelitis may be either acute or chronic; but being pressed for time, I shall not separate the description of these two forms. In describing one, we shall compare its symptoms with those of the other, in such manner as to mingle their common characters, while we point out more prominently those in which they differ.

The *anatomical characters* are absolutely the same as those of encephalitis, viz. injection, softening, whether red or white, and suppuration, either infiltrated or collected into masses.

M. Velpeau found an encysted abscess near the junction of the cervical and dorsal portions of the spinal marrow. Softening and suppuration may produce an almost complete solution of continuity in

the vertebral cord, so that there may remain only a very thin fragment of nervous matter; or it may be transformed into absolute *bouillie*, preserving nothing of its natural organization.

The seat of these lesions is varied: the inflammation may affect the entire thickness of the spinal cord, or each of the sections separately, so that the anterior or posterior portions may alone be implicated,—a limitation which in some cases is both precise and well marked. Besides these distinctions, there are others: thus the inflammation may have its seat in the white substance or the grey, and, indeed, is more common in the latter. Now as this (viz. the grey matter) is situated in the centre of the cord, it happens that when it is considerably softened a vacuity is produced—a sort of central canal, more or less analogous to that which naturally exists in certain animals, and even in the human subject, up to a certain period of fœtal life. I have also seen this kind of softening in an aged person.

The *causes* are the same as those of inflammation of other parts of the nervous system, comprehending external injuries, diseases of neighbouring parts, &c. We ought also to reckon among the causes of myelitis, over action of the organ itself. M. Dupuy, of Alfort, has demonstrated that inflammation of the spinal marrow is not uncommon in animals who are compelled to perform movements too violent or too long continued, as in horses which are over-worked. The result of the observations made by the professor just named, as well as those of many other veterinary surgeons, is, that inflammation of the spinal marrow is much more common in horses than inflammation of the brain.

In speaking of the *symptoms*, we shall follow the same divisions as we have already done with regard to encephalitis.

The intellect affords us only negative results; it preserves its integrity, at least in the beginning; but the inflammation may afterwards extend to the brain, and sometimes does so very rapidly when there is but a short distance between it and the inflamed part,—as for example, when the myelitis occupies the medulla oblongata.

Motion exhibits very various disorders; and it is chiefly with respect to this function that the disease of which we speak is manifested, when it affects the anterior cords. The seat of these lesions of motion is different according to the portion of the spinal marrow which suffers, the movements of the parts which derive their nerves from the seat of the inflammation being those which are deranged.

We may also observe, then, these derangements of function in the upper or lower extremities, or in one limb only, or in all at once. The affection of motion may be displayed only in some muscles of the trunk, or it may be the diaphragm which is deranged in its action.

A general principle may here be laid down—namely, that the lesions of motion in myelitis only implicate the parts beneath the portion of the spinal marrow which is inflamed; the nerves below are no longer capable of transmitting the power of contraction. Nevertheless, M. Lullhier has cited the case of a man in which he found inflammatory softening of the cervical and upper part of the dorsal marrow, although the motion of the upper extremities had not been in the slightest degree affected. When the inflammation commences at the lower part, paraplegia only is at first observed; but in proportion as the disease advances, the muscles of the abdomen, those of the chest, upper extremities, &c. become successively implicated.

The nature of the lesions of motion differ according as the inflammation is acute or chronic: in the latter we can only observe a simple embarrassment in the movement of one or more parts. Sometimes, in place of absolute palsy, the patients only complain of weakness, more or less marked, in the state of repose; they think themselves as strong as ever, but if they be directed to grasp any thing in the hand their loss of power becomes evident. They also walk less perfectly.

In some, a slight and progressive weakness first comes on, to be followed by paralysis; sometimes it is not until two years or more after the first attack of myelitis that the paralysis becomes manifest; occasionally it is very rapid, but this is rare, for chronic inflammation of the spinal cord is much more common than acute. In some individuals cramps take place, which differ in their situation, according to the seat of the lesion; in the abdominal muscles, for example, the cavity is compressed and excruciating pain produced, which is known under the name of cramp of the intestines. In certain other cases the cramps occur in the muscles or the limbs; indeed it occasionally happens that, at first, these constitute the only symptom, to which, however, others of a more marked character soon become added. In yet other cases it is convulsions which appear, particularly in the acute form of the disease: it is such convulsive movements in acute myelitis which exactly imitate chorea, and of which M. Gendrin has related an example in the Gazette des Hôpitaux. In a few instances the myelitis has assumed a more formidable aspect, and has given rise to symptoms of

tetanus. Are we, then, to assume that every case of tetanus is produced by inflammation of the spinal cord? Certainly not; as I shall endeavour, by and by, to demonstrate. However this may be, death takes place, sooner or later, as the usual termination of this form of the disease.

Inflammation of the spinal marrow is also accompanied by various derangements of sensation; and these are predominant when the inflammation is situated in the posterior bundles. Pain is perceived at a part of the spinal marrow corresponding to the seat of the inflammation; this pain may be felt along the whole extent of the vertebral column, if the myelitis be general. It may also be aggravated by motion; but this is not constant, and principally occurs when the brain participates in the disease. Sometimes it is impossible for the patient to lie upon his back, so that he is compelled to recline on the side. It has been ascertained occasionally that pressure upon one particular spinous process corresponding to the seat of disease affects this part, notwithstanding its distance, and the interposition of the cephalo-spinal fluid; and this pressure constitutes an excellent diagnostic mark.

It has been recommended to run a sponge dipped in hot water along the spine, and it has been said that the occurrence of pain would point out the inflamed part, when the sponge arrived at it. I have tried this method, but without being satisfied of its efficacy; I think, however, that it may be useful in distinguishing this from disease of the cartilages. Although we have insisted on the manner of recognizing pain, yet its presence at a particular point of the vertebral column is not a pathognomonic sign of inflammation of the nervous cord which it contains. This pain, in fact, may depend upon an affection of the fibrous tissue, a kind of rheumatism peculiar to these parts. The diseases of the bony column may themselves give rise to such pain, or it may be of a neuralgic nature: thus rachialgia occasions pains which are often even more acute, and more intense, than those of myelitis.

Lastly, the pains may depend on inflammation of the meninges, without the spinal marrow, in any degree, participating in the disease.

Let us next turn our attention to the affections of sensation which take place at a distance from the spinal cord. Pain may be felt in the limbs, in the different parts of the trunk, or in the parietes of the abdomen. They sometimes follow the course of the large tendons; sometimes they are more vague, and have no precise or permanent seat; sometimes they are constant, and sometimes only occur at intervals. Again, these pains, which act so important a part in the history of myelitis, in certain cases assume, at their onset, the appearance of rheumatic or nervous affections, the rather as, under certain circumstances, myelitis betrays itself by pain in distant parts (of the limbs for instance), while nothing whatever is felt along the whole course of the vertebral column.

In place of that increase of sensibility which is announced by pain, there may be loss of sensation, becoming more and more general, in such sort, for instance, that the skin ceases to receive any impression even when violently pinched on any part of the surface. In other cases, some circumscribed portions of the skin preserve their sensibility unimpaired, whilst but a line beyond it is totally insensible. There may also be a simple numbness—a diminution to a greater or less extent, but not an abolition of this function. Sometimes different parts of the surface are affected with tingling, and of a sensation of cold which nothing can overcome: this is particularly remarked, at first, at the points of the fingers, but there is a tendency for the phenomena to extend after they have been limited for a longer or shorter time.

The different functions connected with nutrition are conspicuously influenced by inflammation of the spinal marrow.

With regard to *digestion*: in some cases we observe a difficulty of deglutition, evidently depending upon loss of play in the muscles of the pharynx; and this impediment to the act of swallowing may give rise to the belief that the patient is affected with an angina, which is persistent; but it may be ascertained, on post-mortem examination, that what has existed there has not been any form of angina, but an impediment to the contraction of the pharynx, arising from a lesion of the spinal cord. I have seen a case of this kind, in which there was at the same time paralysis of the tongue. On opening the body very acute inflammation was found at that part of the spinal column from which the nerves which give motion to the tongue arise. M. Fournet has also seen a case in which, after the fall of a great weight on the upper part of the vertebral canal, there suddenly supervened a paralysis, which was exactly limited to the tongue and pharynx, without the slightest sign of the same affection in any other part.

In some persons attacked with myelitis, vomiting occurs, as in inflammation of the brain. Sometimes the contraction of the lower portion of the alimentary canal be-

comes modified, and there is a degree of constipation, which frequently refuses to yield to the most active purgatives.

As to the *circulation*, if the myelitis be acute and general, there is fever, which is sometimes violent; but if the inflammation be chronic or partial, the fever is often entirely wanting. We have then, however, other remarkable symptoms: thus there may be palpitations so as to alarm the patient, and even to induce the physician to apprehend the existence of organic disease. Sometimes the most remarkable symptom is a tendency to syncope. I am at present attending a young woman who has all the signs of chronic inflammation of the spinal marrow. The slightest attempt at movement is sufficient to produce fainting, and she is becoming worse in proportion as the function of the spinal marrow is reduced.

We have, then, two sets of phenomena affecting the circulation: one belonging to the acute form of myelitis, and characterized by fever; the other connected with the chronic, and not accompanied by fever, but affecting the heart, and producing palpitations, whence, as we have just seen, may arise a constant tendency to syncope from the slightest causes.

Respiration is affected whenever that portion of the spinal cord is inflamed whence the nerves arise which supply the respiratory muscles, and also in all cases of myelitis which are of long standing; because in these the disease has a constant tendency to spread from beneath upwards. But the manner in which the respiration becomes embarrassed is not the same here as in encephalitis, where the breathing ceases, because the brain is no longer fitted to transmit through the pneumogastric nerves that influence without which respiration cannot be effected. In myelitis the brain and pneumogastric nerves are entire, the necessity of respiring being felt as usual, while the powers which are charged with the duty of inhaling and expiring the air are alone paralyzed.

Sometimes the diaphragm is paralyzed, or, at least, certain irregularities of its action take place. Among these the most remarkable is hiccup, which lasts until the death of the patient. The result of all these disorders of the respiration which accompany myelitis is, to produce asphyxia. I saw a young man who for a long time had experienced fixed pain at the upper part of the vertebral column. When he placed himself in certain attitudes, and only in certain ones, a crackling noise of a peculiar character, and which I frequently heard, occurred at the spot which was pointed out as the seat of pain; the patient was then, from time to time, attacked with a frightful paroxysm of suffocation, which lasted some minutes, after which every thing returned to its natural order. I think that in this case there is not actually an alteration of the spinal marrow, but probably a change in the superior articulation of the vertebral column, exposing the cord to be touched for a moment, and in certain positions, by a displaced vertebra. We may be led sometimes into the belief that there is disease of the lungs, or asthma, when the embarrassment of breathing arises from the cause just mentioned.

The *secretions* present some remarkable modifications. It has been said that the perspiration is always deficient; but I think that this fact has been too much generalized. The secretion of the kidneys is sometimes completely obstructed, in consequence of paralysis of the bladder; and the lower bowel sometimes participates in this inactivity, so as to produce constipation.

As to the *genital organs*, we sometimes have, in men labouring under acute myelitis, a great tendency to priapism, with pain and convulsive movements; which observation is in keeping with physiological experiments, in which, by exciting a certain part of the spinal marrow, erections, and even emissions, have been produced. On the other hand, impotence, and the impossibility of erection, have also been witnessed as the effects of myelitis. These facts would tend to prove that the genital functions are under the direct influence of the spinal cord, and that they are not exclusively governed by the cerebellum. It has been said that abortion has occurred in pregnant women becoming affected with acute myelitis, in consequence of the convulsive movements of the muscles becoming extended to the uterus; while, on the contrary, in other instances, in the period of collapse, women at their full time have been unable to expel the fœtus without assistance, in consequence of the feebleness of the uterine contractions. We thus have contrary effects according as the inflammation is acute or chronic. But beside these cases, others might be cited in which pregnant women affected with myelitis have gone their full time, and being delivered without any accident: all of which only shews how variable are the phenomena of this disease.

Let us now consider some questions relative to the *diagnosis*, and which it is very difficult to determine. Can myelitis be easily recognized by the symptoms above described? There are many circumstances which may lead us into error: thus the vertebræ may be the seat of diseases which are not perceptible from without—as certain kinds of caries; but it is true that after a time the spinal marrow is im-

plicated, and participates in the inflammation of the investing textures, whether hard or soft. Sometimes the symptoms of myelitis are assumed by another disease of the spinal marrow, in which there is only a lesion of action, without any thing being detected on post mortem examination, either in the spinal marrow itself, or its investing membranes. This lesion of action depends, then, on a deficient influx of nervous energy, or upon some other hidden cause. It is by induction alone that this research can be advanced, for there is nothing appreciable to our present physical means of investigation. Of this, however, I am certain, that I have seen in such cases a complete absence of any perceptible change of structure, in certain patients who had exhibited all the symptoms of inflammation of the spinal marrow for months together. I have seen hysterical young women seized all at once with inability to move one or more limbs, and remain paraplegic for many days. Under such circumstances disease of the spinal marrow is suspected, but a single day may dissipate all the symptoms. These different forms of palsy depend upon that ill-understood condition which constitutes hysteria, just as in certain other cases of the same disease we have blindness as one of its symptoms. It is the same with paralysis from lead; all which proves, however, not that the nervous centres are intact, but only that the organic change eludes the present state of science.

M. Esquirol has related a case in which paraplegia, without any other symptom, coincided with the presence of schirrous masses in the two anterior lobes of the brain; there was absolutely nothing about the spinal marrow. This curious case is in accordance with the hypothesis of those who hold that the motions of the inferior extremities are under the influence of the anterior lobes of the brain.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, Dec. 8, 1835.

HENRY EARLE, ESQ., F.R.S., PRESIDENT IN THE CHAIR.

ON the evening above dated, a paper by Mr. Stafford was read, and the discussion adjourned till Tuesday last. The following is a brief abstract of the paper:—

On the Treatment of Injuries received in Dissection. By R. A. STAFFORD, Esq. Surgeon to the Marylebone Infirmary.

The author commences by noticing the opinions which prevail respecting the effects of dissection wounds; some holding that a peculiar virus is introduced into the system by them, and others that the symptoms depend entirely on the nature of the part affected and the constitution of the individual. Mr. Stafford is inclined to embrace the former notion, there being many cases now on record, showing that the peculiar consequences in question are met with even where there has been no abrasion, scratch, incision, or puncture, and where the absorption of a virus is alone capable of explaining the phenomena. This will appear from some of the following cases, drawn up chiefly with a view to laying down a steady system of treatment.

I.—On the morning of the 2d of March Mr. Stafford was summoned to visit his friend Dr. S., the absorbents of whose right forefinger and forearm were inflamed and painful;—the doctor had dissected a brain the preceding day. There was a mark like a gnat-bite on the second phalanx, but no trace of any puncture. Dr. S. had gone to bed in his usual health, but was awakened by the pain in his finger at four in the morning. Pulse 90, jerking. Leeches and evaporating lotion applied; calomel and senna taken. Worse in the course of the day—nervous system affected as if under the influence of the bite of some venomous reptile. Mr. Lawrence now saw the patient: 20 more leeches were applied, and the fourth of a grain of muriate of morphia administered. The bowels acted freely, and relief was decidedly obtained from the morphia. A deep longitudinal incision was made along the finger, but no pus escaped. The morphia to be repeated as occasion might require. On the following morning the patient was rapidly sinking, when half a grain of mur. morphia was given, with almost immediate benefit. Nothing in the shape of drink but soda and seltzer water would stay on the stomach. Leeches, fomentations, and morphia continued. March 4th—Better, except that the absorbents were not less inflamed. Incision again practised on the finger, also on the palm of the hand, when pus escaped along with the blood. Great relief was also procured from subsequent longitudinal incision and the discharge of pus. The constitutional symptoms gradually gave way, and the morphia was not required in the former proportions. In short, Dr. S. was soon able to leave town, and after six weeks restored to his usual health; the finger, however, still remains stiff.

II.—The second case is that of Mr. Pierce, late assistant-apothecary of the Marylebone Infirmary. He opened the body of a woman who had died of puerperal fever, and on the following morning found himself very unwell, with headache and feverishness. In the evening he com-

plained of soreness in the axilla, but did not suspect that it could have arisen from the absorption of any virus, not being aware of having wounded himself. Leeches and fomentations were applied, but without much benefit. On the fourth day there appeared a general tumor of the side, from the third rib down to nearly the ilium; the parts tense and of a reddish blue colour; no fluctuation. The constitutional symptoms had now attained an alarming character; the patient was fast sinking; when, as a *dernier resort*, an incision nearly an inch deep, and four in length, was made in the swelling of the side. There was no pus, but the patient derived striking benefit from the quantity of blood effused, and the tension taken off the parts. Matter was discharged in a day or two, and suppuration went on thenceforward freely, if not superabundantly for the strength of the patient. But by close attention to the support of the powers of life, the symptoms soon began to amend, and in the course of about two months recovery was effected.

III.—John Moss, æt. 51, while sewing up a body, punctured the second phalanx of the fore-finger. In the course of the same day he felt extreme lassitude and headache. The finger so painful as to deprive him of sleep. The absorbents became inflamed in three distinct lines up to the elbow. Leeches applied to the finger. Fomentation to the inflamed parts. Next day lunar caustic was rubbed along the course of the inflamed absorbents—two inches in breadth. Antimonial mixture every six hours. Same night muriate of morphia administered with good effect. A puncture was made into the finger, and some sanious fluid discharged. The inflammation subsided, and the finger healed in a few days.

IV.—In this case also a puncture was received in the forefinger; the patient, æt. 69, being an attendant in a dissecting-room. The usual symptoms were manifested. Leeches, fomentations, and poultices, were applied. Lunar caustic was rubbed on, as in the preceding case, and the effect seemed to be the arrest of the inflammation. Convalescent in a few days.

V.—The same man received another wound in March last, but neglected it for three or four days, although the symptoms were severe. When examined, the absorbents were found inflamed, as were also the glands in the axilla. High fever, quick pulse, pain in the head, and delirium. Leeches, antiphlogistics, and other energetic treatment, employed; but the man sunk under excessive dyspnœa, followed by coma. At the *post-mortem*, the lungs exhibited the pathological appearances of pneumonia. The axillary glands had partially suppurated: other glands inflamed. Brain congested.

VI.—This was the case of Dr. Lee, who towards the latter end of August last, having a slight abrasion of the left forefinger, incautiously examined a uterus which he had in process of maceration. On the following day he felt pain, and observed swelling about the finger. A week, however, passed without any alarming symptoms, when suddenly shooting pain along the arm was experienced. Nervous and constitutional symptoms now set in. Leeches were applied to the fingers and palm of the hand, with fomentations. There was an obscure sensation of matter being formed: no opening was made. A splint, cold lotion, and absolute repose recommended. Under this system the finger has been gradually recovering its natural state; but still there is felt some pain in the part, nor is the swelling wholly removed.

In his general remarks on treatment, Mr. Stafford strongly recommends the use of the argentum nitratum; in the manner described in some of the preceding cases. This ought to be had recourse to immediately. The next step should be to evacuate the bowels freely. Leeches next should be abundantly applied to the swollen parts, followed by fomentations. Muriate of morphia is the best remedy for allaying the constitutional irritation. Of the expediency of general bloodletting the author entertains an unfavourable opinion, and refers to Dr. Duncan and others, whose experience in this respect accords with his own. Topical bleeding, however, he deems of the greatest consequence; and it is best effected by the repeated application of leeches. When it is ascertained that pus has been formed in the inflamed parts—and indeed whether pus be formed or not—when the patient evidently suffers from the tension of the parts, the swelling ought to be opened, and the incision wherewith this is done should be of considerable length. It is a remarkable fact that in most of the cases related by Dr. Duncan and others, where a swelling or abscess was formed without an opening being made in it, the patients did not recover; while on the contrary, where openings were made, the patients lived. Hence the propriety of free incisions even in the early stage of the swellings. In conclusion, the author suggests the proper treatment for supporting the system when the suppuration is established and the discharge abundant,—bark, wine, and nutrient diet.

The highest object of the healing art is to render itself superfluous.—Hufeland.

THE OTIC GANGLION.

We have examined a very beautiful wax preparation, just imported from Germany by Mr. Schloss, of Great Russell-street, representing the ganglion oticum of Arnold *in situ*, with the tympanum, and all the minute branches of nerves which form a communication between the ganglion and the organ of hearing. The foramen ovale is preserved entire, so that the exact situation of this famous *crux anatomica* is at once perceived. In Arnold's valuable plate (V.), among the *Icones Nervorum Capitis* (recently noticed in this journal), the ganglion is exhibited without the adjacent foramen: but nothing can be more satisfactory than the examination of both preparation and plate together. Running posteriorly from the otic ganglion we see a delicate nerve, which Arnold also claims as one of his discoveries, and calls the N. petrosus superficialis minor; beneath this another, running nearly parallel, which is called the N. tensoris tympani a ganglio otico. On the other side, running anteriorly, we observe the lingual branch, the tensor palati mollis, &c. There is also given in the preparation a view of the Gasserian ganglion, and particularly of the third branch of the fifth, together with some of the neighbouring arteries. The display altogether seems inimitable.

HENDERSON'S CUVIER.

FURTHER EXPOSURE.

To the Editor of the Medical Gazette.

Sir,

I regret to say that I can confirm the letter of your correspondent P., page 191, November 7, 1835, (which only fell into my hands this day) respecting an edition of Cuvier's Animal Kingdom now publishing by G. Henderson, 2, Old Bailey. It is true in all its particulars, and I could add to the series of *honourable* facts there stated, were it not for fear of the law of libel. I send you my address; and beg to inform P. that there are seven subscribers in Lincoln who are perfectly willing, and indeed desirous, of uniting—and union is our only resource—and as far as possible exposing the transaction. Meantime it may be as well to let P. know that we have all ceased taking the work in. It is to us matter of some amusement that Mr. G. Henderson should have fixed his abode in the *Old Bailey*. Verily he has been singularly felicitous in the choice of his neighbourhood. If P. will apply to you for my address, I will immediately correspond with him upon his sending me his.—I remain, sir,

A Late Subscriber to Henderson's Edition of Cuvier.

Lincoln, Dec. 14, 1835.

ANDRAL'S LECTURES.

A correspondent has directed our attention to the great discrepancy between M. Andral's lectures, as published in this journal, and in the Lancet. The circumstance is easily explained. Our object is to bring the opinions of the lecturer fully before our readers, in as short a space as is consistent with perfect clearness, and we therefore give the lectures, pruned of all redundancies, from reports now in course of publication in Paris, with M. Andral's express approval. As to the lecture of last week, to which our correspondent more particularly adverts, the fact is simply this: that while we began at the beginning of the Professor's discourse on *Encephalitis*, the Lancet began in the middle of it; altogether omitting the introductory observations of M. Andral, together with his account of the *causes*, the *anatomical characters*, and all the symptoms referred to the brain and nervous system; including the affections of motion, sensation, &c. Our correspondent will perceive the absurdity of supposing that M. Andral would limit the symptoms of inflammation of the brain to certain deranged functions of the stomach and lungs.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Dec.* 22, 1835.

Abscess	1	Heart, diseased	1
Age and Debility	22	Inflammation	20
Apoplexy	5	Bowels & Stomach	1
Asthma	12	Lungs and Pleura	2
Cancer	1	Insanity	1
Childbirth	2	Jaundice	1
Consumption	30	Liver, diseased	1
Convulsions	10	Measles	4
Croup	1	Mortification	1
Dentition or Teething	2	Paralysis	2
Dropsy	8	Scrofula	1
Dropsy on the Brain	3	Small-pox	9
Erysipelas	1	Sore Throat and Quinsey	1
Fever	4		
Fever, Scarlet	1		
Gout	3	Stillborn	11

NOTICES.

Mr. Munk's letter next week.

A letter directed to Ruricola will be found at our publishers.

G. B.—We shall be glad to receive the illustration.

A Constant Reader, Brighton, on giving us his name and address, will probably obtain the information he desires.

Several communications reached us too late: we are obliged this week to go to press a day earlier than usual.

ERRATUM.

In Dr. Macartney's paper last week, page 452, second column, *for* "suppuration," *read* "evaporation."

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 2, 1836.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

By Jon. Pereira, Esq., F.L.S.

Lecture XIV.

MOLLUSCA.

We have now arrived at the second division of animals, the *Mollusca* (from *mollis*, soft). They are distinguished by the absence of a skeleton either internal, as in the Vertebrata, or external, as in the Articulata. Externally they are protected by a soft lax skin, which envelopes them like a mantle. On the surface of, or within, this covering, there is sometimes deposited calcareous or horny matter, which constitutes the *shell*—as of the oyster and cuttle-fish. To the inner surface of the mantle are attached the motor muscles. The nervous system consists of ganglia and nerves: around the œsophagus the ganglia form a chain or collar, from whence these animals have been denominated the *cyclo-gangliata* (from *κυκλος, a circle*, and *γαγγλιον*). The circulation is double; the respiration branchial.

Orders.—Only five classes of Mollusca are now admitted—namely, *Cephalopoda*, *Pteropoda*, *Gasteropoda*, *Conchiphera*, and *Tunicata*.

Cephalopoda.

The only animal of this class which I shall notice is the *officinal cuttle-fish* (*Sepia officinalis*). The body of the sepia is lodged in a large fleshy or muscular mantle, within the substance of the dorsal portion of which is the internal shell, or *os sepiæ*. The head is furnished with two large eyes; around its base are eight long arms or feet, disposed in a radiating manner, and in the centre is the mouth, having two horny mandibles, like a parrot. Two of the feet or tentacula are much longer than the others. The internal surface of the feet is supplied with concave cups or suckers. Imbedded in the liver is a gland, secreting a dark inky fluid, which the animal discharges at pleasure through a part called the funnel.

The shell of the cuttle (*os sepiæ*) is composed principally of carbonate of lime, with a little phosphate and some animal matter. Here is John's analysis:—

	Hard Upper Portion.	Porous Part.
Carbonate (with a trace of phosphate) of lime	80	85
Non-gelatinous animal matter, soluble in water with some common salt	7	7
Gelatinous membrane, not soluble in water	9	4
Water, with a trace of magnesia	4	4
	100	100

It is not improbable that some hydriodic salt may be present.

Medicinal uses.—This os sepiæ is absorbent and antacid. It is used, when powdered, as a dentrifice.

ARTICULATA.

The third division of animals made by Cuvier, and at which we are now arrived, is that termed *Articulata*, from the articulated or jointed appearance of their bodies. In addition to this peculiarity in their external form and structure, must be added the disposition of their nervous system. This consists of a double series of ganglia, connected by nerves, and occupying the mesial line; or you may describe it as composed of two nervous cords, swelling out at intervals into ganglia. From this circumstance these beings are denominated by Dr. Grant the *Diplo-neura*. We find here no internal bony fabric around, and on which the muscles can be placed; but in its stead what has been denominated an external skeleton. This organ is, in fact, the skin, which covers, gives attachment to, and protects the muscles: it is frequently modified in its consistence, and in some instances possesses all the hardness, though not the strength, of the osseous system of the vertebrata. It is composed of several pieces, moveable on each other, from which circumstance the division has received its name.

Cuvier made four classes of them, namely, the Annelides, the Crustacea, the Arachnida, and the Insecta; and which may be thus represented in a tabular form:

			Classes.
ARTICULATA	blood red		Annelides.
	blood not red	antennæ and branchiæ	Crustacea.
		no antennæ	Arachnida.
		antennæ, no branchiæ	Insecta.

It has, however, been found necessary to add four other classes, namely, Cirrhopoda, Myriapoda, Rotifera, and Entozoa. Of these I may remark, that Cirrhopoda forms one of the molluscous classes in Cuvier's arrangement, though it agrees with the Articulata in its jointed body, and the development of its nervous system. Myriapoda formerly constituted an order of Insecta, from which they are distinguished by the number of their feet. Rotifera and Entozoa were placed among the radiate animals.

	Classes.	Examples.
Articulata, or Diplo-neura	Crustacea	Crabs.
	Arachnida	Spiders.
	Insecta	Cantharides.
	Myriapoda	Scolopendra.
	Annelides	Leeches.
	Cirrhopoda	Barnacles.
	Rotifera	Wheel animalcules.
	Entozoa	Intestinal worms.

Of these, I propose to notice first the

Annelides.

They are readily distinguished from other Articulata by their red blood, the softness of their integument, and the absence of true feet. We subdivide them into those respiring by branchiæ, and those without these organs.

		Orders.
Annelides	Branchiate	Tubicola.
		Dorsibranchia.
	Ebranchiate	Abranchia.

Some of the ebranchiate Annelides have bristle-like feet (the Chætopoda), others are without them (Apoda). In the latter division are the leeches, to which I must now direct your attention.

Hirudines, or Leeches.

History.—In commencing the description of an animal so extensively known and employed as the leech, a short historical account can hardly be uninteresting. We have, indeed, no accurate knowledge as to the exact period when these creatures either became known to, or were employed by, man; but this deficiency of information is not necessarily referrible to their discovery preceding the date of our historical documents. It is true that in the common version of our most ancient record, the Bible, we find Solomon saying, "The horse-leech hath two daughters, crying, give, give;" but critics are not

agreed as to the correctness of this translation. The word "Olukeh," or "Aluka," here interpreted "horse-leeeh," is by Bochart considered as meaning destiny, or fate, either of which terms should, according to this writer, be substituted for that of horse-leech; the daughters alluded to being Eden and Hell. But the Vulgate, Greek, and Lutheran translations, are all against his opinion. In Brandt and Ratzeburg's work you will find a very elaborate discussion of this subject, from which it appears that, in Arabic, the term *Aluka* indicates a leech, while *Aluk* signifies fate; the latter being derived from *Alaka*, to attach or hang to, because every man's fate is supposed to be appended to him, just as a leech affixes itself to the body; so that from this it appears probable the word "Olukeh," of the Old Testament, really refers to leeches. Nay, I think there is some reason for suspecting that the *Sanguisuga ægyptiaca* is the species referred to.

But admitting that these animals were known at this early period, it does not appear that they were employed in medicine: for Hippocrates makes no mention of them, though he notices other modes of drawing blood. Aristotle also is silent with regard to them. In the extracts which Cælius Aurelianus has made from the writings of Diocles, Praxagoras, Herophilus, Heraclides, Asclepiades, and other ancient physicians, who lived between the time of Hippocrates and Themison, no mention is made of the employment of leeches; a remarkable fact in favour of the opinion that they were not at this period in use. In fact, the founder of the methodic sect, Themison, is the first person in whose works we find leeches employed therapeutically. However, it does not follow that he was the first who prescribed them, though our documentary evidence fails in tracing back their use beyond him.

In the Latin and Greek languages, the animal has received its name from its sucking or drawing qualities. Thus the Greeks called it βδελλα, from βδελλω, to suck; the Romans *hirudo*, probably from *haurio*, to draw out; or *sanguisuga*, literally signifying "a bloodsucker," from *sanguis* and *sugo*. It would appear, however, that the latter of these two Latin terms is the more modern; for Pliny, in speaking of Elephants, says, "Cruciatum in potu maximum sentiunt, hausta hirudine, quam sanguisugam vulgo cæpisse appellari adverto."

Natural history.—It is most unfortunate that zoologists are not agreed as to the number either of the genera or the species of leeches. Linnæus and Cuvier admit only one genus, while de Blainville makes eleven. Linnæus, in his Swedish Fauna, included eight species, to which Müller added five or six new ones; so that Gmelin described fourteen altogether in his "Systema Naturæ." Since then other species have been added; and we find Savigny mentions eighteen, while de Blainville makes more than twenty, of some of which there are several varieties. Great confusion also exists as to the distinction of species and varieties. I may instance the common *green* and *speckled* leeches, which are regarded by some as mere varieties, by others as distinct species. In these lectures it is expected I should notice those kinds only used in medicine; I must, therefore, content myself with referring you, for an account of other species, and for some very interesting remarks on the natural history of these animals generally, to an excellent article by de Blainville, in the 47th volume of the "Dictionnaire des Sciences naturelles."

Of the Genus Sanguisuga.

The leeches commonly employed in medicine are referred, by Cuvier, to the genus *Hirudo*, which, in fact, includes leeches of all kinds; by de Blainville to *Iatrobdella* (so called from ἰατρος, a physician or surgeon, and βδελλα, a leech.) I prefer, however, adopting the term *Sanguisuga* employed by Savigny, since it is so expressive of their blood-sucking properties; for it appears that all leeches are not provided with teeth capable of perforating the skin of the vertebrata. Now this is an important fact in their natural history. In consequence of the numerous complaints addressed to the "Préfet de Police," in 1825, that of the leeches sold in Paris some would not bite, while others caused painful and obstinate wounds, he consulted the "Conseil de Salubrité," who deputed MM. Pelletier and Huzard *fils* to examine the subject; and one of the results of their investigation was, that the animal called in France the "horse-leech," and which had been particularly charged with causing painful wounds, would not, or could not, perforate the skin of the human subject; and on examining it anatomically, the teeth were found quite blunt. The reporters also state that this leech is the *Hæmopis sanguisorba* of Savigny, but de Blainville says it was the *Hæmopis nigra*.

Characters of the genus.—The following are the characters serving to distinguish the genus *Sanguisuga*. The body is elongated, the back convex, the belly flat, and the extremities somewhat narrowed, and furnished with disks or suckers. The body consists of from ninety to one hundred soft rings (the number not increasing with the age). The anterior extremity is some-

what narrower than the posterior; it is furnished with ten blackish points, called eyes, and a tri-radiate mouth with three cartilaginous jaws, each armed with numerous cutting teeth. The anus is extremely small, and is placed on the dorsal surface of the last ring.

Species.—The species are probably numerous, but I shall notice two only, *Sanguisuga officinalis* and *Sanguisuga medicinalis.* I ought, however, here to remark, that as their principal or only distinction is colour, some writers de (Blainville and Derheims, for example) regard them merely as varieties modified by the countries and places in which they are found. Without pretending to admit or deny the correctness of these opinions, I think it best, in these lectures, to follow Savigny, Brandt, Moquin-Tandon, and other excellent writers on this subject, who consider them distinct species.

Sanguisuga officinalis—This is the animal usually denominated the *green leech*, and which is called by Carena the *Hirudo provincialis*; by Risso, *Sanguisuga meridionalis.* In Germany it is called the *Hungarian leech.* It is distinguished from the other species by its unspotted olive green belly, and by the dark green back, along which are observed six longitudinal reddish stripes. Savigny mentions that six of its ten eyes are very prominent.

Varieties of this are described and figured both by Moquin-Tandon, in his "*Monographie de la Famille des Hirudinées,*" and by Brandt. Here are the characters of three: (*a*) the dorsal bands interrupted at intervals; (*b*) the dorsal bands reduced to blackish spots; (*c*) the dorsal bands united by transverse ones.

They are natives of the south of Europe, as the southern parts of France and of Germany, Hungary, &c. Those brought to England usually come from Bordeaux and Lisbon.

2. *Sanguisuga medicinalis.*—This is the kind preferred in this country, and which is usually sold in the shops. It is known by various names, such as the *true English*, or *speckled* leech. In Germany it is called *brown*, or *German* leech; and in France the *grey leech (Hirudo medicinalis grisea).* It is readily distinguished from the green leech (*Sanguisuga officinalis*) by its spotted belly, the base colour of which is yellowish-green, the spots being black. The number and size of these spots vary considerably—in some being but few, in others so numerous that they form the almost prevailing tint of the belly, the intervening spaces appearing like yellow spots. On the back are observed six longitudinal reddish or yellowish-red bands, spotted with black, and placed on an olive-green ground. The number of rings vary: ninety-three and one hundred and eight are the two extremes which have been met with.

The variations in colour are very numerous. One of the most remarkable is the *flesh-coloured leech* (*S. med. carnea*), described by Guillez, of Paris, as having the anterior half of the body flesh-coloured, while the posterior half was of the usual colour.

This leech is found native in almost all parts of Europe, but more especially in the northern parts. In England it is now rare.

Anatomy of the medicinal leech.—I propose to take a general view of the anatomy of this species of sanguisuga: I need hardly say it must be short, and, therefore, imperfect.

In the first place, let us examine the *cutaneous system.* The leech has two cutaneous layers, namely, the outer one, or epidermis, and an inner one, or corium. The first is transparent, and analogous to a serous membrane: when thrown off from the body (which is done every four or five days), it offers evident traces of the rings. The corium consists of a condensed cellular tissue, formed, according to Brandt, of globules: it shows the partitions into rings. It contains a number of globules, impregnated with a pigment, varying in its colour in different places, and which is the source of the colours presented by the surface of the animal. It is asserted that the predominant or base colour is, in part at least, owing to the colour of the soil in which the animals are found. Dr. J. R. Johnson, who published some years back a good work on this animal, says, "Mr. Baker, a man of some intelligence, residing in Glastonbury, and who for the last twenty years has been in the habit of collecting large quanties of leeches for sale, informs me that at the Black River, near Glastonbury, they are black, from the peat being of that colour; at Cook's Corner, they are of a reddish cast, from the red peat; while at Auler Moor, where, from a deficiency of peat, they penetrate the clay, they are yellow."

Connected with the cutaneous system must be noticed certain small granular bodies, found both on the belly and back of the leech, and which are particularly evident in contracted or old leeches; they give the animal a warty character; and are regarded as the mucous glands of the skin.

In the next place we may notice the *muscular system*, the most elaborate account of which, with drawings, has been given by Brandt. Firstly may be noticed the circular fibres which are observed in each ring, and are the most external of the muscular layers;

they may be called the annular muscles. 2ndly, Beneath these we have a layer composed of two sets of interlacing fibres, which are placed obliquely in reference to the circular fibres, and which cross each other at right angles. 3dly, Within these are the longitudinal fibres, which are more voluminous and developed than the others. They pass from one extremity of the animal to the other. In the posterior end, or foot, they expand circularly. 4thly, Numerous small muscles arise from the internal abdominal surface, at a little distance from the mesial line; they mount up, and are inserted in the back, between the longitudinal dorsal muscles. 5thly, More external than these, numerous small muscles arise, which are inserted between the muscles of the back. 6thly, In the posterior third of the body are five or six muscles, which arise from the back, and proceed obliquely downwards (so as to form an acute angle with the surface of the abdomen), and terminate in (7th) some longitudinal muscles running from the posterior end of the animal along the abdomen. 8thly, Fibres crossing each other are observed on the inner side of the anterior portion of the head. 9thly, Longitudinal fibres proceed to the envelopes of the jaws. Lastly, there are some annular fibres at the anterior part of the head.

The *digestive system* of leeches may, for convenience of description, be divided into the mouth, alimentary tube, anus, salivary glands, and liver. 1st, The mouth has a tri-radiate form—that is, it consists of three equi-distant lines, or rays, meeting in a centre. It is placed in the middle of the oral, or buccal disk (the superior circular edge of which is called the upper lip, the inferior one being termed the under lip.) Within this tri-radiate mouth are the three sublenticular jaws (called by some, dentiferous tubercles, by others, piercers), which are white, and of a cartilaginous appearance; but Brandt says they consist of a strong firm skin, inclosing a muscular mass. On the free, curved, sharp margin of each jaw there are about sixty small finely-pointed teeth. 2dly, The alimentary canal consists of an œsophagus, a long stomach with cæcal sacs, and an intestine. The œsophagus is a muscular tube, and commences between the inner angles of the three jaws by a roundish opening; it dilates as it approaches the stomach, but at its termination it contracts into a small circular aperture: the whole length not exceeding a quarter of an inch. The stomach occupies two-thirds of the length of the animal, and is divided into about eleven compartments or cells (regarded by some as separate stomachs.) Each of these divisions (from the second to eleventh) gives off on each side a sac (called a cæcal sac), those of the last cell being far the largest, and extending down by the side of the intestine as far as the commencement of the rectum; so that they have been compared by some (improperly as Brandt says) to the cæca of other animals. The stomach is described as possessing three coats, an outer or cellular, a muscular, and an inner coat lined with mucous glands. Posteriorly the eleventh cell of the stomach terminates by a funnel-shaped projection in the intestine. The intestine is about an inch in length; at its upper orifice is a valve, and at its lower end a sphincter: on either side of it, for the greater part of its length, is one of the sacs of the last compartment of the stomach; on its inner surface are several folds. It is divided into small and large intestine, the lower part of the latter being called a rectum. 3dly, The anus is not, as we might anticipate, in the posterior disk, but on the dorsal surface of the last ring. 4thly, Salivary organs have been described; they consist of whitish granular masses placed around the œsophagus, into which tube the common salivary duct opens. Lastly, de Blainville, Carus, and Brandt, speak of a liver. It is a brownish mass placed on the alimentary canal, the ducts opening into the stomach and intestine.

I shall conclude this account by quoting part of Dr. Johnson's directions for observing the several cells or partitions of the stomach. "Place," says he, "a leech fully gorged with blood in a saturated solution of corrosive sublimate in water. At the expiration of a week or ten days make an incision from the belly into the alimentary canal, and pick out with a needle the contents of each cell."

The *vascular system* next deserves our attention. It consists of four great pulsating vessels, giving off numerous ramifying branches; but without any heart, commonly so called. Two of these are placed laterally, a third in the median line of the dorsal surface, and a fourth on the abdominal surface. We know very little about the manner in which the blood circulates; indeed, Thomas asserts it follows no determinate order. However, it is generally supposed that the lateral tubes represent the pulmonary or branchial vessels, the dorsal the aortic, and the abdominal the venous vessel. I may add, that no globules have been found in the blood of these animals.

Where are the *respiratory organs* of the leech? If you examine the abdominal surface, you find arranged in two longitudinal rows little apertures, which have been called *stigmata, breathing holes*, or *spiraculæ*. They occur at every fifth ring, and

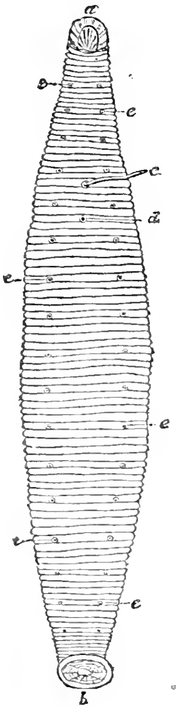

FIG. 81.—*Ventral Surface of S. medicinalis.*

a, Anterior disk.
b, Posterior disk.
c, Penis.
d, Vaginal orifice.
e, Stigmata.

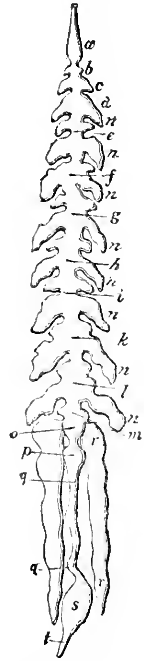

FIG. 82.—*Alimentary Canal of S. medicinalis.*

a, Œsophagus.
b to *m*, Cells of the stomach.
n, Cæcal sacs.
o, Funnel-shaped pylorus.
p, Irregularly expanded commencement of (*q*) the small intestine.
s, The large intestine.
t, The rectum.

lead into little cavities lined by mucous membrane, and which have been called *air sacs, pulmonary vesicles, mucous bags, cryptæ*, or *lateral vesicles*, containing usually a whitish fluid. They are placed on each side of the alimentary canal, in the spaces between the cæcal sacs of the stomach, and are usually regarded as organs of respiration. Brandt, however, asserts that the respiratory function is effected solely by the skin, and that these vesicles are, in fact, receptacles for mucus secreted by a neighbouring glandular apparatus, which has a whitish appearance, and in form represents a folded intestine. This notion, however, is not new, but was held by de Blainville and Johnson.

The *nervous system* of the leech consists of two parts: *one* composed of a chain of ganglia (usually about twenty-three in number), occupying the mesial line of the abdomen, and connected by a double nervous cord. The first (and largest) is placed on the œsophagus, and by some is denominated a brain; it supplies the eyes and the neighbouring muscles. The last one supplies the posterior extremity, or foot, exclusively. The *second* part of the nervous system is that lately discovered by Brandt, and may be regarded as a kind of sympathetic system. It consists of three ganglia (connected to the brain by filaments, and supplying the jaws), and a single nerve connected to them, and running along the abdominal surface of the stomach in the mesial line.

With respect to the *organs of the senses* in the leech, I may remark, that there are no traces of an organ either for smelling or hearing. Feeling resides probably in the whole external surface of the body, but is most delicate about the mouth. The apparent fondness of leeches for certain fluids, as blood, milk, &c. would seem to show the existence of an organ of taste. The organs of vision are ten black spots, arranged in a crescent form at the anterior or cephalic extremity. Dr. Johnson says preparations of them are best made by cutting off the head of the animal, and subjecting it to pressure between strong glass plates.

The *sexual* is the last of the systems which I have to examine. Each leech is bisexual, or androgynous, but there is no power of self-impregnation (the contact of two individuals being requisite, each acting to the other in a double capacity of male and female), so that they are called imperfect hermaphrodites. It has been thought, however, by some, that under certain circumstances self-impregnation can take place. Leeches have now several times been seen *in actu coitûs*, and drawings of them in this position have been published. The male organs consist of several pairs of testicles (called by Johnson *abdominal vesicles*), two vasa deferentia, two organs analogous to vesiculæ seminales (termed by Dr. Johnson, erroneously, testes), two ejaculatory ducts, and a penis surrounded by what some have termed a prostate gland. The penis projects from the abdominal surface at about one-third distant from the anterior extremity. The female organs consist of two ovaries, two oviducts (which subsequently unite into one), and a cavity (called by some uterus), which opens by a contracted aperture (vagina), at about the 29th ring, or about five rings below the penis.

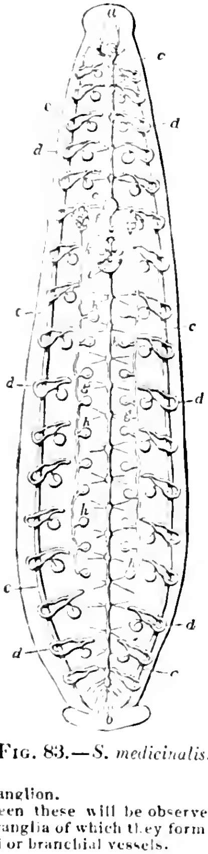

FIG. 83.—*S. medicinalis.*

a, Brain.
b, Last ganglion.
Between these will be observed the chain of ganglia of which they form portions.
c, Lateral or branchial vessels.
d, Folded mucous glands; each is connected by a duct to an air vesicle.
e, Penis, the rounded enlarged base of which is supposed to contain the prostate gland.
f, Vesiculæ seminales.
g, Vasa deferentia.
h, Testicles.
i, Uterus.
k, Ovaries.

There has been a difference of opinion as to the generation of leeches, some considering them oviparous, others viviparous; while a third class of persons, desirous of reconciling these opposite opinions, believe that sometimes the one, at others the second mode of development takes place.

That leeches are essentially oviparous, admits of no doubt; and we have now an admirable account of their development by Professor Weber, in Meckel's Archiv for 1828. Brandt has given a good abstract of this, with the necessary figures, from which the following description is in great part taken. It appears that soon after copulation an unusual activity pervades the ovaries, in consequence of which some *ova* (termed by Weber *germs*, by Carus *yelks*) are separated, and pass along the oviduct to the uterus, where they stop, in order to obtain the matters necessary for their development, and their proper coats. They here become invested with a serous-like membrane, on the inner side of which is produced (either by secretion from the uterine cavity or from the membrane itself) an albuminous whitish mucus, serving in part for the nourishment of the ova, and which is regarded as a kind of *liquor amnii*. Subsequently a glutinous fluid is deposited on the outside of the serous coat. When the ova are expelled from the uterus, part of this fluid gives a coating to them, while part is expelled before and after them. But this coat seems now distended with air vesicles, and has the frothy appearance of well beaten white of egg, probably produced by the violent contraction of the uterus.

The animals usually deposit their ova (in their own native waters) in holes or moist places on the shore, usually from May to the end of September. When first expelled, they are somewhat cylindrical in their form, and have a brownish appearance. The frothy layer adheres very slightly; but after lying in the water for a quarter of an hour, the outer surface becomes somewhat hardened, forming a kind of pellicle or fine skin. After some days a portion of this frothy covering is converted into a spongy tissue (*spongy coat of the cocoon*), covering the capsule of the ova (*cocoon*) wholly or partially. In this state the cocoon has a brownish, fibrous appearance, similar to fine sponge, and varies

somewhat in its size and weight; its longest diameter being from six to twelve lines, its shortest from five to eight, and its weight from twenty-four to twenty-eight grains.

The ova or germs, which have a lenticular form, enlarge, and evince vital movements; and very soon we perceive on each a funnel-shaped tube, extending from their surface inwards, and which appears to absorb the albumen of the cocoon. The ovum goes on enlarging, and becomes somewhat elongated, and subsequently the young leech begins to be developed on the exterior part of the ovum, the aperture of the funnel being the spot where the mouth of the young leech is observed. The abdominal surface of the animal is the first, the dorsal the last, to become developed. When the young leeches have attained a considerable size, they pierce their cocoon.

Diseases of leeches.—The natural duration of the life of leeches is not easily determined; but judging from the slowness of their growth, and the length of time full-grown leeches have been preserved, we may necessarily infer that they are long-lived animals. Dr. Johnson thinks that in their native waters, if they can always meet with an abundant supply of food, they may live at least 20 years. But they are subject to several diseases, some of which are epidemic, and of a very destructive kind. Although the study of the pathology of this animal is of considerable interest in a commercial and even scientific point of view, yet it appears to me to be of minor importance in these lectures, more especially as the discussion can lead to no practical consequences; for although the diseases of leeches are probably very few, yet we have a very imperfect knowledge of their nature and treatment. I shall therefore be brief on this point. Dr. Johnson, to whose work I have had several times occasion to refer, describes the three following diseases as common to this animal:—1st. An ulcer, seated in various parts of the body, but more generally affecting the side. It destroys life in a few days. 2dly, A rigidity and narrowing of one part, whilst another portion is studded with tumors of putrid coagulated blood. 3dly, A flaccid appearance of the whole body, except the lips, which are hard, swollen, purple, and frequently bloody. These diseases are particularly prevalent during the summer months. Brostat, in the fifth volume of Brandes's Archiv describes three epidemic disorders.

The mode of procuring leeches varies. They may be caught with the hand, or by a kind of net (described by Derheims), or by the gatherers going into the ponds with naked feet, to which the leeches adhere; or by baits, especially the liver of animals. The two latter methods are objectionable,—one because it is not free from danger to the gatherers, and the other because the bait is apt to injure the health of the animal.

The modes of transport and preservation may also be noticed. Leeches are sometimes imported in bags, but more usually in small barrels, each holding about 2000, the head being made of stout canvass to admit the air. The best vessels for preserving these animals are unglazed brown pans or wooden tubs. The dealers have a notion (and possibly a correct one) that the leaden glazing is injurious. These pans should be very little more than half filled with soft water (pond, river, or rain water). This does not require changing so often as is commonly supposed. In very hot weather, or when the water has become bloody, or otherwise much discoloured, it should be changed every day or so; otherwise, in summer every four or five days or a week; in winter, once a month is believed, by large dealers, to be sufficient.

The *consumption of leeches* must be enormous. Four principal dealers in London import, on the average, 600,000 monthly, or 7,200,000 annually. In France the consumption is enormous: Fee says, "it is estimated that 3,000,000 are annually consumed in Paris; and as the population of Paris is to that of the whole of France as one is to thirty-three, it follows that, independently of exportation, 100,000,000 are consumed annually, which is equivalent to three leeches annually for each person. Now, if we estimate the average price at fifty francs per thousand, we shall have the enormous sum of five millions of francs paid for this one article of our materia medica."

The effects and uses of leeches must be deferred until our next lecture.

LECTURES ON SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

BY DR. LATHAM.

Subject continued—Forms of Phthisis illustrated by kindred Forms of Disease in external parts — Unmixed Phthisis — Mixed Phthisis — Unmixed Phthisis commonly of long duration—Sometimes lingering in one stage, and reluctant to pass beyond it—Sometimes passing quickly through all its stages, but occupying small portions of the Lungs in succession; ceasing in one and beginning in another—General and Auscultatory Signs of unmixed Phthisis.—Is Phthisis curable?

I WISH to consider some important Distinctions of Pulmonary Consumption,

grounded on Pathology, and brought to our Knowledge by Auscultation during the life of the Patient.

You have all seen an Absorbent Gland of the Neck become as hard and as large as a marble, but without pain, or heat, or discoloration, of the integuments; and hard, and indolent, and marble-like, it has remained for weeks, or months, or years.

This is a mere deposition of Tubercular Matter in the substance of the Gland.

And you have all seen an Absorbent Gland of the Neck hard and large, and without pain, or heat, or discoloration, of the Integuments for a while; but presently pain, and heat, and redness, have arisen, and what was hard has become soft, and the Integuments have become thin, and have ulcerated or burst; and Pus has been discharged, and with it a hard nucleus of Tubercular Matter; whereupon the swelling, heat, and pain, have subsided, and the parts have been restored without any remaining mark of injury, save a slight scar.

This is a Deposition of Tubercular Matter followed by Inflammation in the Substance of the Gland. But the Inflammation is restricted almost, if not altogether, to the Gland itself; and it has no sooner done its work of eliminating the Tubercular Matter, than it ceases entirely.

In like manner you have seen many Glands of the Neck remain hard and indolent, or all or several of them go on to inflame and suppurate simultaneously, or in succession. But the Inflammation and Suppuration have not continued longer, nor extended further, than was needful for the purpose of eliminating the Tubercular Matter.

There is (what is called) the *specific* Limit of a disease. By this is meant the limit proper to its local morbid action, which, for any purpose it has to accomplish, it never need to transgress.

Thus in the instances alluded to, the specific Limit of the disease was strictly preserved; for if the Tubercular Matter was to be evacuated, no less degree of inflammation could have succeeded in bringing it to the surface.

But in such Tubercular Affection of the Cervical Glands, the Disease may spread beyond its specific Limit. It may give occasion to Inflammation both more severe and more extensive than is needed for the mere Elimination of the Tubercular Matter; to Inflammation pervading the whole Neck widely and deeply, and accompanied by diffused redness, and swelling, and pain; the whole subcutaneous Cellular Structure, between the angle of the Jaw and the Clavicle, being loaded with effused Serum and Blood, and numerous apertures dripping with pus.

And all this Inflammation, with its destructive processes, is engendered and spread abroad from a mere Nucleus of Tubercular Matter in a few Absorbent Glands. Yet in another case this same Tubercular Matter lay indolent and harmless, neither the constitution nor the part feeling any apparent inconvenience from it. And in another case it created just Inflammation enough (and no more) to produce a process of ulceration which might bring it to the surface.

Behold here, upon the surface of the Body, that very Disease which in the Lungs constitutes Consumption! Behold *here* transacted before your eyes the same morbid changes and processes which (allowance being made for difference of structure) are *there* transacted within reach of the ear!

There are cases in which Pulmonary Tubercles abide long, and, perhaps, never suppurate, or at a very late period; and there are cases in which Pulmonary Tubercles excite around themselves just enough of Inflammation and Suppuration to procure their own solution or evacuation, and no more; and again, there are cases in which Pulmonary Tubercles produce and spread abroad Inflammation of every degree and every extent throughout the Lungs, beyond what is necessary to produce their own solution or evacuation. And these cases are to be distinguished from one another by Auscultation. The Distinction is of vast practical importance.

Tubercles and Vomicæ are the specific part of Pulmonary Consumption. If you do not detect one or other of these, you cannot pronounce the Patient to be consumptive. The Auscultatory Signs which denote their existence have been already enumerated.

I shall take no notice of those cases in which a few Tubercles are deposited here and there in the Lungs, without any Auscultatory Sign of their existence. We find them after death, where they were never suspected during life, and where the entire Lungs beside are perfectly healthy.

There are no Auscultatory Signs which expressly denote Tubercles. Nevertheless, I have shewn that Tubercles may be detected almost infallibly by circumstantial evidence, to which Auscultation essentially contributes. But, before they can be thus detected, Tubercles must be so largely deposited in some part of the Lungs, as to impede perceptibly the passage of air through it.

Consumption is perpetually presenting

itself to me in this form. An individual loses the complexion of health, and becomes thin; he coughs a little; but perhaps he has no notable Fever, and no constant acceleration of pulse. I auscult his chest, and find a Dulness beneath one or both clavicles, or about one or both scapulæ, and a free respiratory murmur through every other part of the Lungs. Here there is no Disease beyond Tubercles; and while they occupy the upper Lobe, the whole Lungs besides are perfectly healthy.

This form of Consumption will endure for years and years, the Auscultatory Signs continually denoting the same thing, and the patient getting neither a bit better nor a bit worse in the meantime. But he is a wretched invalid, and finds that there is something continually incapacitating him for the severer business of life.

To such a person it is a continual puzzle why he does not get well. He consults an infinite number of medical men; and it is remarkable that he gets no comfort or satisfaction from those who understand his disease the best, and the greatest comfort and satisfaction from those who understand nothing about it. Those, who know what it is, out of kindness do not tell him the Truth, and they cannot asseverate a falsehood stoutly enough to carry any weight with it; whereas those who know nothing about it affirm boldly and unhesitatingly that *it is all stomach*, really believing that the whole and sole disorder is in the stomach, and that it is within the reach of an easy cure.

Surely Auscultation is so essential a Help for arriving at the Truth in such a case, that those who are skilled in the use of it must always agree as to what the Truth is: and, indeed, there is no wonder in *their* agreement: the wonder is, that those who do not arrive at the truth should so constantly agree in adopting the same fallacy. I have been somewhat curious in my inquiries concerning this matter, and the constancy with which I have found the whole malady imputed to the Stomach has appeared to me very strange. There is, however, a circumstance in the history of these cases which gives a colour of Truth to this opinion. The state of the Bowels is very frequently such as to demand the continual use of purgative medicine; and the Cough often comes on, and with it a kind of Asthmatic Breathing, soon after Dinner; and both continue as long as the Stomach is distended with food.

In this Form of Chronic Consumption spittings of Blood are apt to take place occasionally; and, when they do, they must give fearful Intimations of Disease of the Lungs to those who are not yet assured of it by Auscultation. But I have known *them* also imputed to the Stomach.

In this form of Chronic Consumption Abscesses are liable to occur by the side of the Rectum, and to degenerate into Fistulous Sinuses.

But in this Form of Consumption Vomicæ are not postponed *indefinitely*: they at length arise, and from that time the patient sinks rapidly. Often, when a fistulous Sinus has been cured by an operation, and the long abiding discharge from it abolished, an Expectoration of Pus will occur for the first time, and never afterwards cease.

From the first formation of Vomicæ the Patient sinks rapidly. In Pulmonary Consumption, characterised by the length of its Tubercular Stage (if I may so call it), and by a seeming reluctance to pass on to the formation of Vomicæ, when, after several years, Vomicæ do ultimately take place, it is often in great numbers simultaneously, or in very quick succession; so much so, that a Lung which two or three weeks ago was, in a great part, dull to perception, and yielded no sound to the ear but Bronchial Breathing or Bronchophony, will *now* give the clearest Auscultatory Signs that it is literally riddled with Cavities; and not only so, but, if the Patient survive a little longer, that many Cavities have run together, and a Multitude become one. The same simultaneous Gurgling when the Patient breathes, and the same simultaneous Plash when he coughs, will reach the ear from half one side of the chest.

It is remarkable how to the very last the sounds are *often* properly and exclusively those of *Phthisical* Disease, or rather those which it belongs to the essential conditions of Phthisical Disease in the Lungs alone to produce, and *those sounds only*. There are Cavernous Breathing, or Gurgling Breathing and Gurgling Cough, or Pectoriloquy; and in whatever parts of the Lungs you have not these, if you have any sound at all, it is the Vesicular Murmur of Health.

Nothing is more common, upon Dissection, than to find the Lungs most largely beset with Tubercles and Vomicæ; and at the same time every part of them, which a Tubercle or a Vomica does not absolutely occupy, altogether healthy.

Such is one Form of Pulmonary Consumption; and it would seem to be, in many striking circumstances, distinguishable from others. I may fairly wish that I had a less accurate knowledge of it; for that knowledge first came to me from observing its symptoms in two of my most valued Friends, and from watching in them, year after year, the sure but hesitating approaches of Death.

But Consumption is perpetually presenting itself to me under a different character. The Patient will live as long as he whose Disease is slow to advance beyond the stage of mere Tubercles. His condition, however, is different; and that condition varies more from time to time: he will spit for a while considerable quantities of Pus, and then cease from Expectorating altogether. He will suffer Hectic Fever, and then throw it off, and then suffer it again; lose his Flesh, and recover it, and then lose it again. Here, if you auscult the chest, you will find Cavernous Respiration or Pectoriloquy, a Gurgling Respiration or a Gurgling Cough at the Apex of one or both Lungs, and at every other part a clear vesicular Murmur.

These are the cases in which Pulmonary Tubercles excite around themselves just enough of Inflammation and Suppuration to procure their own solution or evacuation, and no more. The Phthisical Disease is carrying on its own specific processes within its own specific limits. It is depositing Tubercular Matter, and then maturating, and softening, and evacuating it; and the result is the formation of a Vomica. But, except in the seat of the Vomica, the whole Lung still remains healthy.

A very dear Friend of my own was twelve years dying of Consumption; and another Individual was twenty. They had an almost constant Expectoration, and Hectic Fever, coming and going during twelve and twenty years; but they died before the days of Auscultation, and, therefore, the exact Condition of the Lungs at different periods during the progress of their Disease was not known. I know a man, now living, who occasionally spits Blood and Pus, and who has occasionally spit Blood and Pus during the last twenty years. At various times during the last four years Auscultation has discovered a Vomica or Vomicæ at the Apex of one Lung, but, withal, a satisfactory Vesicular Murmur in other parts. This Individual, in what regards eating and drinking, has lived a life of abstinence, but a life of great toil in what regards exertion of body and mind. Sometimes his Friends are full of apprehension about him; his Hectic Fever, his Emaciation, his Cough and Expectoration, seem preludes to the worst event; but again he rallies, and his mind and his body recover, or seem to recover, their wonted powers.

But in this Form of Pulmonary Consumption, a time arrives at which there is no more resumption of the appearance or reality of Health, no more pausing between (as it would seem) the formation of one Vomica and another. The Hectic, the Cough, the Expectoration, continue; the Emaciation increases; the Strength declines; and Auscultation has no longer to seek the Gargouillement, the Cavernous Breathing, or the Pectoriloquy, in one spot, but finds them at all times any where between the Clavicle and the Mamma, or any where about the Scapula on one or both sides.

Here, too, however, it is remarkable, as in the other form of Consumption, that the Vesicular Murmur of Health is *often* heard to the last in all parts of the Lungs besides; and, upon dissection, that all parts are often found Healthy which a Tubercle or Vomica does not actually occupy.

The difference between the present form of Pulmonary Consumption and the former is this—that the former lingered long in the Tubercular Stage, Tubercular Matter continuing to be deposited year after year, but no Vomica occurring, until, at a very advanced period, many were formed simultaneously, or in quick succession, and hurried on the patient to Dissolution with great rapidity; whereas in the present, the Vomica, and Vomica only, is the Object recognised by Auscultation. Tubercle must precede it. But the Tubercle is hardly deposited before the process of softening and evacuating it arises, and a Vomica is the result. Thus Tubercle is formed after Tubercle (as it should seem), with some interval of time between, and Vomica after Vomica; but the Vomica is the more abiding Morbid Condition.

These are genuine and unmixed forms of Pulmonary Consumption; and I have dwelt upon them because they are so, and because I am indebted for my Knowledge of them, as distinguished from others, to Auscultation.

Of these two genuine and unmixed forms of Phthisis, the first is unquestionably the most hopeless. Where Tubercles are largely deposited, and continue still to increase, and do not pass on to Vomica, there is never the smallest attempt towards a restoration to Health—not even of a temporary or apparent Restoration.

But where Tubercles arise one by one, or a few together, and this one or these few pass rapidly into the state of Vomicæ,—and where a pause ensues between each successive Formation of Tubercles and Vomicæ,—then, during that pause, there is the opportunity for the powers of reparation to come into action; and, in truth, there often does arise a manifest endeavour after Health,—an endeavour which succeeds so far as to recover some of its conditions, and to suspend the disease: and then, during that pause, there is always the Hope (for where Disease is suspended and Health is partly

recovered, we cannot help hoping) that Reparation may be complete, and the Disease abolished altogether.

But can this be? Does Consumption *ever* admit of cure? If ever, it must surely be when it occurs in that form which we are now considering. Let us, therefore, now give a few moments to this interesting question.

A Vomica certainly admits of reparation so far as not to be a Vomica any longer, but not so far as to leave behind it no trace within the Lungs. It leaves behind it a scar—that is, the disease ceases in the part, but the part is not restored to the exact condition in which it was before the disease began.

In examining by Dissection the bodies of those who die of Pulmonary Consumption, among many existing Vomicæ we occasionally find the Traces of a Vomica healed. At the apex of the Lung we find an indentation, and descending from it, for half an inch or an inch, a thick perpendicular line of tough ligamentous substance. Sometimes this substance, by being pulled asunder, is discovered to contain the remains of a cavity, and sometimes not.

But what imports this Reparation of a single Vomica, if so many besides still exist? A Reparation of a twentieth part of the existing disease cannot be called a cure.

But in those who have not died of any pulmonary symptoms, and who were never known during their lives to have had any symptoms apparently phthisical, the same evidences have been found after death of what once was a Vomica, but no existing Vomicæ together with it.

This is a cure, or tantamount to a cure. It is as much a cure as when a single Scrofulous Cervical Gland goes on to suppuration and heals with a Scar. A single Vomica, you may say, is as much of the essence of Consumption as a hundred; and if the morbid structure (no matter how small) in which the disease essentially consists be repaired, the disease is cured—that is, the Consumption is cured. But it was a Consumption which nobody knew to exist.

Now all this may be very fine reasoning; but it does not meet the plain meaning of the inquiry whether Consumption be curable. It is not proof enough to common sense of its being so, that a few isolated Vomicæ, which give no sign of their existence, should have undergone Reparation.

All the world is asking us whether Consumption be curable? Indeed all the world is interested in the question: for there is hardly a family into which consumption, sooner or later, does not enter; and when a man makes the inquiry (as it were) speculatively, or indifferently, he has most likely a real practical interest in it at home. He says, "Is Consumption a curable disease?" But he *would* say, "I have a wife or a child, a brother or a sister, who is decidedly consumptive; is there the least possible hope left me that they can recover?"

To the question proposed with *such intent*, it is a mockery to answer "consumption *is* a curable disease"; for that its entire process from beginning to end—its formation, progress, cure—may be *secretly* transacted within the body without our knowing or suspecting any thing about it.

If you ask me, as a Physician, whether I have ever had experience of a perfect and satisfactory Recovery taking place, where there have been all the best known *popular* symptoms of Phthisis decidedly marked—symptoms which (*as far as they go*) no Physician could possibly say were not those of Phthisis—I answer "often."

But if you ask me whether I have ever had experience of the like perfect and satisfactory recovery where there were all these popular symptoms, and, withal, the conditions proper to Phthisis, ascertained by competent observers to exist beyond a doubt within the lungs—I answer "hitherto never."

What shall we say then? How shall we answer the popular question in the popular sense, and still answer it truly? We *cannot* say that Consumption is curable; but we *can* say (and truly) that there are cases of *imputed* consumption which put on such an aspect of the *real* disease that they are with difficulty distinguished from it, yet have not its essence. These are all within the possibility of cure.

We *can* say that there are cases essentially phthisical, in which the disease is so lingering in a particular stage, that many years are often required to bring it to its fatal termination. The Decline is gradual almost imperceptible, but sure. These fall within my first description.

Again, we can say that there are cases essentially phthisical (and these fall under my second description) in which the Disease accomplishes its course, as it were, by parts and parcels; many times apparently beginning, and many times apparently ending, but always (as far as I see) beginning again: a year two of Disease, a year or two of Health, then a year or two of Disease again. Yet, upon these terms, I have known those who have passed neither a short, nor a useless, nor an unhappy life. I have known those who have so gathered up the fragments of their broken health as to make them serve for high and useful purposes, and put to shame the fewer and smaller performances of stronger men.

OBSERVATIONS

ON

THE VENEREAL PRACTICE OF BERLIN.

By Alexander Ure, M.D., M.R.C.S.

Late House-Surgeon to the Royal Infirmary at Glasgow.

During the year 1834 I enjoyed a good opportunity of studying the forms of venereal affection most prevalent in Berlin, of ascertaining whether the therapeutic principles which governed their treatment were essentially different from those adopted in other countries, and of determining by minute observation the advantages or disadvantages resulting from their application in practice.

It is only in large towns, the seats of luxury and vice, that adequate scope is afforded for tracing the origin, progress, and termination, of this class of distempers, because a great number of individuals labouring under its effects in various forms are congregated in their hospitals. The incipient phenomena which afford the best clue to the more advanced symptoms can nowhere be investigated with so much facility as in Paris and Berlin, where the civil authorities carefully watch over the health of the females who live by prostitution, and thus detect the infection in forms rarely seen in the public practice of London.

The police, in their efforts to restrain one of the most offensive results of dissolute manners, compel these unhappy persons to undergo a strict examination once or twice every week, before a competent medical tribunal. Every female who is unsound, or who presents even suspicious symptoms, is sent to the public hospital, where she is very carefully examined; and if the judgment of the inspecting surgeon be confirmed, she is detained as an in-door patient, and not dismissed until perfectly cured.

In this way it is supposed that the disease, taken at the very commencement, if it cannot be speedily stopped, is at all events prevented from carrying its ravages to such an extent as to undermine the constitution. Thus also the chances of its being widely disseminated in society are greatly diminished.

Many intelligent observers have doubted whether such legislative enactments, as usually enforced, have any real utility. It is well known that, from hasty examination, the inspector has in numberless instances overlooked morbid phenomena, and thereby sanctioned by his certificates the circulation of disease. It would, moreover, appear from the recent researches of Ricord and Cullerier, that it is often difficult, sometimes impossible, to determine at once whether the affection be truly syphilitic or not.

In the great public hospital, the Charité of Berlin, four wards are allotted exclusively to the reception of persons labouring under symptoms of recent venereal affection. Such as are suffering constitutionally from inveterate syphilis are consigned to the care of the ordinary surgeon, in the *äussere station*, or common surgical wards. Of the above wards, two are set apart for males, and two for females. To the latter are admitted not merely the recognized women of the town, but females of every description, as servants, labourers' wives, &c. in whom any signs of recent contamination are to be detected.

The male wards are light, roomy, and well aired, containing forty beds; the female wards are still more capacious. Both sets are usually filled, especially the latter. Adjoining each is a small apartment, in which the attending physician, or, in his absence, the *stabsarzt*, makes his daily inspections of such as are able to go about. The others are visited in bed. Every patient undergoes a minute examination twice, at least, in the week; but the more important and urgent cases receive daily attendance*.

On the days appointed for examination, the persons found to be free from disease are dismissed by the physician. Under such a regime, the cures are rapid; so that the average number selected for dismission on these occasions, amounts to ten; which is about one-ninth part of the whole inmates. If, as often happens, the sole cause of their being sent to the hospital is some slight abrasion, a few days will suffice for treating it. If, on the contrary, from constitutional or other causes, over which medicine has no immediate control, the disease may assume an obstinate character, and resist the usual remedies, or, after having vanished for

* I feel bound to acknowledge the liberal permission afforded me by Professor Kluge to visit these private wards, as also the readiness shewn on every occasion, by the medical assistant, Dr. Kuhk, in facilitating my inquiries as to the nature and treatment of the affections in question.

a time, it may reappear, months will often pass over before it seems quite extinct. This is frequently the case with condylomata. I saw a young female confined during a period of six months, in consequence of excrescences occurring from time to time on parts which a day or two before had been apparently sound.

The average period of each cure, deducted from the comparison of a great many cases, was twenty-one days.

The recent forms of genital disease which usually occur in the Berlin Hospital, according to my own observations, are ulcers of the genitals, and condylomata*.

It would be no easy task to decide which of the two is of most frequent occurrence. Both are sometimes met with in the same individual, but such a complication is comparatively rare.

The sores are classed into the syphilitic and non-syphilitic. The former are round, or oval, deeply excavated ulcers, with hardened edges, lardaceous or dirty-brown surface, most commonly found in men, upon the corona glandis or inner folds of the prepuce; always attended with a loss of substance. They are usually designated the Hunterian chancre. The non-syphilitic comprise every description of sore destitute of the characters assigned to the preceding. They are for the most part superficial; their centre is of yellowish tinge, dotted with minute red points of granulation on the same plane with the margins of the ulcer, which are of the natural colour of the surrounding integuments, or else exhibit a pale-red hue. These appearances are by no means constant; for here, as in every other part of the body, is the aspect of a sore liable to modification from external as well as internal causes. The state of the abdominal circulation, for instance, exerts a considerable influence in this respect, as every person engaged in extensive practice must have remarked the livid red hue which they assume during the latter months of pregnancy,—an appearance which, viewed inconsiderately, might lead to fallacy in diagnosis, by inducing the observer to assign inflammation or irritation as the cause of what was merely an effect of impeded circulation in the sanguiferous trunks.

* Gonorrhœa is hardly recognized as a ground of reception.

Simple sores are frequently met with at the introitus vaginæ, from violent laceration of one or more of the carunculæ myrtiformes at their base. They are easily recognized by the partially detached, often ulcerated caruncula, which remains, commonly connected by a slender filamentous pedicle at one extremity of the ulceration.

Condylomata are those red warty-looking excrescences, of various form and consistence, with regard to whose origin and treatment great diversity of opinion prevails. They are commonly met with implanted on the surface of the genitals and neighbouring integuments, but they have been observed on the under surface and sides of the tongue, on the tonsils, on the eyelids, about the mouth and chin, and in the meatus narium and auditorius.

These excrescences are in Berlin recognized under two forms: the *marisca*, or soft condyloma, technically called *condyloma acuminatum*, from its pointed shape; and the broad hard condyloma, or *condyloma latum*, from its flatness. This distinction, derived from their organization, is of importance, both in reference to the prognosis and the treatment.

The common localities of the condyloma acuminatum are the corona glandis, the glans, the inner folds of the prepuce, the orifice of the urethra, and in some rare instances within the canal, the vagina, the os tincæ, both sides of the nymphæ, and the internal surface of the labia.

Their primary form is that of minute eminences supplied by delicate vessels, into which the colouring matter of the blood does not appear to enter. A superficial glance can discover only faint streaks upon reddish areolæ, resembling the efflorescence of certain exanthemata, of which it is not easy to convey a clear description. On closer examination, these paler coloured patches seem to consist of *phlyctenular* groups, from which a little whitish mucus exudes. In the course of a few days small insulated prominences, faintly perceptible by the naked eye, may be detected by gliding the point of the finger over the surface of the membrane. These becoming farther developed, progressively augment in volume, and ultimately, by a process of accretion, form a homogeneous mass, attached to the surface by a narrow pedicle, or reposing on

a broad sessile base. Their colour is a bright coral red; they are translucent at the edges. On minute dissection, they are observed to be not superimposed upon the mucous membrane, but to have sprung up through the epidermis or epithelium. They are endued with a delicate membranous envelope proper to themselves, of a smooth glistening aspect, constantly moistened with a thin whitish secretion. Their internal structure is composed of a soft cellular parenchyma, through which the vessels which organize and nourish them may be seen to ramify. To the touch they convey the sensation analogous to what is communicated by gelatinous polypi. They possess a high degree of sensibility, and bleed on being irritated with the finger or any pointed instrument. A writer of great ability defines them to be — "des groupes de vaisseaux lies par du tissu cellulaire, au lieu d'etre epanouis en membranes, ces vaisseaux sont rassemblés en paquets et constituent des arborisations verticales *."

In considering their structure it occurs to me that they might with propriety be termed endermic condylomata.

After these vegetations have subsisted for some time, their surface becomes irregular and rugose, from the presence of numerous elevated points and longitudinal furrows, and they assume the most anomalous shapes. Whence the older pathologists, whose attention was more directed to the form than the substance, have, in their parade of nomenclature, designated them under various appellations, such as ficus, morum, fragum, crista, thymus, porrum, myrmecium.

In some instances their rapidity of growth is truly astonishing. Cullerier has mentioned the following example of a man, in whom, eight days after impure sexual intercourse, a small pustule made its appearance on the left side of the gland. This had been scratched away; but was soon after replaced by a vegetation, followed by several others, which coalesced; and in the course of five months attained the size of a hen's egg, occupying the prepuce and gland†. In their early stage care must be taken not to mistake for them the sebaceous glands, which are often seen enlarged and of a bright red colour, scattered over the labia and vaginal orifice, particularly in females of a blond complexion: and the undue development of which, under the circumstances, Ritgen* conceives to denote a general morbid condition of the genital organs. These muciparous glands always present a minute central dark point, indicating the orifice of the duct, and want the areola which encircle the condyloma, and furnishes perfect diagnostic marks.

The high sensibility with which the condylomata are endowed will serve as an obvious means of discriminating them from the small insulated carunculæ at the intreitus vaginæ, which latter are not so tender as to prevent their being freely touched.

13, Charlotte-Street, Bedford-Squaoe,
Dec. 21, 1835.

* Mons. Desruelles in Recueil de Mémoires de Médecine, de Chirurgie et de Pharmacie militaires, tom. xxvii.

† Journal de Médecine, Juin 1834.

* In an ingenious paper, recently published, on the secreting papillæ of the vagina and uterus, it is endeavoured to be proved that these little prominences of the mucous membrane may be the seat of congestion, inflammation, suppuration, ulceration, nay even gangrene; and although their development may be compatible with a healthy condition of these parts, it may also indicate a state of morbid congestion.—*Ritgen on the Secreting Papillæ of the Mucous Membrane of the Vagina and Uterus in Neue Zeitschrift f. Geburtskunde*, Bd. II. H. 1, p. 1—15.

ON ELECTRICITY AND DR PHILIP'S VIEWS OF THE NERVOUS INFLUENCE.

By C. J. B. Williams, M.D. F.R.S. &c.

To the Editor of the Medical Gazette.

Sir,

I am reluctant to trespass again on the attention of your readers; for they must have perceived that, whatever impressions it may have produced in the minds of others, the discussion between Dr. W. Philip and myself has led to no new convictions in the minds of the disputants†. I expect no better success in this respect from the present communication; and my chief reason for troubling you with it is, to guard those who may not be well acquainted with the subject from being misled by some of

† I have delayed these observations until after the appearance of Dr. Philip's last letter, lest I should again incur the charge of having misunderstood him. Of that charge I would only remark that my reply was addressed to the precise expressions contained in Dr. Philip's letters. In his subsequent explanations he has modified those expressions.

Dr. Philip's statements of fact, in which he will find himself to be in error.

Dr. Philip says, in his last letter but one, p. 396, that *the electricity of electric animals produces no effect on the electrometer*. In Dr. J. Davy's paper on the torpedo, published in the Philosophical Transactions of last year, it will be seen that the electricity of the torpedo *not only affected the electrometer, but was transmitted through wires, magnetised needles, decomposed iodide of potassium;* and produced all the characteristic effects of the electric current except the spark; which it could not do for the obvious reason, that the structure of the animal prevents the application of those means of insulation which are necessary to give the current intensity enough to pass through non-conductors as a spark*.

Even before these researches of Dr. Davy, the nature of the electricity of the gymnotus and torpedo was pretty apparent, in its being transmitted by metallic wires, as well as in the characteristic shock which it produced in other animals. This form of electricity is, therefore, established on grounds equal to those of magnetic electricity, and far more ample than the slender points on which the analogy between voltaic electricity and the nervous influence at present stands. I may here add, that although the phenomena of electric animals may be adduced as an analogical argument to prove that animals can produce electricity; yet, the extraordinary specific apparatus which these animals possess appropriates to them the peculiar function which it is known to bear, and destroys their analogy with other animals in which no trace of such organs can be found.

Another error which I think it necessary to correct is contained in the following passage: Dr. Philip says, "Carbon and oxygen will not combine by mere juxta-position either in the living blood or in the laboratory of the chemist. In the latter they may be made to combine by the electric power; in the former, as appears from facts I have stated, by the nervous influence; so that here also we see electricity possessing the same property as that influence."

Now these assertions, as far as they relate to the living blood and the nervous influence, constitute a *petitio principii;* and as far as they refer to the laboratory of the chemist and electricity, they are contrary to fact. Carbon and oxygen *will unite by mere juxta-position* in the laboratory of the chemist,—witness the innumerable cases in which dead organic matters, both animal and vegetable, convert the oxygen of the air into carbonic acid. Several instances of this are mentioned in my paper on respiration and animal heat; and we cannot take a more appropriate example than the blood itself; which, for days and weeks, even when it has become putrid, will still continue to absorb oxygen and evolve carbonic acid. Here, then, if the union of carbon and oxygen depend on electricity at all, it is, as I have before said, on the intrinsic electricities of the uniting matters, and not on any from without. Further, electricity, *quasi* electricity, shows no power of making carbon and oxygen unite where they otherwise would not unite. An electric current will not cause charcoal to unite with oxygen, unless the current develope *heat sufficient to burn* the charcoal at its usual point of combustion. Heat, not electricity, is here the immediate agent; and it exalts the chemical affinities in the usual way, in a manner that cannot find a parallel in liquid blood.

The passages corrected in the preceding remarks are the only parts of Dr. Philip's reply which appeared really to affect the arguments of my last communication, to which I again refer for a statement of the true position of the questions at issue. I shall now only say a few words on the leading facts on which Dr. Philip's views are founded, and to which he complains I have made no reply.

1. When the eighth nerves are cut in the neck, and the divided ends kept asunder, the secretion of the gastric juice is generally suspended*. When simply divided, with little separation or loss of substance, this effect is not produced. Hence Dr. Philip concludes that the influence conveyed by these

* Mr. Faraday is endeavouring, with the aid of the government authorities, to have some gymnoti brought to England for the purposes of experiment. The prodigious electric powers possessed by these animals, and the consummate knowledge and skill of the experimenter, lead us to hope for the most interesting results from such investigations.

* I say *generally*, because the experiments of Breschet and Edwards, and of Leuret and Lassaigne, positively show that this result is not constant.

nerves can pass through a small portion of intervening texture like electricity, and *therefore* that this influence is electricity. But this is the only point of resemblance between living nerves and conductors of electricity: in all other respects there is a total difference. The nerves have no known structure which can enable them to conduct electricity better than the solids and liquids that surround them; nor has electricity ever been detected in them during life; although tests have been used which would have detected an electric power infinitely lower than that which could produce the chemical effects ascribed to the nervous influence. Still more opposed to this view, is the fact, that of several nerves in the *same sheath*, some are affected by electricity, and others not; yet these nerves present no physical differences to render some better conductors than others.

2. When the secretion of gastric juice is suspended, and the respiratory function disturbed by the division of the eighth nerves, these functions may be restored for a time by passing an electric current through the nerves. Electricity is also capable of exciting contractions in voluntary muscles. From these facts Dr. Philip concludes that electricity can perform all the functions of the nervous influence, and therefore that the two powers must be identical. But it is not proved that electricity acts in these cases otherwise than as a powerful and effectually-applied *stimulant*, exciting those vital energies which had been paralyzed by the injury to the divided nerves. And it favours this last supposition (as Mr. J. W. Earle has justly remarked), that animals thus acted on by the electric stimulus die much sooner than where the electricity is not applied; in one experiment in two hours and a quarter, in which time it is most improbable to suppose that "inflammation of the lungs, without change of structure," could prove fatal *. Further, electricity does not appear to have the power to restore other secretions, as that of the kidneys, when suppressed by injuries to the nervous system; nor does it counteract the effect of these injuries in depressing animal heat. (See my Experiments.) Lastly, the power of electricity to excite a muscle to contract is shared in common with other irritants, mechanical and chemical, which in no known way can excite electricity, and are inferior to this agent only because they are less effectually applied and diffused.

There are many other objections which oppose Dr. Philip's views in the present state of our knowledge; but I have said enough to substantiate my former statement, that *at present* these views can be considered only as ingenious hypotheses. It is no proof of their fundamental truth that a systematic exposition of the phenomena of life may be based upon them. The systems of Brown, of Broussais, and of many other speculative writers, were framed on views which also were partially founded in nature; but their generalizations were constituted on too narrow and partial a basis; and although beautiful and systematic in their structure, they necessarily crumble under the weighty engines of philosophical criticism.

So also, until the fundamental points of Dr. Philip's views are rendered more unequivocal in their character, and are further established by a proved conformity with ascertained laws of the living and dead matter to which they refer, no ingenuity bestowed on the system erected upon them, no apparent sufficiency of their explanatory applications, will avail to gain for them that respect and homage which are due to the truths of science.

Here, then, is the field for further investigation, in which Professor Faraday has told Dr. Philip that he has "great expectation we shall sooner or later succeed;" but until that success is obtained, dogmatising on imperfectly-established views will retard their reception far more completely than that philosophical opposition which discovers the weak points in their foundation, and demands further research to establish them.

I am, sir,
Your obedient servant,
C. J. B. Williams.

Half-moon Street, Piccadilly,
Dec. 22, 1835.

* I know not on what ground Dr. Philip assumes inflammation of the lungs to be the cause of death in animals subjected to galvanism. His own description of the lungs in this experiment is that they "were rather redder than natural, but otherwise *quite healthy*, collapsing perfectly when the thorax was opened." This is very unlike a fatal inflammation!

MISCELLANEOUS PRACTICAL CASES.

To the Editor of the Medical Gazette.

Sir,

I beg to inclose you a few "Selections from my Case-book;" your insertion of

which, if you consider them worthy of a place in your valuable periodical, will much oblige,

Yours very truly,
P. M. LYONS, M.B. A.M.
Physician-Accoucheur to the Brighton Lying-in Institution.

23, Regency Square, Brighton,
Dec. 22, 1835.

Case of Malposition of the Heart, &c. with Imperforate Vagina.

Harriet Parkes, aged 20, a patient of the Dispensary attached to the Lying-in Institution, had been very healthy until eight years ago, when she contracted a severe cough, with considerable expectoration, of a character similar to her present sputa, and occasional fits of hæmoptysis. She was under medical treatment for six months, and then had an intermission of three months; after which the cough returned, and has not ceased since.

During the whole of this time her appetite and bowels have been pretty regular. Has never menstruated or had leucorrhœal discharge except once, about two years ago, when, while suffering from an attack of what she calls "influenza," she noticed a slight glairy discharge; but this having once disappeared, never returned. About four years back had regular monthly hæmoptyses, which continued four months, and on each occasion lasted one day. Was induced to notice this from its periodicity, in which it differed from the preceding attacks. Since that period has had no return of hæmoptysis. About three years ago was under the care of a surgeon, who gave her emenagogue medicines, that produced considerable pain about the loins and thighs, but without any further effect. Except on that occasion she has never experienced any thing at all resembling a menstrual erethism.

March 18th, 1835. — Present state: tolerably fleshy, though rather leucophlegmatic; appetite good; bowels regular; occasionally troubled with flatus; urine natural; sleeps well; cough severe during the day, with muco-purulent expectoration, which varies in quantity, but on an average amounts to ℥iv. *per diem*, and which, unless immediately yielded to, produces severe headache. On most occasions a mere "hawking" will bring on the expectoration in great quantities, and of a very offensive odour. Pulse 90, small and soft; chest proportionate; mammæ sufficiently developed. No emaciation of chest, though there is a remarkable depression under the right clavicle; one, not so marked, under the left. During respiration the ribs of both sides are equally elevated and depressed, and the intercostal spaces are equally well marked on either side. On right side of thorax, between third and fourth ribs, or at two and a half inches from upper edge of clavicle, and two inches from right margin of sternum, a pulsation is *perfectly visible*, which exactly corresponds to what is produced by the apex of the heart striking against the ribs; while no such phenomenon is perceptible on the left side. On percussion anteriorly, the left side sounds clear, from the clavicle to the tenth rib; but on the right side there is a strongly marked dulness in the region of the first four ribs, which becomes still more appreciable as we pass over the mammary region. The heart's action can be traced over the whole of the right face of the chest. The sternum sounds clear through its extent, though somewhat duller towards the right side; right axilla duller than left; ditto of posterior surface of thorax.

Stethoscopic examination.—Respiration pure over whole face of left side, mamma included; action of heart audible, though slightly, over central portion of mammary region, and somewhat more distinctly more inferiorly. On right side muco-crepitus over humero-clavicular space, which, though it becomes less perceptible as we descend, being masked by the heart's action, is audible as low as the inferior edge of the mamma.

The chest being emptied of the muco-purulent matter by a fit of coughing, cavernous respiration becomes very distinct, and, like the muco-crepitus, is perceptible in the axillary region of same side, as also on posterior surface, where it assumes the character of bronchial respiration. In right humero-clavicular space pectoriloquy (?) is painfully distinct, and extends over the same region as the before-named phenomenon, and with similar alterations and exceptions. At the left side neither of these phenomena is at all discoverable. False ribs descend very low; dulness on percussion over those of right side, but clearness, with respiratory murmur, over same part of left. Bor-

borygmus slightly audible in left hypochondriac region; and when she drinks, the gurgling is perfectly distinct in left hypochondrium, scrobiculus cordis, and over left portion of right hypochondrium, but not at all in right side of latter region.

The result of the preceding investigation having led me to inquire how far the *suppressio mensium* might be dependent on an irregularity in the sexual organs, similar to that in the thoracic viscera, I was induced to request an examination, when the labia and nymphæ were found sufficiently developed, but no os uteri could be detected; while the mucous membrane lining one labium was found to be equally and smoothly reflected over the other, leaving no space or vacancy except for a very small dip or depression, which terminated in a cul de sac.

Having now established the existence of two remarkable facts, first, that the heart was misplaced, and not displaced; and secondly, that there was an adequate explanation for the suppression of the menses; and considering that this last-named functional deficiency kept up, if it did not give rise to, the disease in which the lung was involved, I felt desirous to relieve the chest by establishing (if possible) a menstrual flux; and was supported in this opinion by my friend Dr. Jenks, to whom I feel indebted for the acuteness with which he entered into the investigation of all the different phenomena, which tended to elucidate the various peculiarities of the case, particularly the position of the heart, which *he* shewed must be inverted.

March 19th.—A consultation was this day held—present Dr. Jenks, Mr. Lawrence, and Mr. Kell—with the intent of ascertaining if we might cut down upon the mouth of the uterus; and having learned the real state of that organ, decide whether we would be justified in having recourse to emmenagogues. On this occasion it was finally determined that the symptoms were not yet such as to warrant an operation, or the use of medicines of the above-named class.

22d.—I have daily marked her expectoration, which amounts to four ounces *per diem*, and is strictly of the character already described.

April 1st.—Has this day brought me three ounces of expectoration, in which she says there were traces of blood when it was first ejected.

May 15th.—Has presented herself to me for the first time after a lapse of six weeks; complains of severe pain over anterior of abdomen, and in right lumbar region, especially when she sits. Considering these as merely symptoms of colic, I directed her—

> Subm. Hyd. gr. iv.; Pulv. Opii, gr. ss.; with a carminative rhubarb draught.

16th.—Has had several inefficient attempts to relieve her bowels; complains of some pain in sitting posture, and of a sensation of bearing down, as though some substance wanted to pass per rectum. Urine scanty and loaded, even when recently passed; she then experiences temporary relief.

> Ordered an emollient enema, and warm fomentations to the abdomen and loins.

17th.—Was informed by the person who administered the enema, that there were several large piles protruding from the rectum.

> Directed her some soothing alterative medicines.

30th.—Under the influence of those medicines she has passed her time comfortably, and without requiring to see me until this day, when she sent to request I would call on her. I now found her complaining of increased distress from her former symptoms, particularly the difficulty of making water, and bearing down per rectum. I therefore now proceeded once more to institute an examination of those parts, when I discovered a tumor filling up the space between the labia, and almost protruding into the external parts; it was firm but elastic, extending from the urethra back to the perineum, and in shape and size like a section of a hen's egg.

Examination per rectum.—A large tumor, connected by alæ with either side of the pelvis, projects forwards towards the bladder, and backwards so as almost to block up the rectum. The finger being carefully carried over it, it appears to retain the form of the unimpregnated uterus, though very much enlarged. On percussion being applied with the point of the finger to the base of the tumor, the succussion can be distinctly felt externally; and the external parts being tapped, the sensation is immediately conveyed to the base of the tumor. On exploration no tumor could be detected in the abdomen; but pressure being applied deeply and

forcibly, the tension of the tumor was considerably increased.

Having now determined both the nature of the case, and the only feasible mode of relief, I proceeded on the following day, in the presence of Dr. Jenks, Mr. Davies, R.N., Mr. Kell, and Mr. Best, surgeons to the Lying-in Institution, to make a free incision into the vagina, when there gushed out twenty ounces of a brown, streaked, thickish fluid, closely resembling tar in all its qualities, smell and viscidity excepted. A dossil of lint was afterwards inserted into the opening.

June 2d.—Has had a free discharge of limpid urine since the operation; bowels have been relieved spontaneously; no fever; a considerable quantity of menorrhœal fluid has strained away, the colour of which is now beginning to be changed. On examination, the os uteri, which is thick but very patulous, is in immediate approximation to the opening; the transverse rugæ are very perceptible to the touch. I now introduced a female syringe, and continued it for three hours a day during four days, after which it was replaced with a sponge tent, the parts were thoroughly injected with tepid water, and on the sixth day the discharge became free from colour, &c.

10th.—Says she does not spit up within one-third of what she used to do previous to the operation; cough and hawking less troublesome.

12th.—Has had a slight coloured discharge, which began on the 10th, increased in quantity yesterday, but is less in quantity and paler to-day.

July 12th.—Has had occasional exacerbations and remissions of cough since last report, but has been generally going on well since. Expectoration never more, sometimes less, than two ounces *per diem.* Has had no menstrual return since 12th of last month, but an increased leucorrhœal discharge during the last three days; looks and feels much improved in her general health; chest sounds clear on percussion; mucocrepitus hardly perceptible, except on coughing, when it becomes very distinct over the inferior anterior portion of the right thoracic cavity, and over all the greater bronchial tubes of the same side. Vocal resonance much less distinct, except posteriorly; indeed, anteriorly it hardly enters the tube of the stethoscope.

Dec. 19th.—Has continued to menstruate, with some irregularity, constantly since July. Her cough, which during the summer months appeared to improve, has resumed its original severity; and in point of expectoration, as, indeed, in almost all the other pulmonary symptoms, she is in much the same position as that in which I originally found her; but her health withal does not appear to suffer. I have recently introduced a seton over the right side, and am trying the effect of the terebinthinates, but as yet with nothing like marked success.

REMARKS.—The foregoing case, full as it is, calls for few if any remarks from the author, save an apology for publishing it in its present unfinished state; but when it is remembered that it has already extended over a period of eight years, and may probably reach as many more, without the patient's health undergoing any material alteration, the author feels that he will be excused, if not justified, for wishing it to be now placed upon record.

Case of excessive Quantity of Opium being taken without fatal Results.

The mother of Marianne Croskey, a healthy infant, aged four months, and residing at No. 1, Little St. James's-street, states, that on leaving home on the 5th of May, 1834, she left her child in care of a woman who was an inveterate opium-eater, and who shortly after gave the child a lump of opium, which she describes as having been of the size of a small pea, and a third less than her own ordinary dose. Coma immediately set in; and the woman being alarmed, went, at 8 A. M., to a respectable practitioner, who, seeing the child livid and comatose, and having heard the quantity administered, pronounced that the child could not recover, and therefore declined taking charge of it, though he subsequently saw it at 12, and again at 2 P. M.

At 3 P. M. the parents sent to request that I would go and see the child; but being from home when the message was left I did not see it till 4 P. M., when I found that the midwife of the Lying-in Institution, who had arrived shortly before me, had put the child into a warm bath, and was briskly rubbing the abdomen and limbs. State of the child,—sensation and motion none, countenance livid, pupils dilated, no pulse at either wrists or arms, heart feebly beating

45 per minute, The child being in the bath, I directed a full-sized teacupful of warm water, with a teaspoonful of castor-oil and a saltspoonful of common salt to be administered as an injection; and proceeding to a chemist in the neighbourhood, procured an ounce of vinum ipecac. and the following mixture:—

℞ Sp. Am. Arom. ʒss.; Sp.Anisi ♏xx.; Syrupi Croci, ʒij.; Aquæ Anethi, ʒxij.; ft. Mist.

I now proceeded to give the child a teaspoonful of the stimulant, with one of warm brandy and water, every quarter of an hour. This was at first done by putting the spoon far back towards the pharynx, and then closing the mouth upon it. After the first three doses vitality seemed slowly to return. I now began to add a teaspoonful of the vinum ipecac. to each one of the stimulant, and repeated this every ten minutes until vomiting was produced, which commenced at 5½ P.M.; when the infant being put into another warm bath, and a second injection administered, the bowels were relieved of a dark, offensive stool, but possessing scarcely any odour of opium. After this, stimulants were continued every quarter of an hour until 6½ P.M., when the breast being milked into its mouth, the child was sufficiently revived to be able to swallow of itself. I now left it, having first given directions that a teaspoonful of oil was to be administered every hour, and an injection every two hours, until the opium was voided, recourse being had in the interim to occasional small doses of the stimulant. 8 P.M. saw the child again, who is considerably revived. 10 P.M. Bowels have been relieved three times since last report: the last motion smelt strongly of opium. The baby has taken the breast freely, and for some time. The castor-oil to be repeated every two hours, until I see the child in the morning.

May 6th, 11 A.M. Am informed that at 9 o'clock this morning there was found on her napkin a small, softish, but firm lump, of a deep black colour and strong smell, which had been put by for me, but was subsequently removed by one of the neighbours. From this time the infant had not a single bad symptom. I am informed that the surgeon who first saw the child, and who, like myself, saw the mass from which the lump was taken, pronounced it to be crude opium, which was also my own conviction.

Case of General Dropsy, complicated with Enteritis.

Wm. Furnnell, aged 6½ years, was healthy as an infant, but during the last five years has been subject to periodical attacks of diarrhœa, recurring monthly, and lasting each from three days to a fortnight. Has suffered at times from acute pains in the kidneys, which were generally relieved by vomiting: immediately after his first attack of this kind, passed a calculus of about the size of a pea. Urine usually very scanty. Six months previous to my seeing him, had an attack of diarrhœa, for which he was admitted as an out-patient of the General Dispensary, and attended by Mr. Parsons (then house-surgeon), who, viewing the case as one of enteritis, treated him first antiphlogistically, and subsequently with astringents; under which mode of practice the enteric symptoms disappeared, but his face and upper extremities began to swell, and, on their subsidence, the abdomen and lower extremities assumed a dropsical tendency. He was now put upon a course of diuretics, when the enteric symptoms immediately reappeared. I was therefore requested to visit him, in my then capacity of physician to the General Dispensary.

September 20th, 1833.—Present state, no great emaciation; countenance pale, not bloated; pulse 90, hard; tongue coated at the centre, tip and edges red; papillæ rather prominent; abdomen, superior portion tympanitic in erect position, but fluctuating when supine. Measurement around umbilicus 30½ inches. Scrotum and prepuce transparent, and so distended as to compress the orifice of the penis, the whole body of which organ is excessively twisted, whence pain extends along the course of the spermatics. Lower extremities very much swollen; tenderness, amounting to pain, on pressure, along the whole tract of the colon. Stools purged, mixed with pus, and what looks like small shreds and larger patches of the lining membrane of the intestines. Urine scanty, not more than ℥xij. in the twenty-four hours, of a pale straw colour, inodorous, and coagulating on the application of heat.

Directed that no food or drink be given him under *any circumstances*, ex-

cept one pint of milk in the twenty-four hours, and that in minutely divided portions; the scrotum to be punctured in different places with a cambric needle; ʒss. of mercurial ointment to be rubbed into the sides of the abdomen at bed-time, and one of the following pills twice a day:—

℞ Pil. Hyd. ℈j.; Pulv. Dov. gr. xij. ft. Pilulæ viii.

22d.—Scrotum has not been punctured; pain along the course of the spermatics intolerable. My friend Mr. Haviland, one of the surgeons to the Dispensary, being then with me, at my request freely punctured both scrotum and prepuce. The bowels being irritated by the use of the blue pill, I determined to omit all internal medicine, and abide the result of inunction, which was directed to be applied twice a day to the abdomen, groins, and axillæ.

25th.—Breath redolent of mercury. Though stinted to his pint of milk, has voided, in the twenty-four hours, two quarts of urine, which gives evidence of the presence of a large quantity of albumen. Measures 25 inches across the umbilicus. Scrotum and penis very much diminished, but still exuding. Pergat.

27th.—Discharge of urine increased by a pint per diem, but still albuminous. Measurement of abdomen 22 inches. Scrotum and penis of their natural size; perforations healed; lower extremities very much diminished. Has had but two motions each of the last two days, both of which were of a reasonably healthy character; still some slight tenderness on pressure. Pergat.

October 3d.—He has gone on favourably since last report; no change has therefore been made except in his diet, which has been gradually improved by the addition of an egg beaten up in the milk; some plain animal jelly, and a very small quantity of bread. The mercury appearing to have completely affected his system, I have this day discontinued the use of it, and ordered him three tepid baths, with a day's interval between each.

8th.—On his taking the second bath (which was salt water, all our public baths, with one exception, being of that description), the abdomen began to swell rapidly, and his urine to be diminished in quantity, but increased in albuminous quality. Omit the baths, return to the ung. hyd., and let one of the following powders be taken twice a day:—

℞ Hyd. c. Creta, ℈ij.; Pulv. Dov. gr. x.; Pulv. Calombæ C. ℈ij. Ft. pulveres decem.

18th.—Since last report he has been steadily improving up to this day. Present state, bowels rather confined; pain in the region of kidneys; urine scanty and highly albuminous.

℞ Sp. Æth. Nit. ℥ss.; Potass Nitrat. ℈j.; Syrupi, ℥ij.; Aquæ Puræ, ℥vi. M. Coch. Parv. e. Decocti Hordei Cyatho vinoso; ter die sumat. Rep. Pulveres, Olei Ricini, ʒj. quoque mane quum opus sit. Ung. Ant. Tart. ʒij.; Adipis Nullæ, ʒij. tere; lumborum lateri utrique nocte infricetur, ʒj.

20th.—Pain in kidneys has disappeared; urine more copious, scarcely albuminous; abdomen has again returned to its natural size.

Pulv. Calombæ C. ʒj. in par. xij. ter die. To be removed to the country as soon as possible.

24th.—Every unfavourable symptom having disappeared, he goes into the country this day, taking with him 24 powders.

November 1st.—His mother informs me he has had no unfavourable symptom since he left, but is gaining flesh fast, and beginning to enjoy all his boyish amusements.

This boy has not had a return of his attack since, and is now a strong and healthy lad.

Remarks on the foregoing Case.—The dropsy was evidently dependent on renal irritation, but complicated with enteritis, so as to render inadmissible the use of ordinary diuretics. Mercury, therefore, appeared to be the agent most capable of acting with advantage over both affections; but even this remedy was not available by the mouth, as, when so administered, it increased the irritation of the bowels: inunction seemed, therefore, the most advisable form for administering this mineral, to which was united a system of dietary which, while it afforded a sufficient degree of mild nutriment, greatly diminished the ingesta, thus at the same moment removing a very fruitful source of irritation from the bowels, and, by relieving the absorbents, allowed them to expend their increased action upon

the extravasated fluid; a mode of practical dietary which, though not in exactly similar cases, has been had recourse to with advantage by Dr. Wm. Hunter and Mr. Hey, of Leeds; as noticed by Dr. Todd, in his valuable Treatise on Indigestion *; and which was the more admissible in this case, as my little patient was by no means deficient in muscular power.

Case of Sudden Death nineteen Days after Uterine Hæmorrhage.

Charlotte Thorne, aged 29, married three years, of a leucophlegmatic temperament, had, during the period of her first pregnancy, suffered severely from gastric irritation, accompanied with cough and mucous expectoration, and at the time of the accouchement was in a most debilitated state. The child was still-born; the mother recovered slowly, and in about six months after became again pregnant, when she had a recurrence (though milder) of her former complaint. Oct. 19th, 1833, was seized with labour-pains about 2 A. M., and about 6 sent for me; who, finding the os uteri an inch in diameter, continued with her. From this time the labour went on very favourably, though slowly, until noon, when the child was born; and in twenty minutes after the placenta was thrown off; but instantly followed by a sudden and violent dart of blood, which could not be suppressed by pressure, cold applications, or the secale cornutum, though given in repeated doses; I was therefore compelled to introduce my hand, when I found the uterus perfectly flaccid, and so relaxed as to ascend before my hand into the left hypochondrium. Neither the motion of the fingers, nor yet rapping the walls, produced any tendency to contraction; I was therefore obliged to make forcible pressure on the uterus externally with my left hand, while the right continued in the cavity of the viscus. In this manner a perfect and permanent contraction was at length effected, but not before my patient had lost all power of speech and motion: her pulse was scarcely perceptible. All the usual means of resuscitation were now had recourse to (transfusion excepted), and with success, as she continued to improve gradually until 9 P. M., when I directed for her—

Pulv. Opii, gr. iij.; Acet. Plumbi, gr. vj.; Confect. Aromat. q. s. ut ft. Pilulæ tres, i. q.q. horâ tertiâ.

20th, 8 A. M.—Found she had passed a tolerable night; had taken some warm jelly with wine: pulse 90, and steady though small and soft.

21st.—Has passed a very tolerable night; eyes bright, countenance cheerful; is anxious to put her baby, which is a strong one, to the breast (as the milk has begun to appear), which I have permitted.

After this I saw her each day until the 1st of November; when, finding her steadily progressing, and nursing her child, I ceased my visits, and heard no more of her until the 7th, when I was called at 4 A. M. to see her, but on reaching her residence, learned she had expired at the moment in which the messenger had left the house. On inquiring the particulars of her case from the time I had seen her last, I was informed she had gone on nursing, and, as her friends thought, improving, up to the morning of the 6th; that on that day she had left her room, and having been visited by several of her friends, had exerted herself more than usual; but had complained once or twice in the course of the day of a sensation of internal sinking, which became more frequent towards night, though she did not allow it to prevent her sitting up until 10 o'clock, and nursing her child; that at 3 A. M. she roused her nurse in consequence of this sensation; who, having procured her some nourishment, and placed her, at her own request, in her chair, she leaned her head back and expired.

Nov. 9th, 2 P. M., I proceeded to inspect the body, in company with Mr. Haviland. The body, round and well formed, was sufficiently covered with fat. On opening the chest, the lungs, which filled about two thirds of the pleura, crepitated freely throughout. When reflected forward, they shewed no trace of that dark redness which is generally observed at the back and root of the lungs, and is consequent on the gravitation of the blood; but in each pleura was found about ℥iij. of a dark-coloured serum: the pericardium contained about ℥ss. of the same kind of fluid. The heart was softened, flaccid, and contained neither coagulum or fibrin; nor was there any appearance of venous blood when

* See article "Indigestion;" Cyclopædia of Practical Medicine, vol. ii. p. 636.

the cavæ were cut through. All the vessels were healthy, as were the viscera of the abdomen, with the following exceptions:—The mucous membrane of the stomach, which contained about a teacupful of mixed food, presented small scattered patches, of a pink colour; and the membrane generally was so soft as to yield to a slight pressure of the nail. The liver was small, hard, and of a nutmeg colour. The uterus, which was of the size and shape of a large-sized pear, was soft to the touch, and on pressure gave exit to about a teaspoonful of a brown, thinnish fluid, somewhat resembling the menstrual discharge when recent. The friends would not permit the head to be opened; and as there was a total absence of every symptom which might be considered indicative of cerebral affection, I did not deem it necessary to press for permission to examine it, when contrary to their wishes.

In conclusion I shall beg to observe, that if this be a case of death from anæmia, as I certainly consider it, we must look to three different circumstances, or rather to an untoward combination of them, as the cause: first, the severe hæmorrhage which followed the accouchement, and which reduced her to a very low ebb; secondly, the diseased state of the digestive organs, which prevented the restorative process going forward with sufficient rapidity; and lastly, the constant drain which lactation, in her ensanguined state, must necessarily have produced upon the constitution; which, though equal to resist this depressing combination as long as the patient was kept in a state of repose, was forced to yield to the first powerful call for either mental or bodily exertion, or both, as unfortunately occurred in the present instance.

Should this view of the case be considered correct, it will necessarily oblige us to think seriously before we finally determine to adopt, as a rule of practice, the advice recently given us in such cases, even though emanating from one of the most respectable authorities in modern midwifery, who, having advanced the propositon, "Ought patients who have experienced considerable hæmorrhage in labour, to be allowed to nurse?" replies to it in the affirmative*. Highly as I esteem that gentleman, as an authority in all practical points, I cannot help considering, as the result of my own experience, that though the immediate application of the child to the breast in cases of slight hæmorrhage, or rather where there may be an hæmorrhagic tendency, in the manner recommended by Dr. E. Rigby, is frequently productive of considerable advantage, by causing a revulsion of blood from the uterus, and at the same time exciting in that organ a tendency to contract,—a perseverance in nursing, after a severe hæmorrhage or other extremely debilitating affections, often lays the foundation of diseases worse in character than those which it is intended to obviate.

* See a paper on some peculiar forms of Relaxation of the Uterine Tissue, by Dr. Montgomery, in the Dublin Journal of Medical and Chemical Science, Nov. 1835, p. 235.

Case of Galactorrhœa.

Martha Burchell, a patient of the Dispensary attached to the Lying-in Institution, aged 33, of a clear complexion, excepting a dark ring around the eyes, states, that milk began to be secreted about a month previous to her first accouchement, and in her other subsequent pregnancies became perceptible at the period of quickening. Immediately after she had quickened of her eighth child, the left mamma became hard and painful, and finally suppurated in several different places, all which continued to discharge until three months after her accouchement; the right breast secreted milk freely until the last month of her pregnancy, when it likewise inflamed and suppurated. At the expiration of the third month after her confinement, the child being weaned, and the milk ceasing to be secreted, the abscesses healed spontaneously. The Christmas following she again became pregnant, and quickened in April, when, milk being secreted in the left breast, she was immediately attacked with pain in the right one, which subsequently inflamed and suppurated, and has been discharging pus during the last week.

July 1st, 1832.—Pulse 90, soft; night sleepless; tongue pale, but clear; lips furred in the mornings, with coppery taste; bowels rather costive, flatus; urine high coloured; subject, both day and night, to cold clammy perspirations, accompanied by faintness; hands and feet constantly cold. Around the nipple of right mamma there is a large nodulated, but not hard, mass, with a halo of

inflammation immediately encircling the nipple. This lumpy portion extends deeply backwards, and thence towards the axilla. None of the glands in this neighbourhood are involved, nor does the breast feel heavy in proportion. Below, and external to the nipple, is an opening, through which an abscess is discharging freely. Left breast soft and flaccid, except in a single point, where it is both hard and painful.

Having learned that my friend, Dr. Todd, had on the former occasion been consulted by the gentlemen who then had charge of her, I called on him, and at his suggestion adopted the following mode of practice:—

℞ Extracti Conii, gr. x.; Pil. Hyd. gr. xvi.; Pil. Galb. c. ℈ij.; Ft. Pilulæ, xvi. Unam nocte maneque sumat.

July 4th.—Abscess in right breast still discharging, but all around soft. An abscess has formed in left breast, where the hardness had previously existed; this she will not allow to be opened.

A linseed poultice to be applied to this, but one of recent hemlock to that which is discharging, and let the sides of the breast be smeared with the following ointment:—

℞ Ext. Conii. recentis, Ung. Hyd. Axungiæ, aa ℥j. ft. Ung. bis die utendum.

One of the pills of Hyd. c. Conio to be taken three times a-day.

12th.—Abscess in left breast has ulcerated.

Apply the Hemlock Poultice to it also; increase the Hemlock in the pills to gr. xx. ter die sumat; omit the Ointment, which has caused some irritation, and let the sides of the breasts be gently rubbed with Rum and Oil.

20th.—The mercury having slightly affected the mouth, it is be omitted, but the cicuta and pil. galb. to be continued as before. The abscess in right breast is healed, but another is forming immediately in its neighbourhood.

23d.— Some serous discharge from both nipples; a good deal of fever.

Apply the breast-pump, to try if the breasts can be relieved.

Pulv. Jalap c. ℨiss. in p. iii., i. q. q. hora, 4ta.; Mist. Salinæ Coch. ij.; Comp. q. q. hora 2da.
Repr. Frictiones.

24th.—Fever still high; no milk to be obtained by pump or fomentation, though there is still some serous discharge.

Repr. Pulv. Jalap. c.; Omit. Pil. pro. temp.; Omit. Breast-pump; continue the Hemlock Poultice, except over the incipient Abscess, over which apply a Linseed Poultice.

28th.— Abscess ulcerated and discharging.

Apply Hemlock Poultices to the wall of both breasts; renew Pills as on 20th.

August 5th.— Ulcer healing.

Pergat omnibus.

13th.— General health rapidly improving; breasts becoming flaccid; ulcer healed.

Pergat.

20th.—Still improving in every respect.

Repr. Med. Omnia.

28th.—Health, both general and local, so much improved, as to admit of my laying aside the poultices and one pill per day. A slight serous exudation from nipples.

Sept. 4th.—Says she has not enjoyed such good health, when so far advanced, for some years. Breasts quite healed; no apparent tendency to absorption; a drop of fluid still exuding from each nipple.

Repr. Pil. ut die postero.

11th.—Flying pains about the uterus; in every other respect extremely well.

Oct. 2d.—Was this morning delivered of a fine healthy girl, who, in two hours after, was applied to the breast, and obtained milk.

From the above date she went on, without a moment's check, until her recovery was complete; but at the end of the second month she was obliged to wean the child, from want of milk.

REPORT OF FRACTURES

TREATED IN THE LONDON HOSPITAL DURING NOVEMBER.

To the Editor of the Medical Gazette.

Sir,

I enclose you the monthly report of fractures admitted into the London Hospital during November last. They amount in number to 42, and are in the following proportions:—

Cranium	1
Ribs	10
Clavicle	3
Humerus	3
Forearm	1
Radius	3
Femur	7
Patella	1
Tibia and Fibula	8
Tibia alone	3
Fibula	2
	42

I regret that I cannot furnish you with a strict account of the side of the body on which the greater number of the accidents have occurred.

I have the honour to be, sir,
Your obedient servant,
JOHN ADAMS.

New Broad-Street,
Dec. 1835.

DEFENCE OF

DR. ELLIOTSON'S EMPLOYMENT OF IODINE,

AND OTHER REMEDIES.

To the Editor of the Medical Gazette.

SIR,

THAT love of justice and strict impartiality which ought invariably to characterize alike the editor and the critic, will, I doubt not, insure for the following observations an insertion in the forthcoming number of your valuable periodical; more especially as they refer to a communication from Mr. Forbes Winslow, which was published in the Gazette of Saturday last.*

In the communication above referred to, Mr. Winslow has adduced the opinions entertained by Dr. Elliotson, Dr. Addison, and himself, respecting the efficacy of iodine as a therapeutical agent. With his remarks on this subject I can of course have nothing to do, inasmuch as most practitioners deduce their conclusions from facts which they have themselves observed, or from those which have occurred under the cognizance of individuals of acknowledged credit and respectability, and have by them been made known to the profession generally.

Not so, however, his insinuations respecting the practice of Drs. Elliotson and Addison: they may and do legitimately fall within the pale of inquiry, he having in a strange, and, I must say, unwarranted, manner, made assertions concerning the professional character of these celebrated physicians which, if correct, would tend to hurl them from that station to which, as well by their literary and scientific, as by their professional acquirement, they have been so justly elevated.

I have witnessed, for a considerable period, the practice of Dr. Elliotson at the North London Hospital; and not having been, I trust, altogether inattentive, I may perhaps be allowed to state that your correspondent's assertion respecting his *indiscriminate* use of iodine is totally unfounded, and that Mr. Winslow appears to have deduced his conclusions from incorrect and consequently imperfect premises. Not that I wish it to be understood from this that Dr. Elliotson seldom employs iodine; on the contrary, it is a remedy which he very frequently prescribes, and this, indeed, with the most beneficial results, as is well illustrated by a reference to the clinical reports, from which it will become apparent that eight-tenths of those patients who are treated with this remedy are discharged cured or relieved, and this, indeed, without the exhibition of any other therapeutical agent.

This fact alone speaks volumes, and is, I imagine, sufficient to overthrow all the preconceived ideas of your correspondent.

Lastly, with regard to its empirical administration, which, if not expressed, is at least evidently implied in Mr. Winslow's communication. As a refutation of this I need only refer your readers to the Doctor's clinical lectures, from a careful perusal of which I think it must appear evident, even to the most sceptical, that in all cases where administered, its use has been not only clearly indicated, but also that its exhibition has been directed by scientific and general principles.

That the charge now adduced by Mr. Winslow against Dr. Elliotson, with regard to iodine, has been heretofore urged against him on the subject of other remedies, I am ready to allow; but do not all these circumstances tend to prove, by the universal assent now given to the opinions which he has invariably maintained, that his ideas on

* We should have given immediate insertion to the present letter, had Mr. Munk sent us his name in the first instance.—E. G.

these points were founded on correct bases? What effect do those sarcasms now produce with which he was so continually assailed on first introducing quinine* to the notice of the British public, and proving to them by a series of well conducted and well regulated experiments, that it was a remedy possessing herculean powers over certain conditions of the system; but that to obtain its specific effect in peculiar states of the economy, it was requisite to administer it in doses which set at defiance all the preconceived opinions of its discoverer.

Are not the facts obtained by him respecting the efficacy of iron† in chorea—of hydrocyanic acid‡ in gastrodynia — of sulphate of copper§ in dysentery—of hydriodate of potassa‖ as a diuretic—of creosote¶ in vomiting, *not* arising from inflammation—highly useful? And have not the conclusions which he has deduced from the facts he has observed been treasured up by his contemporaries, and made use of by them in the alleviation and cure of disease? Do the invectives of his brethren at all invalidate the conclusions at which he has arrived? Do they prove that he has been empirical or indiscriminate in their administration? Certainly not.

In conclusion, I may inquire of Mr. Winslow, in what manner the application of new remedies to particular conditions of the system are to be discovered, or their actions understood, unless it be by the collection of facts; and how are these facts to be obtained but by the administration of the remedy?

I am, sir,
Your obedient servant,
W. Munk.

London University,
Dec. 15, 1835.

* Med.-Chir. Trans. vol. xii. p. 543.
† Ibid. vol. xiii. p. 232; and vol. xv. p. 161.
‡ Cases illustrative of the Efficacy of Hydrocyanic Acid, &c. &c. 8vo. 1820.
§ Med.-Chir. Trans. vol. xiii. p. 451.
‖ Clinical lectures, in various periodicals.
¶ Ibid.

DOES IODINE CAUSE SALIVATION?

To the Editor of the Medical Gazette.

Sir,

Allow me to ask, through the medium of the Gazette, your correspondent, Dr. Ely, of Chatham, what length of time elapsed since his patient had taken mercury and the iodine was prescribed?—as facts, I agree with the Doctor, are valuable in practical medicine. But, sir, I do not admit it to be a fact, although the salivation occur during the administration of the iodine, that it is produced by the latter, but quite the contrary, or surely the cases would be more numerous on record.

I look upon iodine salivation as untenable as sarsaparilla salivation, which some few years back was occasionally mooted, but very rarely now. And how is this, I would ask? For the plain reason, that when any of those cases were duly investigated, the salivation was found to arise either from a prior use of mercury, or the preparation of sarsaparilla containing it in solution. There is another point that I am sceptical about in salivation. We have well-written cases purporting to prove the wonderful effects of a single dose of mercury. Probably the dose has not exceeded one-eighth of a grain of calomel, or at the most iij. grs. This susceptible state of the constitution to the peculiar action of mercury, I have not as yet met with, although I have examined two cases where it was supposed to have occurred from a single dose, when, in truth, that dose only proved as an excitant to excite that action of the salivary glands which had not become previously developed; and it is this same stimulating power that iodine exerts over those organs to cause the same phenomenon—salivation. But of what avail would the stimulus be unless the system contained mercury? I am induced to ask the above question, hoping it may furnish data as to the time which may elapse while the action of the mercury, as it were, may remain dormant in the system, and then shew itself.

I am, sir,
Your obedient servant,
Jas. H. Horne.

Gerrard-Street, Dec. 29, 1835.

SEQUEL OF THE BRENCHLEY CASE OF FORCIBLE LITHOTOMY.

[It will probably be recollected, that some months ago (Med. Gaz. vol. xv. p. 920,) we had occasion to notice the forcible attempt made by a surgeon and clergyman at Brenchley, to perform the operation of lithotomy on a pauper boy;

and for which the parties were tried at the Maidstone Spring Assizes. The unfortunate patient does not seem to have survived the violence long: though we by no means intend to attribute his death to what he suffered under the surgeon's hands—having no ground on which to rest such an opinion. For the following clear and strictly authentic account of the morbid appearances about the urinary organs, we are indebted to an eminent anatomist, who was intrusted with the examination of the body.—Ed. Gaz.]

Appearances observed in the post-mortem examination of James Roberts, æt. 16, who died in the Poor-house of Brenchley, on Friday, July 17, 1835.

The body was thin, but not emaciated; it presented none of the ordinary signs of puberty. On the right side of the raphe, about the centre of the perineum, was observed a dark spot where the integuments were smooth and thin, which indicated the situation of the incision made in the endeavour to extract the calculus. By pressing upwards towards the bladder on this spot, a hard substance (which afterwards proved to be the calculus,) was distinctly felt. The kidneys, ureters, bladder, and urethra, together with the rectum, and the integuments and subjacent parts of the perineum, were removed.

The kidneys were large and flabby; their cavities were dilated, and their lining mucous membrane was thickened, vascular, and in places encrusted with lymph. In the left kidney were found some deposits of pus in the cortical portion, and a quantity of muco-purulent fluid in the pelvis. The ureters were much dilated. The bladder was larger and thicker than natural, and its mucous membrane was dark-coloured and thickened, particularly about the neck, where it was partially ulcerated. The neck of the bladder and the prostatic portion of the urethra (particularly the latter) were considerably dilated, as well as its membranous and bulbous parts. They formed together a large sac, or rather canal, in which was lodged a calculus, extending from the orifice of the bladder to the bulbous part of the urethra. The mucous membrane of the neck of the bladder, and of the prostatic portion of the urethra, was ulcerated extensively. The prostate was flattened by the dilatation of its urethral canal, and it was difficult to distinguish the exact boundaries of the prostatic and the membranous parts of this tube. In the lower part of the bulbous portion of the urethra (which lodged one extremity of the stone) was found a small pouch which extended to the thin part of the integuments of the perineum, which had been the seat of the incision. In front of the bulb the urethra was healthy, and of its natural size.

The calculus measured about three inches in length, and three-fourths of an inch in its greatest diameter; it was apparently composed of the triple phosphate of ammonia and magnesia.

One extremity of the stone projected into the bladder; below this part it had a constriction which was embraced by the neck of the bladder. The body, or large central portion of the stone, rested in the dilated prostatic and membranous parts of the urethra. It had another constriction corresponding to the anterior extremity of the membranous part of the tube, and beyond this, a second head, which was lodged in the dilated bulb, and which was that part of the stone which could be felt with the finger from the perineum.

The calculus appeared to be so lodged and embraced by the surrounding parts as to be immoveable; so that the urine must have dribbled away by its sides. The incision in the perineum must have laid open the bulbous part of the urethra.

The kidneys, bladder, and calculus, are preserved in the Museum of King's College, London.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'Alembert.

Nouvelles Recherches sur le Rheumatisme articulaire aigu en général, et spécialement sur la loi de coïncidence de la péricardite et de l'endocardite avec cette maladie, ainsi que sur l'efficacité de la formule des émissions sanguines coup sur coup dans son traitement. Par J. Bouillaud, Professeur de Clinique Médicale à la Faculté de Médecine de Paris.

The design of M. Bouillaud in this work is to prove the pathological coincidence between pericarditis and rheumatism, and the practical efficacy of blood-

letting, "*coup sur coup*," in its treatment.

In order to establish the former proposition, he in the first place examines the opinions of various writers, particularly among his countrymen, in order to shew that they have only mentioned the subject incidentally, which, indeed, would seem to have been the case with Corvisart, Laennec, Chomel, Andral, and Louis. But when the author extends his observations to English writers, we find no other authority quoted except that of Dr. Hope. The inference to which we are naturally led, both by the doctrine of M. Bouillaud and the manner in which he endeavours to establish it, is, that he is himself very ignorant of the state of medical practice in this country. There is probably no point in modern practice more firmly established than the connexion between acute rheumatism and inflammation of the investing membranes of the heart; and that Dr. Hope did not direct more especial attention to the circumstance, is to be attributed to his well-grounded conviction that to have done so would have been to insist upon a fact already perfectly well known. He therefore contented himself with stating in general but express terms, that in acute rheumatism the most frequent, as well as the most pressing danger, resulted from the supervention of carditis or pericarditis.

The only novelty, therefore, in the pathology of M. Bouillaud, appears to us to consist in a question of degree. All who have seen much of acute rheumatism will readily acknowledge that the coincidence of it with pericarditis is very frequent; but M. Bouillaud puts it interrogatively, whether in the great majority of cases of acute general articular rheumatism, with fever, there does not exist rheumatic inflammation of the sero-fibrous tissue of the heart. This coincidence, therefore, with him is the rule, and its absence the exception.

He admits, however, that the rheumatic inflammation about the heart may exist in many different degrees of intensity, and shields himself against the charge of having represented it as more common than it really is, by implying that its presence often escapes the investigations of the unskilful. The symptoms which indicate inflammation of the pericardium as a concomitant of rheumatism are thus described by M. Bouillaud:—

"The existence of pericarditis in a patient affected with acute articular rheumatism becomes certain when we observe the following symptoms:—Dulness of the precordial region; much more extended than in the natural state, being doubled or tripled in every direction; projection of the same region; beating of the heart distinct; little or not at all sensible to the touch; sounds of the organ remote, obscure, accompanied by various anormal *bruits*, some depending upon friction of opposite surfaces of the pericardium against each other, and others proceeding sometimes from the complication of the pericarditis with inflammation of the valves of the heart. Pain more or less acute in the precordial region, palpitations, irregularity, inequality, or intermission, of the pulse, are sometimes added to the preceding symptoms.

"The coincidence of endocarditis with acute articular rheumatism I hold to be certain when we have the following symptoms:—The bellows, rasping, or sawing *bruit*, in the region of the heart, which further yields a dull sound over a space considerably larger than natural, and presents also a certain degree of projection, though less than we witness in pericarditis with effusion. The action of the heart raises the cardial region remarkably, and is generally irregular, unequal, and intermittent, sometimes with a vibration or tremor, the pulse being hard, strong, and unequal, corresponding to the beating of the heart."

The above description is followed by the detail of a considerable number of examples, not fewer than twenty cases of rheumatism having been met with by our author between the month of August, 1835, and the beginning of October. These consist of three series: the first consisting of general acute articular rheumatism, accompanied by fever more or less severe; the third set consists of those cases in which the rheumatism was slight, and without fever. Between these two is another category, containing those cases in which a lesion of the heart, supposed to be organic, was met with in persons formerly affected with rheumatism of an acute character and frequent recurrence. The list of these is intimately connected with the two

others, and the organic lesions it displayed are held to have been endocarditis, or pericarditis, in a chronic form; that is to say, with accidental productions which had resulted from the previous inflammation. And assuming this reasoning to be correct, it gives us a rheumatic affection of the heart or its membranes, in eight cases out of nine of acute articular rheumatism.

After the relation of twenty cases of rheumatism in support of the above opinion, M. Bouillaud proceeds to consider the symptoms, progress, intensity, duration, and termination of the disease. Of these subjects the only one in which we find sufficient novelty to arrest us, relates to the *mobility* of the disease. He holds that acute articular rheumatism is not capable, according to common belief, of rapidly changing its seat, or rather of being very suddenly dissipated. If, for example, there be considerable swelling of the knee, with effusion into the joint, it is true that the pain of the joint may suddenly cease, with or without occurring in another part; but it is not always the same with the effusion, which he holds to be the essence of the disease, the mere pain being only a kind of neuralgic symptom. Rheumatism of the joints, which does not exceed a certain degree of severity, may shew a great mobility; but such is not the case with rheumatism of a more intense degree, which tends to suppuration, or which has already terminated in a sero-purulent effusion. This, if left to itself, is not dissipated until after the lapse of considerable time; but the pain which accompanied it may disappear long before the articular effusion, just as a pleuritic stitch in the side may cease before the reabsorption of any effusion caused by the inflammation.

A remarkable fact, according to our author, which has itself been observed, although its cause has escaped detection, is the persistence of fever in some rheumatic patients, although the joints have long been free from the disease. The alleged cause, to which M. Bouillaud attributes great importance, is the existence of rheumatic inflammation of the heart, vessels, pleura, &c., which hold on their course in a manner completely indolent,—a circumstance which has led to their being so generally overlooked. This is also regarded as an additional proof that pain is not essential to rheumatism, and that this effusion is not so mobile as has been supposed, since in the cases alluded to it is so rooted as to keep up a state of fever for many successive weeks.

In speaking of the morbid anatomy of rheumatism, M. Bouillaud remarks, that suppuration is of more frequent occurrence than has been supposed. He quotes the authority of Stoll in support of this opinion, and informs us that he has himself seen three cases in which rheumatism terminated in suppuration of the joints. Two of these are detailed; the first is as follows:—

"April 11th, 1828. The body of a young woman was opened at La Charité, she having been affected with acute articular rheumatism, in the course of which a pleurisy supervened; the patient was only fifteen days in the hospital. The left knee-joint internally was red, somewhat dry; condyles of the femur eroded; no pus. The right knee-joint was filled with genuine pus, mixed with synovia. There was but little sanguineous congestion (during the last days of her life the patient had ceased to have pain in this joint). One of the wrists was red, and evidently inflamed. The portion of the crural vein nearest the joint which contained pus, was filled with purulent matter, mixed with reddish sanies; in all the rest of its extent it was obliterated by a coagulum, as were its subdivisions; at various points a certain quantity of pus was observed. The parietes of the vein were thickened, particularly towards the knee; the artery was healthy; the nerve which accompanied the vessels was redder than natural, and sensibly enlarged."

"Of five cases of articular rheumatism, mentioned in a summary of my clinique, from March 10th to August 30th, 1832, one proved fatal. The following is a short note of it:—

"The individual was attacked, during his stay in the hospital, with a violent erysipelas of the left hand and forearm, accompanied by violent fever and cerebral disturbance. He died the fourth day from the accession of this disease. On post-mortem examination, several large collections of matter were found in the integuments of the arm and hand; and the veins of this limb, as well as several others, were inflamed. Most of

the joints contained thick, turbid, synovia, having some specks resembling pus."

Other cases are quoted from various authors; and we presume no reasonable doubt can be entertained of rheumatism occasionally terminating in suppuration. But at the same time we must remark, that the only cases adduced by M. Bouillaud as having been witnessed by himself, are of a very unsatisfactory nature, both, and more especially the latter, being examples of those purulent deposits connected with inflammation of veins, and well described by Mr. Arnott (Medico-Chir. Trans. vol. xv.)

We now come to the treatment, and this, although the most important point of all, may be discussed in few words.

The rule of M. Bouillaud is to bleed, "*coup-sur-coup;*" and the meaning of this expression will be best understood by giving the author's formula, if we may so call it, as applied to an acute case of rheumatism in a patient of medium age, constitution, and strength.

First day.—The patient, supposed to be visited in the evening, is bled to the extent of four pallettes.

Second day.—A double bleeding of from three and a half to four pallettes, and in the interval local bleeding by leeches or cupping, the latter being preferred. By this local depletion, it is calculated that three, four, or even five pallettes are obtained.

Third day.—One general bleeding, and one local, similar to the above.

Fourth day.—The fever, pain, swelling, and other inflammatory symptoms, have sometimes disappeared. If so, no more blood is drawn; but if not, three or four pallettes are again taken from the arm.

Fifth day.—Resolution is by this time generally accomplished; but if otherwise, three pallettes are to be taken by venesection, and the same to be repeated at any time that a relapse may occur.

The average quantity of blood required to cure acute rheumatism is from four to five pounds, but in some instances amounts to seven or eight.

The auxiliary remedies mentioned by M. Bouillaud are not very important, with the exception of opium. They consist of the antiphlogistic regimen, blisters, compression, bandages spread with mercurial ointment, and lastly, cataplasms. No mention is made either of calomel or purgatives,—an omission which, in our opinion, is of vital importance. The great difference, in other respects, between the treatment recommended by M. Bouillaud and that usually adopted in this country, relates to the quantity of blood abstracted, and to the persevering manner in which the venesection is repeated. Yet does not the practice of our author differ so much as might be supposed from that formerly recommended by, probably, the greatest of English physicians; — we mean Sydenham: and we shall close the present article by quoting his own words upon the subject:—

"Ut primum accersor, statim sanguinis ℥x. è brachio LATERIS AFFECTI mitti jubeo . . . . . . Die sequenti, sanguinis tantumdem detrahi præcipio; atque intercalato die uno alterove, pro ægri viribus, tertiò; dein, interjecto trium quatuorve dierum intervallo (prout ægri vires, ætas, constitutio, aliæque circumstantiæ suadent monentque), quartò, atque ultimo ut plurimum, venæ-sectionem repeto; raró enim usu venit, ut ultrà quartam vicem venam incidamus, nisi vel regimen justò calidius præcesserit, vel medicamina calidiora ægro præter necessitatem fuerint ingesta."

An Introduction to the Study of Practical Medicine; being an Outline of the leading Facts and Principles of the Science, as taught in a Course of Lectures delivered in the Marischal College of Aberdeen. By JOHN MACROBIN, M.D., Member of the Royal College of Surgeons, Edinburgh, &c. &c.

THE first part only of this work has yet been published; it relates to the pathology and treatment of congestions, inflammations, hæmorrhages, dropsies, and fevers. We never analyse treatises which are merely elementary, but content ourselves with perusing them, and offering our opinion for the guidance of such of our readers as may be disposed to think it entitled to such confidence. On this principle we hesitate not to recommend Dr. Macrobin's book to our junior friends as one of the best introductions to the study of medicine which has fallen under our notice; it is but an "introduction," however, and can only be looked upon as containing an outline of the diseases of which it treats. Still that outline is correct, and conveys a favourable impression of the course of lectures from which it is taken.

The present is, we believe, the first work which has emanated from the Medical School at Aberdeen, and we are gratified to perceive the activity of which it constitutes an example. In many respects Aberdeen is well calculated to succeed in this respect. It has an excellent hospital, a first-rate professional library, contained in a handsome building belonging to the "Medical Society," efficient teachers in the various branches of the science, and pupils who, for the most part, have received a good preliminary education, many of them having taken their degree in arts at one of the two Colleges which the town contains.

We shall be glad to find that Dr. Macrobin extends his work to the other subjects of practical medicine.

Illustrations of the Comparative Anatomy of the Nervous System By Joseph Swan. Part I.

This first portion—of what we have no doubt will prove to be a splendid work—contains seven plates, quarto size, admirably drawn and engraved, and accompanied by explanatory letter-press. The nervous system of the crab, the lobster, the centipede, the leech, the slug, the earth-worm, &c. is displayed with a degree of clearness which we had hardly hoped to have seen attained; and the views of the sympathetic and spinal nerves of the cod are pre-eminently beautiful. We are satisfied that the publication of this work must contribute to raise the reputation of the author, high as it was already, and to place him among the first anatomists of this or any other country.

MEDICAL GAZETTE.

Saturday, January 2, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

EVILS OF THE PRESENT POOR-LAW, AND THEIR REMEDY.

The universal and continued outcry against the working of the amended Poor Law, proves pretty clearly that this vile and heartless specimen of legislation has not a redeeming quality about it. Time, which is said to bring a remedy for every evil, has as yet done nothing towards mitigating this: on it goes doggedly, in the manner of its outset; and every day produces some fresh examples of the malignant principle with which it is animated.

We form our judgment of the measure precisely as the Poor-Law Commissioners pretend that they are willing it should be judged. In their Annual Report, published in August last, they state, that "the change in the system has not, in many instances, been so long in operation as to develope the whole of the effects which may be anticipated from it." The four or five months which have since elapsed have served amply in the way of illustration; and we think we may now as safely form our opinion of the ultimate tendency of the system as if it had been in operation for years. All that we can anticipate from it is pure and unmixed evil for the future; while at the present, and since the introduction of the new order of things, we can see nothing in the conduct of the Commissioners but inhumanity towards the poor, and an insolent demeanour towards the profession.

That part of their report (just referred to) which relates to the medical arrangements in the several districts, is, we have no hesitation in saying, a tissue of the most paltry meanness and falsehood. Of this latter character is the assertion that they have ascertained instances in which medical men, attending the settled poor of a parish for a small fixed sum, had contrived to get upwards of three hundred pounds a year by treating paupers of other parishes under suspended orders of removal; and that this system was the result of a sort of collusion between the parochial surgeons and overseers! This gross charge is made without an atom of proof in the shape of evidence; it has been again and again contra-

dicted by trustworthy and indignant parties: the Commissioners have been called upon, directly, or indirectly (through the anonymous officials who sometimes venture to echo them in the public prints), to adduce specifically, and to point out those instances individually, upon which they ground a statement so derogatory to a large and respectable class of the community. But no; this would be too great a mark of condescension, or, more probably, it would lead to unpleasant explanation; and so it has been permitted by the worthy functionaries and their retainers to pass *sub silentio.*

There are two points connected with the new Poor Law measure which cannot be too frequently adverted to and dwelt upon. The first is, that the tenure of office by the Commissioners subsists on the condition that savings shall be effected at all risks, and that the starving and sick poor shall be tested to the uttermost, in order to find out upon *how little* relief they may contrive to keep soul and body together. The second point to be kept in view is, that the avowed notion of the authors of the *amended* Poor Law is, that by adopting a rigorous and severe system in dealing with the poor, pauperism may be eventually seared away, or abolished totally: by affording no indulgence, and as little countenance as possible to the sick and hungry, they hope—and with very good reason—that there will be no sick or hungry at last to relieve: disgust and despair, they are confident, will aid their cause; and it is only natural to conclude that they calculate also on another worthy agent equally effectual in assisting them—death!

This odious manœuvre of goading the unfortunate to desist from soliciting parish relief, we perceive, is steadily kept in view by the Commissioners; but, as might be expected, their only mode of upholding it in principle is by a sophistry which would be beneath contempt, were it not for the dangerous consequences to which it leads. They find out, for example, that by pursuing their plan, a few persons who hitherto availed themselves of parochial medical attendance, while they could very well afford to pay for it, have now abandoned their claims. One of the new contract-surgeons is brought forward to vouch for this notable fact: he says, in his evidence, that he has met with many cases of the kind, and cites *one* instance. Thus, then, because the practice of relieving the poor, under the pressure of their wants and bodily infirmities, has been found liable to abuse, it must be abolished altogether. Because impostors sometimes succeed in extorting the charity of the benevolent, the really necessitous shall no longer have assistance; because there are malingerers abroad, and persons who feign diseases, therefore there shall be no medical relief. Such is the sort of argument, if it be not the identical one, with which our sapient and humane New Poor-law functionaries amuse the public, and establish their right to the enjoyment of their ample salaries.

But the public must be very obtuse indeed, if it cannot see and appreciate what is daily passing before its eyes. Take the recent affair at Thatcham as an example. The new system was exhibited there in its true colours, and with all its native inhumanity thick upon it. The life of a wretched man was sacrificed for want of that medical assistance which ought to have been supplied by the Guardians (as they are miscalled) of the Poor. The circumstances must be fresh in the recollection of the reader; the newspapers contained the details, shocking as they were, elicited at the coroner's inquest. A man, in a dangerous and (as it proved)

fatal state, was removed in a cart a distance of some miles, and seen *but once* by the contract-surgeon (and that after grievous delay) during the last two or three days preceding his death. All this, as well as the general mischief attendant on the system, was abundantly exposed at the inquest, and stands confirmed by the verdict of a respectable jury.

Nor has the sequel of this lamentable affair been less disgraceful to the same parties. The coroner and foreman of the jury waited on the Board of Guardians to make remonstrance on the evils complained of, and to suggest remedy and redress. But the result of the mission was as might have been anticipated: there was no redress nor relaxation of the usual haughtiness. The all-sufficient functionaries hesitated not to act as advocates and judges in their own cause. The jury, on their oaths, found that there was a culpable negligence and insufficiency in respect to the medical attendance yielded to the deceased: the Board, at their leisure, announced that *they* found no such thing—the medical attendance was what it ought to be—and that if the man died, it was no fault of theirs.

There is but one way of dealing, that we can see, with these insolent personages. They are not amenable, indeed, to the power of a coroner; nor do they seem in the slightest degree affected by the influence of public opinion. One tribunal, however, is still open; and we are greatly deceived if the evidence already accumulated have not its due weight in the quarter to which we allude. Parliament meets presently; let petitions be prepared in strong and respectful language, praying the interference of both Houses to prevent the abomination of further proceedings under the New Poor Law. Let it be urged that that law—or rather the *amendments* (so called) introduced into it—and the practical results of which are found to be so iniquitous, shall be repealed, and a more suitable and salubrious system adopted. Then, and not till then, can we expect any change for the better: but *until* such a step have been duly taken, and have failed, we can see no reasonable ground for despair.

PROCEEDINGS OF THE PANTON-SQUARE PHILOSOPHERS.

SKULLS FROM THE MAURITIUS.

Dr. Stewart, a medical officer at the Mauritius, lately sent to Sir James Macgrigor four skulls, requesting that they might be handed over to the Phrenological Society here, for the purpose of obtaining the opinion of that learned body as to the characters of the individuals from whom they had been taken. With them was transmitted a sealed packet, which was to be retained at the Army Medical Board in Berkeley-street, until a written statement of the result of the phrenological examination was forwarded from Panton Square.

The Society, as a body, rather knowingly, declined the proposal; but Dr. Elliotson undertook the task, and forwarded his opinion to Sir James Macgrigor, as required. The sealed packet was then opened in due form, and with the following result. We ought to remark that the skulls and the characters of the individuals were labelled with corresponding numbers. Of Nos. 1 and 3 nothing is known, and they were probably added only as a further test for the phrenologists; but the skulls of No. 2 and 4 belonged to remarkable personages.

Of No. 2, Dr. Elliotson's estimation runs thus:—"Might have been undistinguished in talent, though possibly quick of observation; but proud, quarrelsome, selfish, and sly."

The written packet informs us that "this man, a negro slave, was tried on the charge of having murdered one of his comrades, and tried and convicted of a barbarous attempt to murder another, a female, upon the very slight provocation of accusing him of having stolen a pair of scissors." He was also "sly and cunning," and so far Dr. Elliotson's guess is pretty good, though the degree of ferocity displayed by a double murder is greater than the mere term "quarrelsome" would imply. But three

of the most conspicuous points in the character—namely, jealousy, lust, and intrepidity—are not even hinted at. "He was extraordinarily jealous, (says the packet) and while under the influence of jealousy, quickly rendered ferociously irritable, most abusive in language, and violent in acts. The attack on his first victim was caused by slight attentions shown to his comrade, by his master, for good conduct." And again, as to his measureless lasciviousness, "he was most libidinous in temperament; two examples of the disposition are striking. He made indecent propositions to a person in a class very highly above him; and only a few hours before his execution, on being kindly asked by his spiritual attendant whether he wished to have any thing, he answered, 'Yes, a woman.'" Neither is any phrenological notice taken of his reckless boldness. "His intrepidity amounted nearly to insensibility at the place of execution; he mounted the scaffold boldly, examined the axe, talked freely to the executioner, and laid his head on the block without a sign of fear."

It is proper to add, however, that afterwards it was remarked by the Society, and assented to by Dr. Elliotson, that "mativeness" was very large in the skull; but this, as an *ex post facto* discovery, is good for nothing.

Come we now to No. 4; concerning which Dr. Elliotson's summary is, "*May have been, in every respect, a highly respectable character, though of ordinary intellect.*" We shall give the biographical sketch of this "highly respectable character," from the packet, without a single comment:—

"No. 4 is the skull of Rhugoburg-Sing, an Indian, who was convicted in India of killing one man and wounding two others, under circumstances unknown to the writer of this note. He was transported to Mauritius, and was for some years employed there as a convict. He was violently ferocious, and had a strong propensity to destroy. Some time back he was tried, for having, almost without provocation, knocked down two men with a staff, nearly killing one of them by fracturing his skull. He escaped on the trial through an informality. He was afterwards convicted and executed for a murder, supposed to be committed in revenge for an insult offered to the family of the culprit, who was of a high caste. His behaviour immediately after condemnation was brutally savage, but he afterwards became more quiet, and died courageously. He seemed irritated at his sentence, because no one saw him commit the act. He endeavoured to escape, and severely stabbed two persons who were employed to take him."

In this dilemma, Dr. Elliotson at once adopts the obvious and most satisfactory explanation, that "as phrenology is true," the numbering must be false; in short, that packet No. 4 did not belong to skull No. 4; and as the gentleman by whom they were furnished, himself "a zealous phrenologist," has been written to on the subject, we have no doubt the mistake will be rectified with the least possible loss of time.

SKULL OF SWIFT.

At the second meeting for the season, the Society was entertained by a Mr. J. I. Hawkins, with a critique on an article of ours. How proud we ought to be! But though the gentleman brought to his task an abundant quantity of phrenological zeal, we must say he displayed a marvellous lack of information and candour *. In order to prove that Dean Swift possessed Amativeness "large," Benevolence "small," Ideality "small," Wit "small," &c. &c. by way of gainsaying our assertions to the contrary, and which we derived from the most authentic sources, Mr. Hawkins raked the puddle of the libellers of Swift's character, and thus eked out the principal portion of his paper.

For example, he finds that Dr. Beddoes, in an essay in his Hygeia, throws out some conjectures on the probable cause of the Dean's vertigo, which the Doctor attributes to certain "habits of early and profligate indulgence." This Mr. Hawkins takes for "confirmation strong" of Swift's *amativeness*; though he must know that Beddoes had not an atom of proof of the real existence of any such habits as he indecently and most disingenuously imputes to the Dean. We say that Mr. Hawkins must know this; for he borrowed the quotation from the very page of Scott's life of Swift in which the passage is both cited and censured in terms of indignation by the biographer. Sir Walter, having shewn, in disproof of Beddoes'

* We refer for further particulars concerning the matter of this article, to Dr. Macleod's letter, at p. 543.

assertion, that the gross images indulged in by Swift were the very reverse of the licentious or amative, adds a remark which will apply to certain medical phrenologists, as well as to him for whom it was originally intended—"Until medical authors can account for and radically cure the diseases of their contemporary patients, they may readily be excused from assigning dishonourable causes for the disorders of the illustrious dead."

But again, Swift's *benevolence* was "small." How is this point made out for the satisfaction of the phrenologists? By having recourse, in contempt of Scott and all the best biographers, to the notoriously malignant and disgraceful article on Swift which appeared in the Edinburgh Review in the year 1816. Mr. Hawkins also quotes a hearsay story, which he picked up in a recent visit to Dublin, to the effect, that the Dean used sometimes to institute legal proceedings against persons to whom he had *benevolently* lent money: of this, says Mr. H., "I was informed in Dublin as a notorious fact." Poor simple critic! Did he really think he met any one in Dublin who could give him better information on this point than is contained in the biographies of Swift?

Will it be believed, also, that our phrenologist cites, in proof of his position, that the Dean had *ideality* "small," the well-known remark of Dryden, who, when Swift, then a mere boy, presented him with a copy of verses in the shape of an ode—"Cousin Swift, you will never be a poet?"

And again, to show that Dean Swift possessed *causality* and *comparison* only in a "moderate" degree, as the "organs" of the skull indicated, our critic quotes Scott, where he informs us that logic was a distasteful study to Swift in his College days: but Mr. Hawkins ought to have recollected that a hatred of Burgersdicius does not necessarily imply a deficiency of the reasoning faculty: even Locke himself had no great attachment to the logic of the schools.

But we have bestowed more notice on the silly and shallow performance of our critic than it deserves. In our original article on what appeared in the Phrenological Journal concerning Swift's "*developments*," we proved that either the skull which the philosophers in Dublin had got hold of was not Swift's skull, or that phrenology was an impudent lie. Mr. Hawkins has laboured heartily to affirm the former position, but he has failed to extricate his beloved phrenology from the other horn of the dilemma.

We may as well append, for the benefit of Mr. Hawkins and the phrenologists generally, the remarks which appeared in the *Times* of Wednesday last, on the same subject as the foregoing: they may serve, along with our own observations, to furnish another evening's entertainment for the frequenters of Panton-Square:—

"Dull witted and half-educated men always seek some by-way of notoriety, having just sagacity enough to discover that they have no chance of acquiring any reputation in the regular field of art and science. Among the most offensive of these quacks may be classed the phrenologists, or bumpologists, as they are more appropriately called. Every body knows what blunders these blockheads made over a supposed skull of Raphael, which turned out to be the skull of a grave-digger: and we have already noticed some impertinent trash which has been poured forth respecting the skull of Dean Swift, which was pulled from the body, and subjected to the scrutiny of these pedantic dunces. We see, in more than one quarter, that this disgusting folly is still at work: we cannot waste our time, nor that of our readers, by any detailed exposure of the absurdities which have been published, but we give one specimen as a decisive sample of the rottenness of the bulk. These worthies, then, have discovered, and are ready to prove, by the size of the "organs of wit and ideality" in Swift's skull, both "small," to adopt their jargon, that the world must be totally mistaken about the Dean of St. Patrick's; and that the author of the ***Tale of a Tub*** and *Gulliver's Travels*, had neither brilliancy nor originality! And there are gaping simpletons who swallow these crudities, and call them science."

A LAST WORD TOUCHING THE CHAIR OF SURGERY IN EDINBURGH.

A NOTICE in the Lancet of this day (January 2d) confirms all we have said in reference to the absurd puff about the Surgical Chair in Edinburgh having been refused by Mr. Liston. We conjectured that some unauthorized indi-

vidual had asked Mr. L. to become a candidate, and he, knowing how matters stood, very wisely said he would not. It now appears that a Mr. Dick was the individual. "*This* is the whole of the matter."

INSURANCE OFFICES.

FEES FOR CERTIFICATES: WHO OUGHT TO PAY FOR THEM?

To the Editor of the Medical Gazette.

Sir,

I am induced, by your leading article of last week, to address a few observations to you on the interesting subject discussed in it.

After carefully weighing all you have said, I am still not convinced that a medical man has any just claim on an Insurance Office, for certifying respecting the health of an individual whom he has attended, or with whom he is acquainted, and who refers the Office to him for a testimonial as to the insurability of his life.

The case, when divested of all unessential considerations, appears to me to stand thus:—An individual, for his own purposes, wishes to insure his life: the Office to which he applies, says to him—We will insure you on these conditions: first, that you will present yourself to our examining Physician or Surgeon, and satisfy him as to your actual freedom from obvious disease, and your healthy and unexceptionable appearance:—Secondly, you must arrange with your ordinary medical attendant, or some professional person who has a sufficient knowledge of you to enable him to give an opinion on the subject, that he shall answer such questions as we may think it necessary to put respecting your past state of health, and the general habits and predispositions of your constitution, with a knowledge of which a mere inspection by our own physician cannot supply us. If you will do this, and if we are satisfied with your appearance, and with the answers given by your medical referee, we will insure your life; if not, we decline insuring you.

Now, in this case, as it appears to me that the medical man answers the questions for the sake of his patient and employer, who, without his testimony, cannot get insured, whatever remuneration he may think due to him, should, in my humble judgment, be claimed not from the Office but from his patient, who is, in fact, virtually though not actually, the asker of the questions.

Besides, supposing the medical man refuses to answer the questions; whom, I would ask, does he chiefly disoblige? not, surely, the Insurance Office, for in such a case the Office refuses the life, and there is an end of the matter as far as they are concerned; they have one insurance less, which might or might not have turned out an advantage to them; but it remains for the medical man to account to his patient for not doing what he requests him to do; and without which, he was told by the Office, from the commencement of the affair, that his life could not be insured. He it is who by such refusal has lost the opportunity of making an advantageous purchase, or a marriage-settlement, or a provision for his family; and he must settle the matter between his medical attendant and himself.

The case of one individual insuring another resolves itself into the same elements, *mutatis mutandis*, as the above, and therefore need not be dwelt upon.

I have argued this question on the supposition that a fee ought to be offered to and accepted by a medical man who writes a certificate for his patient; it being only a doubt, when such certificate happens to relate to the insurance of his life, from what quarter the fee ought to come to him: I trust, however, I may be allowed to say, without giving offence to those who may differ from me on this head, that I was early advised by an eminent professional friend, now no more, to decline altogether taking any remuneration for certificates, and I have always acted on this principle, not because I suspect myself, or suppose that others deliberately suspect me, of being capable of receiving a bribe to testify to a falsehood, but because I would place myself, if possible, above even the insinuations of a hostile advocate, should the matter to which the certificate refers be at any time disputed in a Court of Justice; for I think it cannot be doubted, but that the receiving of a fee for such a duty depreciates to a certian extent the document itself as a disinterested testimonial, in the eyes of the vulgar, and it is with the vulgar we generally have to do in such cases. This, however, I am aware is not exactly the

question at issue; I will not, therefore, occupy your time by saying any more respecting it.

With regard to the promises made by some of the Offices to medical men, that their answers will be considered confidential, I would join you in reprobating it in the strongest terms—We neither ask nor want their confidence. In answering correctly the questions put to us, we are merely acting the part of honest men, and the only return we can claim, as a matter of confidence, for our disinterestedness and candour, is that our opinion should not be wantonly or unnecessarily divulged; or divulged at any rate in such a manner as should disturb the mind of our patient, and place us in an uncomfortable posture with respect to him. More than this they cannot honestly promise us, unless our certificate is to be considered a dead letter.

I quite agree with you also as to the unreasonable demands for gratuitous labour that are frequently made on the profession, especially by the government or the public, who have certainly no more right to call on us to transact their business without adequate remuneration, than they would have to ask an attorney or a barrister to prepare briefs and plead against felons for the mere satisfaction of performing this useful duty. All I contend for is, that the duty of granting certificates for the insurance of lives is not quite a fair illustration of your position. I am, sir,

Your obedient servant,

MEDICUS LONDINENSIS.

Dec. 28, 1835.

CLINICAL LECTURE

ON

TIC DOULOUREUX, OR FACIAL NEURALGIA,

Delivered at St. George's Hospital, Dec. 15, 1835;

BY SIR BENJAMIN C. BRODIE.

"JOSHUA KINGETT, 48 years of age, was admitted into the hospital on the 14th Oct. 1835. On his admission he stated that for the last ten months he had been suffering the most severe pain, which was entirely confined to the left side of the face; that this pain at first had an intermittent character; but that latterly it had become constant; and at times was so acute that, to use his own language, he would have rejoiced if any one had knocked him on the head. At these times he seemed almost to lose the sight of his left eye, and very often suffered from tooth-ache. At the time of his admission the pain was chiefly confined to the cheek and nostril, which were puffy, and tender to the touch. There was no disease to be observed on looking into the nostril. The bowels were always torpid, and the tongue was covered with a whitish-brown fur. He was directed to apply the veratrine ointment, in the proportion of a scruple of the veratrine to an ounce of lard. A portion of this was to be rubbed in twice a day, and he was to take five grains of blue pill every night, with a draught containing five drachms of infusion of senna, five drachms of compound infusion of gentian, a drachm of tincture of senna, and a drachm of sulphate of magnesia every morning.

"On the 23d, having pursued this plan for about week, he thought that he was a little better. A bad tooth was discovered in the upper jaw, which was extracted. The tongue was a little cleaner He was directed to take infusion of rhubarb and columbo, of each six drachms, with a drachm of compound tincture of cardamoms, and half a scruple of carbonate of potass, three times daily. He was to go on taking the blue pill."

On the 29th the report runs thus:—"He has improved rapidly; the pain is now very tolerable; the bowels are open twice daily; the tongue is nearly clean."

On the 7th November it is said, "The pain, which had almost left him, returned with great severity two days ago. He has had no sleep since, in consequence of it. The tongue is again white and furred. The medicine was not sufficient to act on the bowels, which have been confined for the last two days. He was directed to take five grains of blue pill every night, and a dose of compound infusion of senna with sulphate of magnesia every other morning."

On the 15th it is said that "he had been again relieved as soon as the bowels were well opened."

On the 17th November I placed him on the following plan of treatment. He was to take five grains of blue pill, five grains of compound extract of colocynth, with three grains of extract of lettuce, every night. This medicine acted well on his bowels; he has been purged ever since he took it, two or three times daily. He has continued to take it up to the present time. The tongue is now quite clean. He is entirely free from any thing that deserves the name of pain, although he has

still some feeling of uneasiness in the face."

A violent pain in the face attacking the patient at intervals,—a pain so violent that the patient wishes that somebody would destroy him, and yet there being no disease perceptible in the parts to which the disease is referred: it is to a pain of this kind that we commonly apply the name of *tic douloureux*, or as some call it, with more propriety, *facial neuralgia.* We must regard this case, then, as one of tic douloureux, or, if you please, facial neuralgia.

You will observe, that besides other classifications which you may make of the pains that occur in disease, you may divide them under these two heads. There are cases in which the pain is felt where the disease exists, as there may be inflammation in the knee, and pain in the knee in consequence; carcinoma in the breast, and pain in the breast in consequence; disease in the liver, and pain therefore in the hepatic region. Then there are other cases in which the pain is referred to parts which are not actually the seat of disease. Thus, there may be pain in the knee while the real disease is in the hip; there may be pain in the shoulder while the real disease is in the liver; there may be pain in the breast, while the real disease is an hysterical state of the constitution generally.

Tic douloureux, or facial neuralgia, belongs to this last class of pains. The pain which is felt is referred to some part or other of the face, or to the whole of one side of the face, and yet there is no disease there. You are not to suppose that the cause of the pain in this complaint is always the same: the fact is, the pain is but a symptom, and it may depend upon different causes; so that in those patients who are said to be affected with tic douloureux, the real nature of the disease varies very much in different cases. You may have half a dozen persons with tic douloureux in the face, the symptoms in all of them being the same, or very nearly the same, and the real disease may be different in every one of them. The pain, as I have said, has the same character in all these cases, and it differs from the pain of most other nervous affections. You will observe that the branches of the fifth pair are all under particular anatomical circumstances; that they all proceed from that remarkable plexus which is bathed, as it were, in the blood of the cavernous sinus, and that the branches of it all run through bony structures; the second and third branches especially being enveloped in bone to a great extent; and probably it is from one or other of these anatomical circumstances, or from both of them combined, that the pain derives its peculiar character.

The pain in all these cases, whatever may be the cause of it, generally comes on gradually. At first it is a pain which, though severe, may be borne; but at last it becomes quite intolerable,—so intense that the patient always says he would rather die than bear it. At first he complains of an odd twinge every now and then in the face; and it generally begins in the cheek where the second branch of the fifth pair of nerves is distributed. The twinge becomes more severe, and recurs more frequently. At first it recurs only two or three times daily, and lasts for an instant; then the twinge becomes more severe, of longer duration, recurring several times in the twenty-four hours; and so it goes on increasing. When the disease is at its height, the patient is in as wretched a condition as you can well imagine a human creature to be in. The pain attacks him every quarter or half hour, sometimes oftener, coming suddenly and unexpectedly on him at uncertain intervals. He states that at first here is a sensation of spasm, which is followed by a violent and continued pain, accompanied in some cases with a sense of pressure acting from above. You see the patient acting with all the muscles of the trunk, as if it were necessary that he should make this effort in order to support himself under a heavy weight that was forcing him to the ground. This will last perhaps for two or three minutes, and then the pain goes off, and he is quite well again till the attack returns. The recurrence of the pain is always readily induced by the patient's attention being directed to it. If you ask him how his face is to-day, the attack comes on directly; but if you hold him in earnest conversation upon any other subject, it may stay away for half an hour. The patient often cannot get to sleep on account of the pain; but having once fallen asleep, he may continue so without the pain recurring for several hours. I have known this to happen even in the very worst ca es.

When the pain comes on there is often violent spasmodic contraction of the muscles of the face; and perhaps it is this which causes the face, on the side on which the disease exists, to become swollen and puffy. The conjunctiva of the eye on that side looks red and blood-shot. The pain, I say, generally begins in the cheek; and often it is altogether confined to the parts to which the second branch of the fifth pair of nerves is distributed; but in extreme cases it will sometimes extend to the forehead, that is, to the parts supplied by the first branch of the fifth pair of

nerves; and to those supplied by the third branch of the fifth pair, that is, to the chin, and even to the teeth. In some cases the tongue and palate are affected also.

In some cases the disease torments the patient for a month, six weeks, or even six months, and then, without rhyme or reason, vanishes, and he continues well for an uncertain period: then it recurs, and continues as long or longer than before. In other cases the disease may vanish, not for a time but altogether, the patient obtaining a complete recovery. In other cases, again, there is never an actual giving way of the disease; it goes on tormenting the patient day after day, month after month, year after year; and in some of these cases other symptoms ultimately supervene, and the disease proves fatal. But of this I shall speak again hereafter. In addition to what I have already stated, it is worthy of notice that the disease attacks only one side of the face; I never saw it in both sides.

On what cause do these symptoms depend? Many persons thus affected have a bad tooth, and they generally go and get it drawn, it being thought that the carious tooth may be the cause of the pain. I never knew a case myself where the patient was relieved of genuine tic douloureux by the extraction of a carious tooth; and I remember that in a conversation which I had some years ago with a very experienced dentist, he told me that he had frequently been called upon to draw bad teeth where the patient had laboured under tic douloureux in the face, and he could not remember that the operation had ever been of any service. I have said that the disease may depend on different causes. Sir Henry Halford has published a paper, in which he mentions some cases bearing all the character of genuine tic douloureux, in which the symptoms seemed to be connected with a diseased condition of the bones of the face; and I have no doubt that such is their origin in some instances. There was a man in this hospital suffering from a pain in the face and cheek, having all the characters which I have just endeavoured to describe, and in whom there was disease of the bone of the upper jaw. If I remember right, for I have preserved no notes of the case, he went through a course of sarsaparilla; a portion of the bone exfoliated, and after this the pain was very much relieved. I saw another case where there was pain very like that of tic douloureux existing in combination with disease in the bones of the upper jaw, but of which I know not the result. But these are rare instances. There is no diseased bone to account for the pain in ordinary cases. Then from what else may it arise? You will find it sometimes in young women of hysterical constitution, a product of hysteria. Where there is hysterical pain referred to the part in which the branches of the fifth pair are distributed, it assumes the form of tic douloureux. Then at other times the pain is intermittent and periodical, depending on that peculiar state of the system which may produce the phenomenon of ague, and may be cured as ague is cured, by quinine or arsenic. In other cases, again, the disease evidently depends upon the state of the digestive organs, and the patient is cured by great regularity as to diet, and a course of medicine which is calculated to put the digestive organs into a more healthy condition. In another order of cases, the pain in the face is the result of disease in the brain. The late Dr. Pemberton, who was for many years physician to this hospital, and was engaged in a large practice at this end of the town, in the midst of his career of prosperity became affected with tic douloureux, and suffered from it in the most horrible manner. I never saw any individual, under any circumstances, suffer more. He went into the country, and died with symptoms of disease in the brain.

There was a gentleman who had tic douloureux in the face for a very long time. The pain at last left the face, and then he was attacked with fits of epilepsy. As the pain left the face when the patient became affected with epilepsy, that alone seemed to be sufficient ground for believing that there was some disease in the brain. After that, however, there was a ptosis, or a dropping down of the upper eye-lid, on the same side on which the tic douloureux had existed. After a more than usually severe epileptic fit, he fell into a state of apoplexy, and died. Mr. Green, Mr. Freeman, and myself, who had attended him, examined the body after death. We found all the membranes of the brain bearing marks of chronic inflammation; the vessels connecting the *dura mater* and the bone unusually large; the *tunica arachnoides* thickened, and at the upper and back part of the left hemisphere of the cerebrum adhering to the inner surface of the dura mater, in a spot about an inch in diameter. The cerebrum generally was soft and vascular, exhibiting a red mottled appearance on many places. The softening of its substance was most distinct in the crura cerebri, fornix, and adjacent parts. The nerves of the fifth pair were carefully dissected to the extremity of the cavernous sinus, but presented no morbid appearances.

There are still other cases in which you cannot trace tic douloureux to its real source. There is something or

other somewhere or other in the system, which acts as a source of irritation to the nerves of the face; but where that something is, and what it is, we cannot discover. Indeed, generally speaking, I should say that nothing is more difficult than to trace any local nervous affections to their real source. The disease may be in one part of the body, and the pain or spasm which it produces may be in another. I have known a patient have violent neuralgia of the foot, which depended on a stricture of the urethra, and which, whenever it occurred, was invariably relieved by the use of a bougie. I have known another patient have neuralgia of the foot depending on internal piles, which came on when the piles were protruded through the anus, and went away when they were reduced. I have known a spasmodic wry neck, or a nervous pain in the back, to alternate with insanity.

If it were worth while to do so, I might mention other cases illustrative of this observation, that the disease may be in one part of the body, and, from some nervous connexion, it may produce pain in some other part of the body. We cannot explain the matter much further than this. I may, however, venture to make this additional observation—namely, that there is good reason to believe that the seat of the nervous communication, on which those sympathies depend, is for the most part not in the nerves themselves, but in a higher place—in the brain, or in the spinal cord.

Treatment.—The treatment of *tic douloureux*, of course, must differ in different cases. In some instances it may be relieved by one method; in others, by another; but in the greatest number of cases it cannot be relieved at all. A very old operation, which had fallen into disuse, but has been revived of late years—namely, that of dividing the trunks of the nerves, to the extremities of which the pain is referred. It has been said that if the pain be referred to the extremity of the second branch of the fifth pair of nerves, you should divide the second branch where it passes out of the infra-orbitary foramen on the face; that thus you will cut off the communication between the extremities of the nerve and the brain, so that the painful sensation may no longer be communicated to the sensorium. Now this would do very well if the seat of the disease were really in the extremity of the nerve; but there is no reason to believe that it is so, and there is every reason to believe the contrary.

The irritating cause, whatever it may be, manifestly acts not on the extremity of the nerve, but on its origin; and both reason and experience prove that the division of the nerves below the origin is of no service. I have myself performed this operation without the smallest benefit to the patient.

In the late Dr. Pemberton's case the branches of the nerves were divided by Sir Astley Cooper. Sir Astley did not recommend it, and, if my recollection be accurate, when Dr. Pemberton first applied to him to do it, he declined acceding to his wishes. He did it at last in order to satisfy the patient; but the division of the nerves, instead of giving relief, very much aggravated the evil. It is altogether an unscientific operation, from which we have no more right to expect any benefit than we should have if we were to amputate the testicle, because pain was referred to it in consequence of a calculus being lodged in the ureter.

In those cases in which the disease has an intermitting and periodical character, you can always relieve it, as you may all other cases of intermittent and periodical disease, by the exhibition of quinine, bark, and arsenic. But then, if you give quinine, it must be in large doses; you must begin with ten grains, and go on increasing it. I saw this very morning a gentleman who had formerly a nervous pain in the back, almost as bad as tic douloureux in the face. It was intermittent and periodical. I told him, when he consulted me about it, that I was sure that quinine would cure him. He took ten grains without benefit; he took twenty with little benefit; and was not cured till he took half a drachm daily. He remained well for two or three years afterwards. The combination of bark and arsenic, also, is an excellent remedy in these cases of intermittent and periodical disease; but I generally prefer giving quinine first, because it is a more innocent medicine, requiring no watching, and not subject to the inconveniences which belong to the use of arsenic.

I was consulted, in conjunction with another practitioner, concerning a young lady who had tic douloureux of the face. She was hysterical, and the disease had followed the occurrence of some circumstances which had occasioned great agitation of mind. The case was evidently connected with hysteria and an irregular state of the menstruation. We gave her steel and ammonia in combination, which put her into better health, and in the course of a few weeks the tic douloureux, which had existed for many months, had disappeared.

If you can really trace the pain to disease in the bones of the face, you must of course, instead of directing your attention to the pain which is the symptom, endeavour to cure the disease in the bone which

produces it. A piece of bone may exfoliate; and if the dead fragment has caused the pain by pressing or otherwise irritating the trunk of a nerve, the pain may thus be removed; or perhaps the patient may get well under the use of sarsaparilla, which, as you know, acts most beneficially in a number of cases of disease of the bones; or if sarsaparilla fail, you may serve your patient by the exhibition of calomel and opium, oxymuriate mercury, some preparation of iodine, or the mezereon; every one of which may in its turn be advantageously resorted to in cases of disease of the bones.

In cases where the pain depends on an organic disease of the brain, you must of course turn your attention to the primary affection, although it is probable that in the majority of these cases you will be able to render the patient but little real service.

But supposing that you can trace the disease to no other source, and that you find the tongue furred, the bowels confined, and other indications of an ill performance of the digestive functions, you have a right to conclude that this very probably is the origin of the pain in the face; at any rate you are called upon, in the first instance, to ascertain what will be the result of putting the digestive functions in better order. It was upon this principle that I proceeded in the case to which I called your attention in the beginning of the lecture; and you see that the practice has answered so far wonderfully well. As the bowels were opened, and the tongue became clean, so the pain abated. A great number of diseases depend on the state of the digestive organs. You will meet with examples of this every day; and there is nothing more remarkable in a patient having tic douloureux from a deranged state of the digestive organs, than there is in having sick headache in consequence of an overloaded stomach, or a lumbago from costive bowels.

But supposing that you cannot trace the disease to its real source,—that the patient is in other respects well,—that all the functions are well performed,—that there is this frightful pain, and you have no clue to lead to the real seat of the original malady, and therefore no clue to the practice you ought to adopt,—you are driven to the expedient of trying remedies at hazard,—a very unsatisfactory mode of proceeding, it must be acknowledged, but you have no alternative. You may give the patient quinine, which is useful in many cases of nervous pain, even though it be neither intermittent nor periodical; or you may give carbonate of iron, which I do not hesitate to say relieves many neuralgic affections also. Half a drachm of the carbonate may be given three times a-day, and the dose may be gradually increased to a drachm. I never saw any good arise from pushing the use of the carbonate of iron beyond this; and I can easily conceive that much evil may arise from its being given in those enormous doses in which, if I am rightly informed, it is given by some practitioners. It is easy to conceive that when thus exhibited, the bowels may be actually clogged by it, just as in other cases they are found clogged by cubebs or by Ward's paste. Whenever you give these insoluble substances, you should give an occasional purgative, to prevent the accumulation of an insoluble mass in the bowels. I heard of a patient who died of inflammation of the bowels, in consequence of taking large doses of cubebs, which were not purged off. So I can conceive that inflammation of the bowels may be produced by the large doses of carbonate of iron being suffered to accumulate in the intestines.

If the quinine and carbonate of iron fail, it may then be worth while to try the effect of zinc or copper, or some of those other metallic salts which are occasionally useful in cases of chronic nervous affection.

But supposing that you have tried all ordinary means without benefit, are you to go on *ad infinitum* tormenting the patient with medicine? The first rule of our art is to do no harm; and if you have tried all reasonable expedients without benefit, you had better not go on to further experiments. No one can be dosed constantly with medicine without the health being injured by it, ultimately, if not immediately; and if you have not some reasonable grounds for giving medicine, you should not run the risk of doing harm by its continued exhibition. It is much more wise and honest, when you do not know what to do, to advise your patient to wait, and take the chance of the pain subsiding of itself, as it does in many instances. But where you cannot cure your patient, you may often succeed in making his life less intolerable than it would otherwise be. Some patients are capable of being much relieved by the use of opium, and among them there are a few with whom opium never disagrees, so that they may take it without harm. Even in these, however, it should be given only when the pain is more than usually severe. Let them avoid taking it constantly, because then the opium loses its effect. In slighter cases, the patient may perhaps be benefitted by ext. of lettuce, ext. of henbane, or some other of the slighter narcotics. In all cases the patient is likely to derive advantage from avoiding

as to diet, and mode of life in other respects, irregularities, including all unusual demands on the nervous system, great mental exertion and anxiety.

In the present case, one of the first things which I did was to direct that the part should be rubbed with the veratrine ointment. This has been lately proposed as a remedy possessing a most extraordinary influence over a number of diseases, neuralgic affections among the rest. I saw one patient who thought himself relieved by it of a pain in the forehead, connected with disease of the frontal bone in the neighbourhood of the frontal branch of the fifth pair of nerves. I was, however, by no means satisfied that the relief really arose from the use of the ointment; and in several other cases I have had recourse to it without the smallest advantage: however, there could be no objection to the use of it on this occasion, and I thought it worth while to make one experiment more—you have heard the result.

Although I employed the veratrine ointment in this instance, I am not one of those who would be trying indiscriminately all the new remedies which, in these days, are being constantly brought before the public; nor can I think well of this modern fashion of resorting on all occasions to novel methods of treatment. I see many practitioners who would always rather give a new medicine than an old one, but I advise you, if you wish to succeed in your profession and to be useful to the public, to pursue a different course. Make yourselves masters of the old remedies. Learn how to handle them, and what good they will do, and as a general rule, have recourse to them in the first instance. If the old remedies fail, and you are at a stand-still, then, and not till then, have recourse to the new ones. If you always begin with new remedies, you throw away all the valuable results, not only of your own experience, but of the experience of those who have gone before you. You have to begin, as it were, *de novo*, and the first consequence of this will be that you will not cure your patients; and the second, that you will have none to cure. Where old remedies fail, I say that it is not only not unreasonable, but proper, that you should ascertain what can be done by new ones; but it is very unwise to employ the latter, where there are sufficient grounds to believe that those already in use will answer the intended purpose. I should be very sorry to see the march of science impeded by an unjust apprehension of experiments and innovations; but, surely, there is a broad enough line between a discreet and prudent use of new remedies, and that indiscreet and hasty use of them which we find to prevail in the practice of the medical profession at present.

PAROCHIAL SURGEONS AND THEIR VILIFIERS—

THE DEFENDERS OF THE NEW POOR LAW SYSTEM.

[We give insertion to the following letter (from the *Times*), as it was partly promised by our correspondent, Ruricola, in his last communication to us.]

Sir,

In your paper of the 28th of November I read a letter from a guardian of the poor, signed "Rusticus," vindicating the arrangements of the poor law authorities in the matter of medical relief.

As this is, I believe, the first public attempt to defend what is so generally considered to be indefensible, I looked with care through his communication for arguments in support of his views. I was, however, surprised to find that it contained nothing but a quotation of regulations which were already sufficiently well known, an unauthorized contradiction of authorized statements, and an unwarrantable attack on a large body of men whose characters, fortunately, stand too high to be injured by his representations.

With regard to his first point—viz. "that medical men are bound to attend on all sick paupers resident within the union," and his assertion that "medical men very generally have wished to interpret this as relating only to those on the parish books," I can affirm, from a much more extensive acquaintance with the opinions of general practitioners than it is possible that "Rusticus" could obtain, that they do not wish so to interpret this clause, but are glad to take it in its literal meaning; that it is regarded by a large majority of the profession as a most desirable preventive to the vexatious and injurious mode of attendance on extra parochial poor formerly in vogue; and that, so far from their craft being in danger, they are well satisfied that that practice, extremely troublesome in its nature, and uncertain in its remuneration, should be abolished.

The insinuations of the Poor Law Commissioners, and the bold assertions of "Rusticus," as to the profitable nature of this attendance under the old system, are

wholly without foundation; for, in the first place, parish authorities were accustomed to take a most dishonourable advantage of medical men, by availing themselves of their readiness to attend sick non-parishioners in cases of emergency, and then protracting or altogether refusing an "order," thus leaving the medical man without any legal claim for his previous exertions and supply of medicines, the charges for which were therefore scarcely ever allowed.

2d. These charges were usually at a much lower rate than ordinary professional demands.

3d. No proof is alleged that these bills, however large in some instances, were not fairly proportioned to the amount of labour and expense bestowed by the attendant, and for which he had no alternative left but to send in an account. Not only, therefore, are the parish medical men most unjustly charged with the evils arising from the system itself, but certain gentlemen, recently intrusted with power under the Poor Law Amendment Act, constitute themselves judges of the remuneration due to a medical man for his professional exertions

I believe that no other liberal profession can be treated in the same way. The charges of a lawyer, if exorbitant, may be referred to Masters in Chancery, &c., members of their own body, and redress obtained from them; but what attorney or solicitor would bear to have his bills submitted to the taxation of his clients?

4th. The attendance on parishioners residing at a considerable distance from the parish was generally part of the contract; a limit of six, or even of ten miles, was not uncommonly specified as the distance within which the medical officer was to attend, free of extra charge. The losses sustained in this way, and by the before-mentioned conduct of overseers and vestries, were such as to render an alteration as desirable to the medical profession as to the rate-payers.

"Rusticus" further announces his opinion that relieving officers are the most competent persons to give orders for medical relief; as he gives no reason for this unaccountable conclusion, I might be content to let it pass without notice, feeling assured that few of your readers who have paid attention to the subject would agree with him. Let me, however, remark, that so long as the power of giving orders depends on the caprice or convenience of a relieving officer, the poor must suffer a needless hardship. Accident and sudden illness require of course much more immediate attention than applications for weekly pay or provisions. The circumstances are totally dissimilar, and the officer who may be qualified and prepared to bestow sufficiently prompt attention on one set of applications, may be, on various accounts, quite unable, as well as unsuitable, to act as the necessities of the case may require in the other.

But "Rusticus," in the plenitude of his knowledge, coolly asserts, "your correspondent 'Ruricola,' is wrong in saying that in some unions a like power is granted to overseers and guardians;" and supports his criticism by a reference to the proceedings in "our union." Now, although I doubt not that this union affords an ample field for observation, I beg to tell him, that I deduce my statement not only from one union, or from half a dozen, but from an extent of country that would make "our union" look rather insignificant. I therefore repeat my former remark, with the additional information that the mode of granting orders is not the same in all unions, some leaving it entirely to the relieving-officer, some intrusting it freely to the guardians and overseers, and others adopting a medium course.

And now, sir, I come to your correspondent's summing up: "Let me warn you against receiving all the statements of medical men on these points. It is their interest to misrepresent the whole of the Commissioners' arrangements on the head of medical relief." A modest accusation truly! Pray what would be said of any medical man who should descend to the ungentlemanlike course of aspersing the motives of the Poor Law functionaries by such remarks as the following:—"Let me warn you against receiving all the statements of the Poor Law Commissioners in their report on medical relief. It is their interest to misrepresent that matter, for in proportion as they reduce any part of parochial expenditure, so do they secure their continuance in office, and their receipt of 2,000*l.* per annum for a longer period;" or, "Do not listen to the statements of guardians; they are considerable rate-payers. It is their interest to misrepresent an expenditure which so nearly affects their own pockets, their object is only to save themselves, whatever may be the miseries of the poor."

If "Rusticus" has no better argument left than to asperse the motives of his opponents, let him prudently forbear; he is handling a weapon which may be turned with double force against himself.

Such, sir, however, are the arguments of those who attempt to defend the present system of parochial medical relief: un-

able to support their proceedings by a reference to the broad principles of humanity and justice, they satisfy themselves, and hope to succeed with the community, by slandering the laborious and ill-requited body of parochial surgeons — gentlemen whose disinterestedness, whose gratuitous exertions, and whose devoted attention to the wants of their suffering fellow-creatures, are probably not surpassed by the excellent qualities of the worthy "guardian" himself.

Your early insertion of this letter will be conferring an additional favour on

Your much obliged and very obedient servant,

RURICOLA.

THE OLD AND NEW SYSTEMS OF PARISH ATTENDANCE.

To the Editor of the Medical Gazette.

SIR,

THE Editor of a contemporary periodical has for some time past manifested symptoms of uneasiness on the subject of the present Poor Law medical contract system, apparently undecided as to the course he should pursue; whether to uphold the profession at the expense of the Commissioners, or the Commissioners at the expense of the profession. Probably if he could have divested his mind of all political associations for the time, and given the subject a fair and impartial consideration, his known tact and ingenuity might have led him to the suggestion of some plan whereby the evils of the present wholesale contract system might in some degree be mitigated. He asks, what is the plan which is to be sanctioned as a substitute? what plan can medical men themselves suggest?

Not being vested with senatorial dignity, it is not to be expected that I should be sufficiently versed in the art of legislation to answer satisfactorily the honourable gentleman's query; nor do I think that it admits of a very ready reply. Looking at the differences which present themselves in all the various unions, it is scarcely to be expected that any one general plan can be laid down, which shall be applicable to all cases. Until some such plan can be devised, I would recommend that the Commissioners should, as a general rule, desist from interfering with existing contracts, and leave it entirely in the hands of the local authorities. It is generally admitted that the old contract system was bad, and in many instances injurious and oppressive, but it will not bear a comparison with the present. Economy has been sufficiently observed, and care enough has been taken by overseers for some time past that the *parish doctor* should not be *too well paid*; and from the knowledge I have of parochial contracts, I feel justified in asserting that no injustice would be done to rate payers by their continuance.

Let the rule be, "to continue the contracts as they are found:" exceptions would, of course, arise; as for instance, where the contracting practitioner lives at such a distance from a parish as to render applications to him by the poor difficult; where the rate of remuneration being higher for any particular parish than for those in the neighbourhood, a saving can be effected by fair and open competition; and in many other cases which could quite as well be judged of by the local authorities as by the Commissioners. I do not mean to assert that, by the adoption of this rule, an equal and just rate of remuneration would be established throughout the kingdom; but I apprehend that such a course would carry much less of tyranny, of injustice, and of petty oppression, upon the face of it, than the compulsory one now in use, and might therefore be adopted until the legislature can provide an efficient and permanent remedy for the evils now so loudly and so justly complained of.

Hitherto parochial contracts have generally been undertaken by medical men with reference to their general practice; and even if it were politic, which I must take leave to doubt, few will be found to assert that it is either just or fair that such arrangements should be wantonly and unnecessarily destroyed—that they should be broken up or interfered with, unless the amount of benefit to the public from such interference be at least equal to the amount of injury inflicted upon the profession. It is to be feared that in many instances country practitioners will find that their practices are much cut up, their means curtailed, themselves deprived of comforts which habit and a somewhat advanced age may have rendered almost essential, the hopes and expectations of their families blighted, and every chance of any future provision for them altogether destroyed; not because any good was to result to the community from the means taken to effect this, but simply because the Poor Law Commissioners, in the plenitude of their liberality, had so willed it. To take a parish from a man who may have had the charge of it for more than a quarter of a century, and whose talents are known and appreciated, merely to give it, perhaps, to some youth

whose only known qualification is his pliant submission to the commands of the Central Board, is too obvious an act of injustice both to the poor and to the profession, to need any comment: equally so is it to amass together several thousand human beings, merely because they all happen to exist within a defined space, and insist upon one man taking charge of the whole, whether it may be compatible or not with his other engagements, or whether it be possible or not for him properly to execute the duties imposed upon him: and still more iniquitous is it arbitrarily to fix upon a rate of remuneration admitted on all hands to be totally inadequate, and to compel acquiescence on the part of the profession by a threat, in many instances carried into execution, of introducing some protegé of theirs, whose character and future conduct may, with tolerable accuracy, be predicted by reference to the character in which he appears. The barefaced injustice,—I may say iniquity, of the present system, is such that I am truly astonished that any set of men of gentlemanly feelings and correct principles can have brought themselves to adopt, or even to sanction it. To the profession I would earnestly recommend that they should keep themselves aloof from, and refuse all intercourse with any intruder who may appear amongst them under such auspices. I would also recommend that all cases of insufficient attendance, whether from neglect or otherwise, should be made known, so that the public may not only know, from time to time, what savings have been effected, but also at what expense of life and misery such savings have been purchased.

Your obedient servant,
EDWARD BARBER.

Cambridge, Dec. 23, 1835.

CURE OF WOUNDS WITHOUT INFLAMMATION.

LETTER FROM DR. MACARTNEY.

To the Editor of the Medical Gazette.

SIR,

I BEG to state, through the medium of your journal, that the paper on the cure of wounds without inflammation, extracted from the Memoirs of the Royal Academy of Medicine, is much changed since it passed out of my hands. Any writing must suffer in style and perspicuity from being translated first into French and again into English; but that would be a matter of minor importance. In my paper, however, there are not only typographical errors, but some passages with a contradictory meaning: the paper is throughout abbreviated, and some essential parts omitted altogether; one in particular, where I claimed the priority with respect to the use of what the French call *irrigation*. There were three lithographic figures, explaining the mode of generating and applying steam, of which no notice is taken.

I take it for granted that the publication in your journal is a translation of the paper as it has been printed by the French Academy, and that the mutilations were made in other quarters, partly from persons not understanding the principles announced in the paper.—I am, sir,

Your obedient servant,
JAMES MACARTNEY.

Dublin, Dec. 23, 1835.

SIR GILBERT BLANE'S MEDAL.

(*From a Correspondent.*)

WE announced, now several years ago, that the venerable and distinguished Sir Gilbert Blane had sunk a considerable sum of money for the purpose of awarding a Gold Medal every two years, to two such Medical Officers of the Navy, serving afloat, whose treatment of diseases and accidents on board of men-of-war was most approved of by the principal Medical Officers of the Navy, and the Presidents of the Colleges of Physicians and Surgeons—the selection of the successful competitors being made by the three named functionaries, after a careful examination of the Surgeons' journals.

Since that period, four awards of the medals have taken place, but none were forthcoming, in consequence of the great delay in cutting the die, and which was a source of great anxiety and uneasiness to the founder, during the last two years of his protracted existence. This, however, has been now accomplished by the completion of the die, and the medals are struck off. It is beautifully executed, and bears a most correct likeness of Sir Gilbert Blane on the *obverse*—as, at least, we understand from a friend, who for 36 years was intimately acquainted with him, and who was shewn the medal by the kindness of Sir William Burnett, of the Admiralty, the present Physician-General to the Navy. The *reverse* is a wounded seaman, apparently lifeless, supported in the arms of another. An erect female figure stands by with a *trident* in her hand (Britannia), and behind her feet a *serpent* entwined with an *anchor*. The motto over the reverse is "Mente Manuque." The money and the die are deposited at the Royal College of Surgeons.

Dec. 29, 1835.

CONCOURS AT STRASBURG.

A CONCOURS was recently held for the Chair of Legal Medicine, vacant in the Faculty of Strasburg by the death of M. Foderé. Two candidates presented themselves, and went through the requisite trials in presence of a large number of spectators. But the judges were dissatisfied, and unanimously determined not to make any nomination. There will consequently be a new contest for the appointment.

COETANEOUS PARALYSIS.

NOTE FROM MR. NOBLE.

To the Editor of the Medical Gazette.

SIR,

You will oblige me by noticing, in your next week's journal, a typographical error, in the publication of my remarks on the subject of Taste and Common sensation, which appeared in the Gazette of last Saturday. I am made to speak of *cutaneous* paralysis of two functions in the same structure; when I wrote, as will be seen on reference to the manuscript, "coetaneous paralysis, &c." This error might be of but little importance, did I not refer, in the next sentence, to cases of simultaneous abolition of motion and sensation as exemplifying such coetaneous paralysis; thereby rendering the novel expression "*cutaneous* paralysis," very much an absurdity.—I am, sir,

Your obedient servant,
DANIEL NOBLE.

Manchester, Dec. 29, 1835.

PHRENOLOGICAL SOCIETY—

LETTER FROM DR. MACLEOD.

To the Editor of the Medical Gazette.

SIR,

THE last number of the Lancet contains what professes to be a report of the proceedings of the Phrenological Society, from which it appears that a recent meeting was devoted to the consideration of certain editorial observations published in the London Medical Gazette, some months ago, in reference to a skull supposed to be that of Dean Swift. Seeing that my name was introduced into the report, and mixed up with much impertinence, in such a manner as to convey the impression that this had been done by the author of the paper, I wrote to Mr. Fearnside, the Secretary of the Society, requesting to be informed whether such was actually the case. To this Mr. Fearnside replied, "I have no recollection whatever of your name having been mentioned by Mr. Hawkins while reading his paper, or, in fact, during the conversation that arose upon it."

It would thus appear that the introduction of my name, the citation of passages with inverted commas as my words, and even the conjectural explanation of the author's motive in selecting me as the victim of his criticism, namely, because "Dr. Macleod professed to be a medical man"—all this, I say, appears to be an unauthorized and dishonest interpolation.

Various circumstances have led to a general belief that the accounts of the proceedings of the Phrenological Society, which appear from time to time, are furnished by a common reporter; but whoever the party may be, as at least he "professes" to be a phrenologist, I take leave to remind him that there are faculties called candour and honesty, which, when but moderately developed, deter men from unprovoked aggression, and prevent them from quoting as the words and opinions of any one, expressions and sentiments which the person named has never acknowledged, and which, as in the present instance, he may have become first acquainted with, like his assailant, from seeing them in the pages of a public journal. It is true, I have not thought it necessary to take notice of the numberless instances in which language and sentiments which were never mine have been attributed to me by the Lancet, because I know that its abuse is part of a system, that it is "a purposed thing, and grows by plot;" but the present case is somewhat different, and I notice it solely because one who, for aught I know, may be a respectable person, is represented—and it would appear falsely represented—as taking impertinent liberties with my name, and ascribing to me a paper of which *I am neither the author nor editor*.

But I have also another motive in making this declaration—my dislike to appear in borrowed plumes; for the paper thus attributed to me is, in my humble judgment, a very clever one, and demonstrates most convincingly, as it is evidently intended to do, either that the skull in

question was not Dean Swift's, or that the present system of phrenology is little better than an ingenious fiction.

I am, sir,
Your obedient servant,
R. Macleod, M.D.

23, Henrietta Street,
Dec. 31, 1835.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN DECEMBER.

George Harrison, Great Marlborough-Street.
Alexander C. Morison, London.
T. W. Atkinson, Broughton House, Cartmell.
John R. Wall, Ross, Herefordshire.
Astley Purton, Alcester, Warwick.
George Vaux. Birmingham.
John Lawrence, Brighton.
Henry Bell, Cockermouth.
William A. Cox, Bath.
James K. Parkinson, Hoxton Square.
Samuel K. Parson, Campsey Ash, Salop.
Robert G. Higgins, Newport, Salop.
Henry Weekes, Chatham.
Robert T. Maginniss, E. I.
Charles Stokes, London.
Robert Wilson, Milnthorp.
Andrew M. Needham, Wenn.
James Watson, Sheffield.
James Whitehead, Westwood, Oldham.
James Berry, London.
Richard Jones, Suffolk.
Henry Wilkins, Portsmouth.
Gerrard Potter, Adlington, Lancashire.
Edward J. Staples, Bristol.
John W. Potter, Ongar.
Robert Stevenson, R. N.
Ronald Montgomery, Brentford.
Percival Leigh, St. Cross, Hants.
David N. Carr, Alnwick.
James C. A. Staig, A.
Luke D. Fitzgerald, Ballinarobe, Mayo.
Arthur Stillwell, Uxbridge.
John Bowes, Richmond, Yorkshire.
Thomas Lowe, Solihull, Warwickshire.
Thomas F. Sagar, Leeds.
Anthony C. Clifton, Islington.
Robert V. Leese, South-Street, Finsbury.
William Clark, Devizes.
Thomas Baskerville. Canterbury.
Frederick Cripps, Wisbeach.
Joseph Ward, Birmingham.
George J. Jones, London.
Charles Cooper, Killala, Mayo.
John W. Pearce, Cambridge.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

December 23, 1835.

Geo. Warren Watts Firth, near Norwich.
John Barrett, Corton, Somersetshire.
Edward Hugh Thorpe, Hastings, Sussex.
Charles Bodenham Garrett, Bushey, Herts.
James Compigne Chase, Northampton.
Peter Gregson Heatley, London.
Alfred Heale.
James Buckland, Malmsbury.
Edwin Clarke, Birmingham.
Cardinal Brewster, Tolleshunt D'Arcy.
Richard Nickols, Cawthorne, Yorkshire.

December 31.

William Brook Addison.

LITERARY INTELLIGENCE.

Shortly will be published, a Treatise on the Analysis of Blood and Urine, in Health and Disease; with Directions for the Analysis of Urinary Calculi; by G. O. Rees.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Dec.* 29, 1835.

Abscess	2	Heart, diseased	3
Age and Debility	42	Hooping Cough	1
Apoplexy	4	Inflammation	20
Asthma	19	Bowels & Stomach	2
Cancer	1	Brain	6
Childbirth	4	Lungs and Pleura	5
Consumption	62	Insanity	1
Convulsions	29	Measles	5
Croup	2	Mortification	4
Dentition or Teething	4	Paralysis	3
Dropsy	11	Small-pox	20
Dropsy on the Brain	21	Spasms	1
Fever	2	Thrush	1
Fever, Scarlet	11		
Gout	2	Stillborn	18

Increase of Burials, as compared with the preceding week . . . 155

METEOROLOGICAL JOURNAL.

Dec.	Thermometer.		Barometer.	
Thursday . 17	from 28	to 39	30·28	to 30·21
Friday . . 18	32	46	30·03	29·93
Saturday . 19	30	36	29·92	29·90
Sunday . . 20	28	34	29·89	29·97
Monday . . 21	27	33	30·01	30·15
Tuesday . . 22	21	34	30·24	30·40
Wednesday 23	12	31	30·44	30·45

Prevailing winds, N.W. and N.E.

Except the 22d, generally cloudy; rain on the morning of the 18th; a little snow on the afternoon of the 19th and 23th.

Thursday . 24	from 26	to 31	30·28	to 30·33
Friday. . . 25	10	28	30·30	30·27
Saturday . 26	13	27	30·28	30·27
Sunday . . 27	13	36	30·19	30·17
Monday . . 28	32	46	30·08	30.02
Tuesday . 29	31	42	30·13	30·20
Wednesday 30	37	46	30·14	30.21

Wind N. and N.W. till the 27th; South and and S.W. on the 28th and 29th; and N.E. on the 30th.

Except the 25th, generally cloudy; a dense fog on the morning of the 26th; rain on the afternoon of the 30th; the frost suddenly left us on the 27th, and as suddenly returned on the night of the 30th, as might be expected, being an almost invariable rule after a fall of rain. The thermometer has not been so low since the 3d of February, 1831. On Christmas day, 1830, the thermomoter was however, nearly 2 degrees lower, being 8 deg. 5 min.

ERRATA.

In Dr. Lyons' paper, present No. (in part of our impression) p. 520, *for* Galvactorrhea, *read* Galactorrhea; and in p. 518, col. 2, for Adipis Nullæ, *read* Adipis Suillæ; also *for* Aquæ Puræ, ʒvi., *read* Aquæ Puræ, ℥vi.

Wilson & Son, Printers, 57, Skinner-St. London.

THE
LONDON MEDICAL GAZETTE,
BEING A
WEEKLY JOURNAL
OF
Medicine and the Collateral Sciences.

SATURDAY, JANUARY 9, 1836.

LECTURES
ON
MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XV.

THE MEDICINAL LEECH—ITS PROPERTIES AND USES.

IN my last lecture, I gave an outline of the structure of leeches, and pointed out the zoological characters of those used in this country. It is my intention now to notice the different modes of application; the manner in which these animals perforate the skin; their effects; and uses.

As most persons have had more or less experience in applying leeches, I do not think it necessary to dilate much on this subject. *First*, let the part be well cleansed (sometimes it may be necessary to shave it): then dry the leeches, by rolling them in a clean linen cloth: place them in the lid of a pill-box, and apply to the affected part. This, in my opinion, is preferable to applying them by the fingers, or in a wine glass. A narrow tube (called a leech-glass) will be found useful when we wish to affix one of these animals to the inside of the mouth, or any particular spot.

It is necessary to beware of several circumstances which influence the fixing of leeches. The *first* is the condition of the animal, whether healthy or otherwise. *Secondly*, the nature and condition of the part to which it is applied; thus, leeches will not readily attach themselves to the soles of the feet, or the palms of the hands, or to hairy parts—the presence of grease, vinegar, salt, and some other substances, will prevent their biting; whereas milk, sugared-water, and blood, are said to have the contrary effect. Scarifying the part has been advised to promote their attachment. *Thirdly*, the condition of the patient will affect the fixing of the animal. Derheims says that leeches will not bite those under the influence of sulphur, on account of the evolution of sulphuretted hydrogen by the skin. *Fourthly*, the effluvia, or vapours of the room, as the fumes of tobacco, sulphur, vinegar, &c., it is said by the same authority, will prevent them biting, or even cause them suddenly to fall off.

The quantity of blood a leech is capable of drawing varies considerably. I believe four drachms to be the maximum. In one instance within my own knowledge, this was found to be the quantity, by weighing the leech before and after the operation. This, however, is a rare case. On an average I do not think we ought to estimate it at more than one drachm and a half. Of course this has no reference to that lost after the animal has fallen off, and which varies according to the vascularity of the part, in children being oftentimes very considerable. When the leech has had sufficient it drops off; but it is said that if the tail be snipped, the animal will continue to bite, the blood passing out posteriorly as fast as it is taken in by the mouth. I have tried several, but I find they usually let go their hold the instant the tail is cut. Cloquet has made the same remark.

In order to disgorge the leech of the blood, the usual practice is to apply salt to its body; but it is objectionable, (if you wish to preserve the animal) since the surface is frequently thereby blistered, and several days elapse ere the creature regains its former activity. Some advise squeezing the blood out by the mouth; others the application of vinegar to the head. If no kind of emetic be employed, the blood remains for a considerable time in the stomach of the leech undigested, but without putrifying.

After-treatment. — When leeches have fallen off it is generally desirable to promote the sanguineous discharge, which is best done by the use of warm fomentations or cataplasms; or even, in some cases, by cupping-glasses. When applied to children, be cautious as to the directions you give respecting the promotion of bleeding: your instructions may be misinterpreted either through ignorance or wickedness. Some years since, the application of a leech was ordered to the chest of a child labouring under pneumonia; it was at the same time mentioned that the bleeding should be encouraged. The directions were literally fulfilled—the discharge of blood assiduously promoted—until so large a quantity had been lost, that death was the result. No attempt was made to stop it, nor notice sent to the Dispensary, in the practice of which the case occurred. The child being illegitimate, and the mother evidently careless of its recovery, led some to suspect that this did not take place through mere ignorance. In another instance, two leeches were ordered for a child aged about eighteen months, suffering with pneumonic inflammation, a consequence of measles. The following day the poor little creature was found in a fainting, or rather dying state, with face and lips completely blanched. On inquiry it appeared the leech-bites were still bleeding, and no attempt had been made to stop the discharge, the mother thinking it would be beneficial, more especially as the pneumonic symptoms had considerably abated. As predicted, the little sufferer died within twenty-four hours.

In some persons there appears to be an hereditary predisposition to hæmorrhage, so that very slight wounds are attended with serious, and even fatal effects. Mr. Wilson has related the case of a child where one leech had nearly caused death, by the serious hæmorrhage. When about three or four years old, this child bit its tongue, and notwithstanding that every attempt was made to stop the discharge, death took place from loss of blood.

I have been called to many cases of hæmorrhage after leech-bites, and never failed in stopping it by compression. Sometimes mere exposure to the air will be sufficient; or, if this fail, we may apply a dossil of lint and a bandage. In other instances you will not find this succeed, and there are various plans, any of which you may then adopt. I usually employ compression, thus: roll a piece of lint into a fine cone, and introduce it into the bites by means of a needle or probe; over this lay a compress and bandage. Sponge may be substituted for the lint. Various other modes have been proposed; some, I think, exceedingly cruel, since I do not believe them ever necessary. I allude, now, to the application of a red-hot needle; and to passing a needle through the orifice, and wrapping thread round, just as a farrier stops the discharge of blood from the vein of a horse. Some employ absorbing powders, as gum-arabic; or styptic washes, as a saturated solution of alum. One very effectual means is to apply lunar caustic, or the nitrate of quicksilver. Sir C. Bell, in one case, stitched up the wound.

Leeches in the mucous cavities.—The ancients were very apprehensive of the ill consequences likely to arise from swallowing leeches. That their fears were not groundless, is proved from the following circumstance, related by the celebrated Baron Larrey. When the French army entered upon the deserts which separate Egypt from Syria, the soldiers, pressed by thirst, threw themselves on their faces, and drank greedily of the muddy water, and which, unknown to them, contained leeches (*Sanguisuga ægyptiaca*), having the form of a horse-hair, and the length of a few lines only. Many of them felt immediately stings, or prickling pains, in the posterior fauces, followed by frequent cough, glairy spots, lightly tinged with blood, and a disposition to vomit, with a difficulty of swallowing, laborious respiration, and sharp pains in the chest, loss of appetite and rest, attended with great uneasiness and agitation. On pressing down the tongue of the individual first attacked, a leech was discovered, which was with difficulty removed by the forceps. Little or no hæmorrhage followed, and the patient recovered. Those which had attached themselves to the posterior fauces were removed by the use of gargles composed of vinegar and salt water. The Chief of Brigade, Latour Mauberg, commander of the 22d regiment of Chasseurs, swallowed two in the deserts of St. Makaire, a day's journey from the Pyramids, which so much weakened him, that his convalescence was long and difficult.

Derheims relates a case where a young man, who had leeches applied to his anus, was so unfortunate as to have one enter his rectum unnoticed. The animal made several punctures; and was not expelled until some hours after, when salt water injections were used. The wounds caused by the bites, however, did not heal for several months, during which time the patient suffered considerably, and constantly passed blood with the fæces.

Whenever practicable, salt-water injections should be resorted to. Here, however, is a case in which they could not be employed—it is taken from Derheims. Two small leeches were applied to the gums of an infant during the period of dentition, and by the inattention of the nurse they

fixed themselves at the back part of the mouth, and becoming gorged with blood, caused great difficulty of respiration. The infant, by strongly closing the jaws, prevented the removal of the animals, who only ceased their hold when they were filled with blood. The hæmorrhage continued for two hours.

In Dr. Johnson's work on the leech, allusion is made to some experiments by Dr. Stevens, from which it would seem that the leech is acted upon by the gastric juice. Now this, as the author remarks, is contrary to what one would suppose. That these animals may, when taken into the stomach, occasion serious symptoms, and, therefore, are not immediately digested, is proved from the following case taken from the "*Recueil Périodique.*" A lady accidentally swallowed a leech she was applying to her gums. Acute cardialgia soon came on, with a feeling of erosion, and creeping in the interior of the stomach; sometimes convulsive movements in the limbs and muscles of the face; frequency and irregularity of the pulse; universal agitation and paleness of the countenance. The physician who was called in, recollecting the fact ascertained by Bibiéna, that leeches could not live in wine, administered half a glass every quarter of an hour. The symptoms were soon alleviated; and the fourth dose caused vomiting, by which the dead leech was evacuated, with much glairy matter, mixed with clots of black blood. By a proper subsequent treatment the patient recovered in eight days.

Manner of biting.— Let us proceed to examine the mode by which a leech perforates the skin and extracts blood. When about to bite, you observe he first attaches himself by his posterior disk, feeling about with his head until he finds a fit spot to attack. Here I may remark, that, by some writers, the two terminal disks have been compared to cupping-glasses or suckers in their action; and their adhesion to solid bodies, therefore, referred to atmospheric pressure; but in consulting the works of some of our best writers on this subject (Thomas, Carena, de Blainville, and Brandt), you will find this mode of operation denied. If it be true, (and Blainville says it is), that leeches can affix themselves to solid bodies, and draw blood as well in the vacuum of an air-pump as in the open air, it must be admitted the hypothesis of the cupping-glass operation is no longer tenable. But other reasons have also been urged. It is said, that when a leech has attached itself to a glass, you may slide it along even with part of the disk separated, without the animal letting go its hold; and that when deprived both of head and feet, it can fix itself to bodies merely by pressing its belly against them. It appears, then, that adhesion is effected by the immediate contact of all the points of the expanded disk. Such is de Blainville's explanation (and the other authorities quoted coincide with him); but he admits that the attachment of the anterior disk may at first be effected by atmospheric pressure; adding, it is certain that when this adhesion is complete, it is equally made by the immediate contact of the widened lips. But to return to the mode by which the animal perforates the skin.

Having fixed on a spot suitable for his operations, he applies his oval disk, and firmly fixes it (at first, perhaps, by atmospheric pressure; then by intimate contact), so that the anterior end forms an angle with the other portions of the body. The three cartilaginous jaws bearing the sharp teeth, are now stiffened and protruded through the tri-radiate mouth against the skin, which they perforate, not at once, but gradually, by a saw-like motion. Dr. Johnson says, the jaws are carried from side to side in an oblique direction; and adds, "their action may be seen by presenting to the leech a coagulum of blood, and when the leech is in the act of suction, cautiously removing it. For a few seconds it appears unconscious of its removal, which presents a fair opportunity of observing the oscillatory movement of each piercer" (jaw). He likewise infers that the wound is not produced instantaneously, from the circumstance of the gnawing pain continuing for two or three minutes after the animal has commenced operations. Thus, then, it appears that the leech rather tears than cuts the skin: hence the irritation and inflammation frequently produced around the orifices. The flow of blood is promoted by the suction of the animal, who swallows the fluid as fast as it is evolved. During the whole of the operation the jaws remain lodged in the skin. In proportion as the anterior cells of the stomach become filled, the blood passes into the posterior ones; and when the whole of this viscus is distended, the animal falls off. On examination it will be found that not a particle of blood has passed into the intestine.

Effects.—There are two classes of phenomena observed in all modes of drawing blood; one of which has been termed *local*, the other *general*. In phlebotomy and arteriotomy, the first is trifling, and of no therapeutic value; and we resort to these operations only as means of affecting the general system. On the other hand, we obtain topical effects, both powerful and useful, from cupping and leeching; hence these are termed *local*, while the former

are denominated *general* blood-lettings. It must, however, be remembered, that constitutional or general effects are also frequently obtained from both cupping and leeching.

1. *Constitutional or general effects of leeching* are the same in kind as those caused by the loss of blood from other means. A moderate quantity may be abstracted without any obvious effects on any of the functions; but if the amount taken be increased, the well-known "syncope" is the result. The quantity necessary to produce this varies, however, considerably, and will depend on the mode of drawing it, (whether rapidly, or otherwise); the position, constitution, and age of the patient; the nature of the disease; and many other circumstances, not necessary for me to enumerate. It is well known, that a small quantity will, if taken rapidly, and the patient be in the erect posture, cause this effect; whereas a considerably larger amount may be abstracted, if taken gradually, and the patient in the recumbent position, without giving rise to it. The usual explanation of this is, that when the blood is drawn faster than the vessels can contract, the circulation is temporarily stopped, and fainting ensues. Several reasons, however, lead me to doubt the sufficiency of this explanation. Leeching, then, as being a slower mode of abstracting blood, is less likely to cause syncope than venesection, or even cupping.

But there is another immediate effect of loss of blood, which may either accompany syncope, or occur independently of it;—I mean disorder of the cerebral functions, as marked by convulsions, delirium, or coma. On this subject you will find some interesting remarks in the work of my able colleague, Dr. Marshall Hall, on the "Morbid and Curative Effects of Loss of Blood." I may observe, that convulsive movements are by no means uncommon in syncope from general blood-letting, and I think are not always to be considered as denoting that the remedy has been used beyond the safe degree. I have on several occasions been told by patients about to lose blood that they are apt to faint and struggle when bled, and I have in consequence been requested to prevent them from injuring themselves. Delirium and coma are less frequently met with. In two or three instances I have seen delirium occur after blood-letting, but have not been satisfied whether it was the effect of the disease or of the remedy: coma I have never seen result from bleeding. Both symptoms must be regarded as serious and dangerous when they do occur. There is, lastly, another effect of loss of blood, namely, great depression of the vascular system, followed by sudden dissolution. A case of this kind is mentioned in the 11th volume of the Lancet, page 94, as having occurred in St. Bartholomew's Hospital.

It is necessary that I should notice some other consequences of blood-letting;—I mean the immediate effects this agent has over the disease, and which, in contradistinction to those before mentioned, we may denominate therapeutic. As might be expected, an operation so powerfully affecting the vital functions cannot be passive in its influence over morbid action, but the phenomena vary so much in different diseases, and even in the same disease under different circumstances, that it becomes extremely difficult to offer any general results. That loss of blood is sometimes beneficial, at other times hurtful, is well known. Of its immediate beneficial effects, I may offer one or two illustrations. In inflammation of the lungs, I have repeatedly heard patients declare that the respiration became easier, and the pain removed, while the blood was flowing; and in some cases from this time amendment took place. In ophthalmia, the redness of the conjunctiva disappears during the syncope from blood-letting, and sometimes never returns with equal intensity.

Having now noticed the most important effects of blood-letting, I may here remark, that they are considerably influenced by disease. You will recollect in a previous lecture I alluded to the Italian doctrine of contra-stimulus, and mentioned it was supposed by the followers of this theory, that, during a state of excitement, the system was capable of bearing or tolerating a much larger dose of certain medicines termed contra-stimulants, than could be endured in the natural or healthy condition. Now precisely the same kind of doctrine is held by Dr. Hall with respect to the relative effects of blood-letting in health and disease. Every practitioner is acquainted with the fact, that, in certain morbid conditions, patients bear the loss of larger quantities of blood than in others. I need only mention apoplexy, inflammation of the serous membranes, peripneumony, and phrenitis, as examples of increased tolerance; while chlorosis and cholera may be cited as instances of diminished tolerance. On this point there cannot be, I think, two opinions.

I confess I am not prepared to assent to the inferences Dr. Hall has drawn from these facts, nor to the rules he has laid down in the diagnosis and treatment of disease founded on the circumstances just mentioned. The susceptibility to syncope is so great in some persons, that we should, I suspect, be often led into error, if we

were to infer the absence of inflammation merely from the occurrence of fainting after the loss of a few ounces of blood. Besides, it not unfrequently happens that a patient faints on the first, but not on the second or third bleeding. I have more than once seen this; and my friend Dr. Clutterbuck, whose extensive experience in the use of this remedy is well known, and who I am happy to find is about to give the profession the results of his practice, assures me he has frequently observed it. Neither do I think it would always be safe to bleed *ad deliquium*, even if we are satisfied that inflammation be present; for in some it is difficult to occasion syncope, although the quantity of blood lost be so great as to endanger the safety of the patient. The practice of Dr. Hall, however, is much to be preferred in this respect to that of Mr. Wardrop; for although both recommend bleeding to syncope in inflammation, the former places his patient in the erect, the latter in the recumbent posture. And here I cannot help remarking that the practice of ordering patients to be bled to syncope in the recumbent posture appears to me a dangerous one. That fainting will sometimes occur in the erect position, before a sufficient quantity of blood has been drawn, we all know; and to prevent this occurrence, it is frequently proper to bleed in the recumbent posture: but I must protest against bleeding patients to *syncope* in this position.

I have yet to notice another class of the general effects of loss of blood, which may be denominated secondary or remote, and which are in no way useful in the treatment of disease. In some cases excessive reaction occurs, attended with throbbing of the vessels of the brain, pain and disorder of the cerebral functions. You will see examples of this in women who have suffered severely from uterine hæmorrhage. The two children killed by leeching, already alluded to, were examples of death caused by exhaustion from loss of blood with insufficient reaction. Other secondary or remote effects of blood-letting are mentioned: they consist principally in disorder of the sensorial functions, marked by delirium, coma, or even amaurosis. For cases I must refer to the works of Drs. Abercrombie and Hall, and Mr. Travers.

Having hitherto spoken of the consequences of bleeding generally, I must now refer more particularly to leeching. The constitutional or general effects caused by the application of leeches are best observed in children and delicate females—more especially the former. I have, on several occasions, seen infants completely blanched by the application of one or two leeches Pelletan mentions the case of a child, six years old, who died from the hæmorrhage occasioned by six leeches applied to the chest. Leeching, then, is here, to all intents and purposes, a mode of general blood-letting, arising in part from the powerful influence which a small quantity of blood produces in infants; and secondly, because one leech will cause the loss of more blood in them than in adults, owing to the greater vascularity of the cutaneous system. It is apparent, therefore, that in the diseases of children leeching may, in most cases, be substituted for venesection. But in disorders which are rapidly fatal, as croup, opening the jugular vein is undoubtedly to be preferred, since it is necessary to produce an immediate and powerful effect. As children advance in years they become capable of bearing larger evacuations of blood; and, therefore, leeching excites a less influential effect. It is quite impossible to say at what age venesection ought to be substituted, or, in infancy, what number of leeches should be applied; since they take away such unequal quantities of blood. These are points that must be decided by the practitioner in each case. Here is a tabular statement of the amount of blood which Dr. James Blundell has taken from children at different ages.

Ages.	Quantities
2 months	1 oz. to $1\frac{1}{3}$ oz.
4 months	$1\frac{1}{2}$ oz. to 2 oz.
8 months	2 oz. to 3 oz.
12 months	3 oz. to 4 oz.
18 months	4 oz. to 5 oz.
3 years	8 oz. to 10 oz.
6 years	10 oz. to 12 oz.

But the quantities are exceedingly large, and in most instances greater than you will find it prudent to abstract. Guersent, physician to the Hôpital des Enfans, at Paris, says, that in infants up to two years of age, we ought never to draw more than three or four ounces of blood in twenty-four hours.

2. *The local effects of leeching* must now be noticed. The jaws of the leech may be compared to three saws, each armed with sixty teeth. You will, therefore, not be surprised at the pain and afflux of blood occasioned by the laceration of the skin by a single leech. I have sometimes seen one of these animals produce intense redness to the extent of an inch around the bite. You will best observe this when the skin is delicate, as that covering the mammæ of the female. When a number of these animals are applied, their united local effects must have some influence over a neighbouring disease.

There are also certain topical effects which occur subsequently—such as ecchymosis,—the irritation and inflammation of the lips of the punctures,—the diffused redness and the soreness in the parts intervening between the bites; which cannot be without influence over morbid action. They act on the principle of counter-irritation. In taking into consideration the beneficial influence of leeches, we must, therefore, not forget these, nor the fomentations and poultices subsequently employed.

When leeches are applied to the temples, especially if they fix close to the external canthus, a diffused swelling frequently arises, similar to that caused by erysipelas. This is not referrible to any noxious qualities of the animal, for it happens when the finest and most healthy are employed; nor to the teeth of the animal being left within the wound, since I have frequently seen it when the leech has fallen off spontaneously.

In concluding these remarks on the local effects of leeches, I have only to add, that independently of the local irritation caused by the puncture, I believe the evacuation of blood from an inflamed part may be more beneficial than the same quantity taken by the usual operation of venesection. In other words, I am disposed to admit what were formerly termed the *derivative* effects of local bleeding. The amount of benefit obtained by the application of leeches to parts that have been injured by falls, &c. as in fractures and dislocations, has frequently appeared to me much greater than could be referrred to the combined influence of the quantum of blood lost, and the local irritation of the punctures; so, also, with respect to the good effects of leeching hæmorrhoidal tumors. Mr. Wardrop thinks, more benefit is in some cases obtained by the application of leeches at a distance from the affected organ, constituting what has been termed a *revulsive* operation.

I trust the remarks now offered will be sufficient to prove, that in estimating the therapeutic influence of leeches, the quantity of blood drawn is not the only element in the calculation; and I think, in practice, you will find constant proof that leeching is more beneficial than can be accounted for by the mere quantity of blood drawn.

On the *uses* of leeches I must necessarily be very brief, since to enter properly into the subject would involve pathological details unsuited to pharmacological lectures.

1. In children and delicate adults, as females and aged persons, leeches will be found an exceedingly useful substitute for general blood-letting, where the object is not to occasion any immediate or sudden effect on the disease. In children, avoid applying them to the neck, or other parts where compression cannot be conveniently made.

2. In local determinations of blood, unattended with febrile symptoms, local blood-letting, when it can be resorted to, is generally, though not invariably, preferable to phlebotomy. The advantages of leeching over cupping are, the less pain, and the ease with which blood may be procured; for it is evident that in swelled testicle, in inflammation attending fractured limbs, and in acute inflammation of the mammary gland, patients could not, in most cases, bear the necessary pressure of the cupping glass; and in some parts of the body, as the abdomen, blood can only be procured from cupping by a very dexterous manipulation.

3. In internal and other inflammatory affections, accompanied with constitutional disorder, the rule is to employ general in preference to local blood-letting. But circumstances occasionally render the reverse practice justifiable and proper, as where the disease is not active, and the patient delicate and weak. In many instances you will find it most advantageous to combine both modes of drawing blood: for example, in abdominal inflammations, the application of leeches, preceded by venesection, will sometimes do more good than the same quantity taken by the lancet alone.

4. There are some diseases in which no substitute of equal efficacy can be found for leeches. Such, I conceive, are hæmorroidal tumors, and prolapsus of the rectum. In these cases general is not equal to local blood-letting, and cupping is out of the question.

5. In various organic diseases leeches will often be found an exceedingly useful palliative means. I would particularly mention as examples, affections of the heart and lungs.

There are few diseases in which loss of blood is required, where leeching is positively objectionable; indeed, erysipelas is almost the only one that can be named. Here it has been supposed that the local irritation caused by leeches would add to the severity of the malady; but I believe that even in this case, the objections are more imaginary than real. There are, however, numerous instances in which leeching is negatively objectionable: in some the quantity of blood drawn by these animals is insufficient to make much impression on the disease, as in visceral inflammation of robust persons; in others, where the disease is very rapid and fatal, the effects of leeches are too slow, as in

croup. Venesection is the remedy in all these instances. Bear in mind, however, that the weakening effect is always proportionate to the quantity of blood drawn.

I proceed to examine those animals used in medicine which belong to the

CLASS INSECTA.

The bodies of insects are frequently marked by deep insections, from which the class has received its name; but you will bear in mind that the other divisions of articulata also possess this character.

Definition.—Insects are articulated animals, having three pairs of articulated members, breathing by tracheæ, provided with antennæ, and passing through several transformations and changes, denominated their metamorphoses. Essentially these animals are oviparous, but in some instances the egg is retained in the body of the mother until it is hatched (constituting the animals formerly termed *Insecta ovo-vivipara*), or until it is transformed into the pupa state.

Metamorphoses.—In insects there are four distinct stages of existence, namely, the egg, the larva, the pupa, and the imago.

1. The *egg*, or *ovum*, varies in its shape.

2. The *larva* is the young insect produced by the egg. In those insects which undergo what are called *imperfect metamorphoses*, the larva has the form of the parent, but is destitute of wings. In most cases, however, the larva (although containing within it the rudiments of the future insect) has an exterior form very different from that of its parent: these are the animals that undergo the *complete metamorphoses*. The larvæ of the latter possess the same general structure as the Annelides; sometimes having neither distinct head nor feet, when they are called *maggots*; sometimes possessing a distinct head and six feet, constituting the *larvæ* commonly so termed; and in other cases, besides six horny, there are several membranous legs; these are termed *caterpillars*, or *erucæ*.

3. The *pupa*, or third stage of development, is formed by the larva throwing off a number of membranous coverings. In this state the insect takes no food, and, generally, does not move. In those with incomplete metamorphoses, the pupæ are furnished with the rudiments of wings, and are called *nymphæ*. Those pupæ which have a gold colour are denominated *chrysalides*; though this term is by some applied to all pupæ, by others to the pupæ of butterflies.

4. The *imago*, or *perfect insect*, is the term applied to the animal when it quits its pupa case.

The nomenclature of the parts of the perfect insect will require a short notice. In all perfect insects we distinguish three divisions of the body of the animal, namely, the head, the thorax, and the abdomen.

1. The *head* (*caput*) has attached to it the parts of the mouth (*partes oris*), the antennæ, and the eyes. The mouth consists of an upper lip (*labrum*): a lower lip (*labium*), with its palpi (*palpi labiales*); a pair of upper jaws or mandibles (*mandibulæ*), without palpi; a pair of under jaws (*maxillæ*) provided with palpi (*palpi maxillares*). The *antennæ* are two articulated bodies, one of which is placed on each side of the head, between the angles of the mouth and the eyes. The eyes (*oculi*) are of two kinds, those called compound (*oculi compositi*) and those denominated simple (*oculi simplices, ocelli, stemmata*).

2. The *thorax* consists of three segments (each perforated by the air-hole, called *stigma* or *spiracula*), the anterior called *prothorax*, the middle termed *mesothorax*, the posterior denominated *metathorax*. The prothorax supports the first pair of legs; the mesothorax supports the second pair of legs and the first pair of wings; while the metathorax sustains the third pair of legs and the second pair of wings. The part of the legs are named after those of vertebrate animals, and thus we have the following joints or pieces,—the *coxa*, the *trochanter*, the *femur*, the *tibia*, and the *tarsus* (the latter made up of from one to five joints). The first or anterior pair of wings, when horny, have been termed wing-cases, or *elytra*.

3. The *abdomen* is made up of a varying number of segments, each perforated by the air-hole (*stigma* seu *spiracula*).

Orders of insecta.—Latreille, in Cuvier's Règne Animale, admits twelve orders of insects. One of these,—the myriapods, is, by almost universal consent, now made a distinct class, since the animals of this division differ from those properly called insects both by their external and internal structure. Three orders are described by him as being wingless (*aptera*), namely, *thysanoura, parasita*, and *suctoria*. But as almost all the orders contain apterous insects, it would be evidently better to take our divisions from some other character than the presence, absence, or number of the wings. Now, it is generally admitted that we derive much more constant characters from the metamorphoses and the structure of the mouths. Dr. Hermann Burmeister has, on these principles, founded a classification which appears to be much more correct. Here is a sketch of his arrangement; and for further details I must refer you to his valuable work, "*A Manual of Entomology*," now being translated by Mr. Shuckard.

		Orders.	*Examples.*
Metamorphosis imperfect.	Mouth suctorial	1. Hemiptera	Bugs, Cochineal, &c.
	Mouth masticating	2. Orthoptera	Locusts, Grasshoppers, &c.
		3. Dictyoptera	Cockroaches.
Metamorphosis perfect.	Mouth suctorial	4. Diptera	Flies.
		5. Lepidoptera	Butterflies, Moths.
	Mouth masticating	6. Neuroptera	Dragonflies.
		7. Hymenoptera	Bees, Wasps, &c.
		8. Coleoptera	Cantharides, &c.

On this arrangement I have to remark, that by an imperfect metamorphosis is meant, the strong resemblance of the larva, pupa, and imago, to each other, the pupa possessing locomotion, and eating; whereas in those insects with a perfect metamorphosis the larva is a long maggot or caterpillar, or wornil, the pupa being generally quiescent, and not eating.

In my next lecture I purpose examining the coleopterous insects employed in medicine.

LECTURES

ON

SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

BY DR. LATHAM.

ON SYMPTOMS.

Subject continued—Mixed Phthisis—The mixed character of its Auscultatory Signs.

I HAVE described two forms of Pulmonary Consumption, distinct from each other in certain pathological particulars, and distinct also in their Auscultatory Signs. Both were specimens of genuine and unmixed Phthisis;—genuine, because they consisted of tubercles and vomicæ, which are the essence of the disease; unmixed, because they consisted of tubercles and vomicæ *only* from first to last; these fulfilling their specific morbid processes within their specific limits, and leaving all other parts of the lungs which they do not actually occupy free from disease.

These two forms of Phthisis were represented as chronic; and chronic they are generally found to be: for disease which does not easily impart irritation to surrounding structures, is usually slow in its own actions.

Observe, I am not making distinctions of Phthisical disease for purposes of mere arrangement and nosology; but I am selecting such forms of it as I find suitable to the purpose I have in hand; that purpose being to show that there is such a disease as genuine and unmixed Phthisis, discoverable by Auscultation, and distinguishable by Auscultation from *mixed* Phthisis, which is presently to be considered.

There are forms, then, of Pulmonary Consumption, which Auscultation is able to distinguish from those already described mainly in this particular circumstance, viz. that the tubercles and vomicæ produce and spread abroad other disease within the lungs beyond the parts which they themselves occupy, and beyond the sphere of their own specific actions; and that other disease is Inflammation. These forms are characterized during life by a mixture of the Auscultatory Sounds belonging to Phthisis, and the Auscultatory Sounds belonging to that other disease; by those which denote tubercles and cavities, and those which denote effusion, of whatever kind, into the bronchial and vesicular structure of the lungs; by dulness beneath the clavicles or about the scapulæ, or by Cavernous Breathing or Bronchophony, Gurgling Breathing, Gurgling Cough or Pectoriloquy; and, at any or every part, where these are not found, by Sibilus or large or small Crepitation.

The Phthisical disease, however, must have reached a certain point of its development, before each order of Auscultatory Sounds is clearly discernible.

Hence there are cases in which the disease indeed is mixed, or of two kinds concurrently, while the Auscultatory Sounds are of one kind only. The specific Phthisical disease and the common Inflammatory disease exist together; but at the same time the Auscultatory Sounds present only indicate the one and not the other: and, strange to say, the sounds that *are* present often belong, not to the *primary* Phthisical disease, but to the Inflammation which is secondary and incidental to it.

Mere tubercles, at their first formation, are capable of imparting such irritation to the whole lung, as to produce inflammation of its entire bronchial and vesicular structure, and an early and an abiding effusion within it; and this effusion gives occasion to its own Auscultatory Signs. But the tubercles themselves may be so scattered through the lungs, and so little

accumulated in any one situation, as of themselves to cause no perceptible obstruction to the passage of air, and consequently to give no Auscultatory Signs by which they could be suspected to exist, even if there were no disease beside themselves.

In the autumn of last year (1833) a young man (Thomas King) was admitted into this hospital, and remained here three months, until he died. His case is so important pathologically, and so aptly illustrative of those practical points I am now considering, that I must be allowed to dwell upon it a little at large.

He had already been ill three months before his admission. His complaint began with Hæmoptysis, which occurred to the amount of four or five ounces when he was making some unusual exertion, and continued in smaller quantities for a few days, and then ceased altogether.

At his admission he was pale and emaciated; his pulse was 140 in a minute, and of extreme feebleness. He had profuse perspirations at night; he suffered an agonizing Dyspnœa, and brought up a scanty glutinous expectoration, with great effort of coughing.

Auscultation discovered at this time a clear respiratory murmur in every part of the left lung, and in the right a clear respiratory murmur, confined to its *very upper* part, and then a widely-diffused small Crepitation, which gradually faded away into absolute dulness as you approached nearer the bottom. It was not long, however, before the condition of the left lung was the same to Auscultation as the right.

I know no distress greater than that which attends the collection of viscid mucus in the lesser bronchi and vesicular structure of the lungs. There is a constant dyspnœa and ceaseless provocation to cough, and an agony, and striving to tear up from the respiratory passages something that will not come. Such distress was unmitigated in this poor fellow for three months together.

This same condition of the lesser bronchi and vesicular structure, when it results from acute inflammation, seldom lasts long; yet even for a few days the agony of the Dyspnœa and fatigue of the Cough are hardly tolerable. But *here* they were abiding and unaltered for three months, kept up by inflammation (if you please), but inflammation of no action and no power, where the pulse was always of extreme frequency and extreme feebleness.

No remedies that were employed at all assuaged the distress or altered the conditions of disease within the chest; and the Auscultatory Sign to the last, the only sign, was small Crepitation largely diffused through both lungs, which, at their lower part, were almost dull. He died exhausted.

Now, long before his death, I certainly did suspect that this effusion throughout all the Bronchial passages was not kept up solely by the idiopathic disorder of their lining membrane, but that something would be found elsewhere within the lungs capable of producing and maintaining it; and I certainly did conjecture that we should find tubercles, partly from the whole malady having commenced with Hæmoptysis, and partly from not knowing what else there could be.

Dissection discovered the pleura adherent on both sides,—on the left partially, on the right universally; and the whole membrane converted into a thick cartilaginous substance; the entire lungs loaded with bloody serum, and their whole texture so softened as to break down easily under pressure of the fingers. These were all the results of common inflammation, which had reached to every tissue which composes the structure of the lungs.

But dissection discovered, moreover, myriads of tubercles, distinct, and sprinkled universally throughout the lungs; grey, and as minute as millet-seeds, in the lower lobes, yellow and of a larger size in the upper.

This is not a common specimen of disease, but it is a very instructive one, and *therefore* I have quoted it. It is remarkable, in contrast with the forms of Pulmonary Consumption which I have before described, that *here* the minutest tubercles, not one of which had passed on to the state of vomica, should be capable of producing inflammation, and diffusing it so generally, and maintaining it so constantly; and that these mere tubercles could continually supply such an amount of irritation that no remedy should in the least degree abate the inflammation (otherwise perhaps curable in itself) which they were always present and ready to renew. In this case the secondary and incidental morbid processes entirely outran those which were primary and specific; and the patient perished of inflammation of the entire lungs, the Phthisical disease having not passed beyond its earliest and tubercular stage.

But, trusting to my own experience, I should say, that it was a rare thing for Pulmonary Consumption thus to kill, by producing *chronic* inflammation and *chronic* effusion into the entire bronchial and vesicular structure of the lungs, itself still remaining in the tubercular stage, and the tubercles not yet occasioning any auscultatory impediment to the passage of air. It is more according to my observation that, *in this stage*, or under these conditions, it should produce *acute* inflammation,

either proving fatal in a few days, or requiring active treatment to prevent it from becoming so. And it is still more according to my observation, that in this stage, or under these conditions, it should produce Hæmoptysis; which also may be called acute, being accompanied by fever and much vascular action, and liable, too, to prove fatal in a few days, or requiring active treatment to prevent it from becoming so. In the fatal cases the unsuspected tubercles are only discovered after death.

Yet attacks of acute Inflammation or acute Hæmoptysis from such a cause are not usually fatal: but, the cause still remaining, they are apt to recur again and again; and at length, when the tubercles increase sufficiently to obstruct the passage of air through certain parts, and to occasion dulness here and there, and a more energetic and compensating respiration elsewhere, then the secret of the former inflammations or hæmorrhages are revealed. At this point the mixed characters of the disease begin to declare themselves by the mixed characters of the Auscultatory Signs.

Now, although Pulmonary Consumption (as we have seen) unquestionably can, and sometimes does, produce Inflammation or Hæmorrhage of the Respiratory Passages, long before it is so far developed as to give any direct tokens of its own existence, yet this is not the usual course: it most frequently happens, that neither Inflammation nor Hæmorrhage are added to it, until tubercles have at least reached that degree of accumulation when they begin to give occasion to certain Auscultatory Signs of their own.

I think I have observed that, as long as the Pulmonary Consumption remains in its tubercular stage, if an Inflammation or a Hæmorrhage be added to it, they are apt to occur in distinct attacks, occasionally and casually.

I formerly mentioned the frequent cases of Hæmoptysis admitted into this hospital, which were connected with tubercles of the lungs. The attack is usually sudden; the quantity of blood lost in a short time considerable; the treatment required usually active; and the result, as far as the mere hæmorrhage is concerned, usually successful. Moreover, the Auscultatory Signs denote the mixed nature of the disease. While the spitting of blood continues, and perhaps for a short time after it has ceased, there is a large or small Crepitation commonly arising from a considerable space at the lower part of one or both lungs. This denotes the bronchial or vesicular effusion, as distinguished from the deposition of tubercles. Then there is a diffused dulness both to Percussion and Auscultation somewhere, perhaps between the clavicle and mamma on one side, and an exaggerated respiratory murmur somewhere else, perhaps between the clavicle and mamma on the other side. These denote the deposition of tubercles, as distinguished from the bronchial or vesicular effusion.

To my experience bronchial or vesicular hæmorrhage is more familiar as an accompaniment of Phthisis, as long as it continues in its tubercular stage, than bronchial or vesicular inflammation; the effusion of blood than the effusion of serum or mucus. But when Inflammation *does* occur, I have generally remarked in it the same circumstances and attendant conditions which belong to the Hæmorrhage, the same sudden and distinct mode of attack, and that degree of excitement of the blood-vessels which requires the same treatment, and the same successful result. Moreover, there have been the same Auscultatory Signs, namely, crepitation at the lower part of the lungs, produced by the effusion of serum or mucus; and dulness at the upper part, produced by the deposition of tubercles. The only difference is, that in one case serum or mucus is expectorated, and in the other blood.

I here speak of Hæmorrhage and of Inflammation as of two things, not wishing to swerve from the customary language of medical men; but, if I might use my own language, I should speak of *vascular action terminating sometimes in effusion of blood*, and *sometimes of serum or mucus*, as of one thing tending to two results; for whether the result be the effusion of blood, or the effusion of serum or mucus, the vascular action requires the same treatment according to its degree, and is as much inflammatory in one case as the other. Moreover, it is this vascular action which is our only practical concern; we treat it, and *it only*, and have no special care of either blood, serum, or mucus, for which in themselves we can do nothing.

But it is when Pulmonary Consumption has advanced beyond the tubercular stage that we find the most frequent examples of its mixed character. Bronchial or vesicular effusion is almost the constant accompaniment of vomicæ; and the expectoration is now often as much supplied by the mucous lining of the air passages as by the cavities themselves.

You have only to go into the wards of the hospital, and you may at once acquaint yourselves in a dozen instances with the mixed character of the auscultatory sounds. Gurgling cough, gurgling and cavernous respiration, pectoriloquy, one, or several, or all together, will shew that this, that, and the other patient, have Vomicæ in their lungs; and large and small Crepitation, one or both concurrently, will shew

also that this, that, and the other patient, have fluid effused here, there, or every where, within the respiratory passages.

Now when vomicæ have been long formed, and the expectoration long established, Hæmorrhage and Inflammation are less liable to occur in sudden and distinct attacks. The blood, or mucus, or serum, which are now separated from the surface of the air passages, result from a vascular action of less force, but of more permanency, and are themselves more abiding.

It should be remarked, however, that blood, which is more common in another stage of Pulmonary Consumption, is more rare in this; not that blood does not *now* sometimes appear, but it appears rather as a part of the expectorated matter, and streaking or staining it, than as pure and sincere blood.

Assuredly, after the expectoration is established, sudden and profuse gushes of blood seldom occur. Probably the expectoration itself is the security against them, the circulation thus obtaining all the relief it stands in need of. Probably, too, the security becomes greater in proportion as the expectoration is more copious and more free, and proceeds from a larger extent of mucous surface.

All this is, in the nature of things, very probable, and it is confirmed to me by the striking fact which, in a few instances, I have known, of a copious muco-purulent expectoration suddenly ceasing, and a frightful Hæmoptysis at once bursting forth; as if the circulation, being suddenly baffled, had sought and found the nearest way to free itself. In these instances, when the hæmorrhage ceased, the expectoration was re-established.

It should be mentioned, that in the destructive processes connected with the formation of many and large Vomicæ, the blood-vessels of the lungs do not always escape ulceration, or rupture, while they are yet pervious; and then a mortal hæmorrhage is the consequence. But such hæmorrhage is purely accidental, and independent of any proper hæmorrhagic action (if I may so call it) in the vessels themselves.

Let me guard you against a vulgar error. Hæmoptysis and rupture of a blood-vessel are, in the popular sense, convertible terms; so much is one conceived to be the natural and necessary consequence of the other. But rupture of a blood-vessel, which has been esteemed the only cause of Hæmoptysis, is unquestionably the rarest cause of all; and this accident, which one might expect to find frequent in Pulmonary Consumption, nature has taken great pains to guard against; for no sooner does the destructive process of forming vomicæ within the lungs begin, than she sedulously betakes herself to closing up the arteries which lead to them by clots of blood: and as to the veins, partly (I believe) by the same process, and by otherwise arresting the circulation through through them, she reduces them to impervious shreds.

Now in all cases of Pulmonary Consumption arrived at the stage of vomicæ, I would recommend a constant regard to the extent of the disease beyond its specific limits. I would recommend that, besides attending to the sounds indicative of cavities, you should take especial note of *Crepitations*, and how they vary in the distance to which they spread themselves from time to time. The Gargouillement, and the Pectoriloquy, and the Vomicæ, from which they arise, are beyond our reach *remedially*; not so the crepitations, and the vascular action which produces them. In my treatment of Pulmonary Consumption, I am accustomed to make these crepitations serve me for practical indications, endeavouring by all means to lessen and circumscribe them, and thus seeking, under the guidance of Auscultation, to bring back the disease as much as possible within its specific limits.

The bronchial and vesicular effusion, which is the concomitant of vomicæ, submits itself to the influence of medicine in various degrees. Very often, when there are gurgling cough and gurgling cavernous respiration and pectoriloquy, at certain points, and, withal, large and small crepitations diffused widely through the lungs, a seasonable remedy will entirely sweep away the latter sounds, and leave the former *alone*. A small cupping, a few leeches, a blister, a liniment, a mustard cataplasm—one or other of these, according to the degree of vascular action, applied at the right time and in the right place, will produce immense relief, in this manner bringing the disease for a while within its specific limits.

It is thus, as perhaps you may have remarked, that almost every phthisical patient brought into the hospital experiences great relief for a short time after his admission. The poor, alas! are not only the chief victims of Phthisis, but they suffer the disease with all its occasional superadded evils, which their exposure, their hardships, and their needful toils, will not allow them to escape. With them, the superadded evils are often beyond all proportion to the disease itself. The tubercles and vomicæ may be few, and the bronchial and vesicular effusion immense; and this superadded effusion may be for the first time submitted to a remedy when they reach the hospital, and then it is often in a great part or altogether swept away. No

wonder that, from the relief which follows, the patients should sometimes believe themselves cured at once and entirely.

But the effusion again and again returns, and requires again and again to be abated.

Thus I have given a sketch only of a vast subject, which is interesting, fearfully interesting, to all mankind. I have not crowded it with a multitude of instances, for I had not room for them; and I was afraid they might obscure the outline, which was all I designed to give.

In tracing this outline my own experience has guided me; and yours will soon enable you to fill it up, and to determine how far it is true and how much it is worth.

There are many strange things respecting Pulmonary Consumption — many striking discrepancies between case and case, and many contrarieties of opinion among the well-informed as to its proper mode of treatment, which, heretofore, the best of us have been unable to reconcile or explain. In one case it is a slow decline; in another a gallopping consumption. It is spoken of as incurable; and yet now and then an individual recovers, or seems to recover. Some are for treating it by bleeding. Some by bark and steel, and ammonia. Some restrict their patients to vegetables and asses' milk, and some give them animal food and wine; and all can boast of their success.

But these things have now ceased to be mysterious any longer; thanks to a more enlightened pathology and to that method of diagnosis which not only marks the due course and progress of the specific disease, but does not allow even emergent and contingent events to escape its scrutiny.

This same method, still aided and guided by a just pathology, has enabled us to discern more clearly the avenues through which nature admits relief, and to direct our remedies with a steadier aim, albeit such remedies are of various and even opposite kinds.

Unquestionably, within the limits of *possible* success, Auscultation has contributed to render our treatment of Phthisis more successful.

It has been the means of discovering no new remedy. How should it? It has made nothing curable now, which was incurable before. But, while it has taught us to distinguish the unmixed from the mixed Phthisis, the disease from its accidents, what is reparable from what is not *in the patient while yet alive*, it has enlarged the resources of practical medicine, by more clearly presenting to it the objects within its reach.

By keeping those objects, thus offered to you, steadily in view, you will be able to fulfil more satisfactorily every purpose that rational treatment can contemplate in a disease which, if it be not incurable, is rarely cured. You will know better how to remove its many superadded evils, to postpone or mitigate its many pains, perhaps to lengthen life, and to procure for it, while it lasts, some share of happiness.

I cannot finish this little sketch of Pulmonary Consumption, and of the uses which Auscultation serves in distinguishing its kinds, and in furnishing guidance for its treatment, without adding one further remark, and it is *this*:—In Pulmonary Consumption death not unfrequently arrives at a much earlier period than seems consistent with the morbid processes going on within the chest. Patients die sometimes long before the disease of the lungs has reached the point at which it is necessarily fatal. I do not mean that phthisical patients may chance to suffer Fever or Cholera, or may chance to break a limb, and so die of other diseases or accidents. These cannot be anticipated or guarded against. But I mean something more worthy of our consideration.

In proportion as we are more intent upon investigating the local processes of diseases in a particular organ, scrutinizing them pathologically, and nicely weighing their Diagnostic Signs, there is a danger that our minds may be withdrawn from those larger views which regard their constitutional origin, and their consequent liability to fall upon any or all organs of the body. Thus, while we are expecting a man to die of Pulmonary Consumption a twelvemonth hence, he may die in the meantime of (if I may so speak) Intestinal Consumption, Peritoneal Consumption, Mesenteric Consumption, or Vertebral Consumption. For, from the same strumous constitution, which engendered tubercles and vomicæ in the lungs, tubercles and ulcers are formed in the mucous lining of the bowels; hence comes an incontrollable Diarrhœa or Dysentery. From the same cause tubercles are formed upon the peritoneum, and tubercles and matter in the mesenteric glands; hence come Slow Peritonitis, and Ascites, and Marasmus. From the same cause Caries affects the bodies of the vertebræ; hence comes Lumbar Abscess.

All these are liable to arise in the course of Pulmonary Consumption; and the fatal event may be greatly accelerated in consequence. Not that, from arising in the course of it, they are to be regarded as merely incidental to it; for both it and they are all of the same essence, the several products of the same morbid constitution; sometimes one and sometimes another taking the lead; Pulmonary Consumption as often following these, as these following it.

Finally, then, Pulmonary Consumption is no more than a *fragment* of a great constitutional malady, which it would be in vain to think of measuring by the stethoscope, and which it belongs to a higher discipline than any mere skill of Auscultation rightly to comprehend.

ANOMALOUS CONDITION
OF THE
LARGE INTESTINE, AND OTHER VISCERA, IN A CHILD.

To the Editor of the Medical Gazette.

Sir,

A case of irregular development of the large intestine lately fell under my observation, which may not be unsuitable for the pages of your journal. It occurred in a little boy fifteen months old, who died after an attack of pneumonia, following pertussis.

On opening the abdomen no omentum was to be seen, nor any portion of intestine corresponding in situation to the transverse arch of the colon. The distended small intestine appeared to occupy the whole cavity, excepting that a portion of bowel, larger than the rest, was observed running across the hypogastrium.

From its size and direction I was led to conclude that it might be the transverse arch of the colon misplaced; but it had no longitudinal bands, and there was no omentum attached to it. On drawing it aside, it was found connected with the spine by a mesentery. The natural conclusion was, that it must be a portion of small intestine, unusually inflated, and deviating from its ordinary course; but, on tracing it on the right side, it was found to turn down over the sacrum, and to terminate in the rectum. On following its course to the left side, it proved to be continuous with the sigmoid flexure of the colon. On drawing the small intestines entirely downwards, the remainder of the colon was discovered; the transverse arch lying at the back part of the abdomen, flaccid and empty, with a small omentum attached to it. The deviation from the ordinary arrangement was thus found to consist in a secondary or inferior transverse arch of the colon, running from left to right, presenting some of the characters of small intestine, and furnished with a mesentery.

This mesentery, in proportionate size and general character, exactly resembled that of the small intestine, excepting that some of the glands lay upon the surface of the bowel, instead of being situated at a short distance from it. There was nothing remarkable in the contents of the bowels, but their coats were extremely thin.

Some portions of the lungs were condensed by inflammation, and these organs were extensively emphysematous; but the amount of morbid change was scarcely sufficient to account for the death of the patient. Several days before death, dyspnœa and the other symptoms of pneumonia had subsided, and the child appeared to die rather from failure of the vital powers than from positive disease.

Several of the appearances were characteristic of imperfect development; namely, a very thin calvaria, defective ossification of the anterior fontanelle, and an unusually large foramen cæcum, anterior to the crista galli; emphysema in the lungs, and atrophy in the intestines. The child had been very subject to diarrhœa; but during the last month the bowels were inactive, and required the frequent use of turpentine enemas. This was the sixth child, in a family of eight, which had died after a similar series of symptoms; but the bodies of the other children were not examined.

This case might naturally lead to a consideration of some of the theories which have been advanced on the subject of congenital malformation; and also of the speculations of Schultz on the function of the cæcum; but it is not my intention to enter at present upon these inquiries. I trouble you with an account of the case, because I am not aware that a similar variety has been distinctly recorded.

A new fact is always interesting; and although it may not, at first, seem capable of any very practical application, it is not unreasonable to suppose that, when compared with analogous facts, and associated with future observations, it may prove useful and important.

I am, sir,
Your obedient servant,
Theoph. Thompson, M.D.

Keppel Street, Russell-Square,
Dec. 28, 1835.

P.S.—The preparation is in the possession of Dr. Carswell.

ON THE CAUSES OF PROLAPSE OF THE FUNIS IN LABOUR.

By John Roberton, Esq.
One of the Surgeons to the Manchester Lying-in Hospital.

The manner in which this distressing accident takes place, has never, so far as I know, been adequately explained.

Early in the ninth month of pregnancy, the uterus is still high in the abdomen. As the period of parturition approaches, it descends. In some women this process of descent begins earlier than in others. Some have mentioned to me its commencement, as being observable to themselves, ten or twelve days before labour. In others the descent takes place chiefly in the ten or twelve hours preceding labour, and becomes accelerated when the hour of labour, as manifested by the *show*, nearly approaches.

This gradual, slow, painless descent of the gravid uterus is fraught with important uses. It is owing to this that the small end of the womb, containing the presenting part of the child, dips down into the pelvis, causing the head of the fœtus to present naturally (before the womb opens and the expulsive pains begin) in the position most favourable to its further descent, when the throes of labour shall actually commence.

Soon after the commencement of the labour pains, the head having been thus *located* in its natural position in the inlet of the pelvis, the os uteri opens, descends lower and lower, and the bulging part of the head, covered by the womb and the membranes and waters, comes to occupy and pretty completely fill the brim. If in this stage, during a pain, the waters escape, then the head of the fœtus instantly descends into contact with the lips of the womb, and closes the opening. So perfectly does the head occupy the os uteri from this stage forwards, that generally a portion of the waters is retained in the uterus, and passes off along with the child. Such is the mode by which nature shuts the mouth of the womb during labour, whereby its floating contents—viz. the funis and the remnant of waters—are kept from escaping into the vagina. It is true there is another cause which often helps to keep the funis from escaping before the end of the labour; I allude to its becoming twined round the neck, body, and limbs of the child. I will not undertake to say in what proportion of births the funis is discovered encircling once or more times the fœtal neck; but certainly this happens very often.

We are now prepared to understand how the prolapse of the funis may take place. The funis is a heavy rope; specifically heavier than the liquor amnii. Hence, when the waters escape, the funis, if nothing prevent—if the presenting part of the child do not occupy the inlet of the pelvis—sails into the vagina along with the current. If the patient is standing or sitting when the membranes rupture, this will increase the chance of the accident.

The next inquiry regards the circumstances which favour the descent of the funis: these will be far more impressively stated by cases than by mere description.

Case 1.—A patient of the hospital. Os uteri flabby, and not fully dilated. Six hours had elapsed since the escape of the waters; pains trifling; head presents high in the brim, having a number of twisted portions of the funis in advance of it, in contact with the scalp.

2. A patient of the hospital. Waters completely drained off; os uteri well dilated; no pains; a considerable coil of funis in the vagina, and the head resting at the brim.

3. A fold of the funis pulsated in the vagina. This passed down directly behind the pubis; between which and the head it was jammed. The bulging portion of the head occupied the inlet, and it seemed occupying its position firmly, for I could not move it upwards with my finger. The history I received of the accident was this: several hours before my visit, as the woman was standing by the fire, the waters passed off. On lying down immediately after, the midwife discovered the funis prolapsed.

4. Several hours previous to my visit, as she was kneeling by the bed, the waters escaped, when the midwife, on making her next examination, discovered the funis. The funis, I found, pulsated; it was very thick, and protruded a little beyond the os externum. There were no pains; the head lay at the brim.

5. Mrs. W., in the ninth month of pregnancy; so very large that her friends had the notion she carried twins. Early in the morning I found her having

slight labour pains, but made no examination. At breakfast-time the os uteri was not sensibly dilated. In about two hours after, as she was seated by the parlour-fire, a great discharge of water took place; not less than a gallon, according to the patient. In half an hour I arrived, and found the os uteri about half dilated, the head presenting, a hank of the funis in the vagina, and the labour going on.

6. Was called in the evening, by a professional friend, to a woman in Crown-lane. From midday there had been pains. At three in the afternoon she had been down stairs; and feeling a call of nature, as she was sitting in the act of obeying it, the waters suddenly escaped. The surgeon, on his arrival (several hours after this event), found the funis protruding from the vulva, and the head presenting at the brim.

7. Mrs. B. was taken in labour at two o'clock P.M.; pains trifling. At ten o'clock, as she lay in bed, the waters escaped in a very large gush. On examining immediately after, it was found by the attendant (I was not called till after many hours) that the funis was in the vagina, and that the head presented.

So much for descent of the funis in natural labour.

8. The waters had passed off when I paid my visit; a pulsating coil of the funis protruded from the os externum vaginæ. The shoulder presented. The funis, as I afterwards found, was three feet in length.

9. Evening. The patient had been in labour throughout the day; the hand of the fœtus was nearly at the os externum vaginæ, and the vagina was filled by a great many coils of the funis. The head was detected at the brim, but lying obliquely, resting on the top of the pubis.

10. The membranes presented a large bag, filling the brim, but no part of the fœtus could be detected. By and by a foot was felt through the membranes. These being ruptured, both feet and a large coil of the funis descended.

11. The waters had escaped ten hours before my arrival. The vertex rested at the brim, towards the right acetabulum. The right foot was down by the head, and a large coil of the funis pulsated in the vagina.

12. Found the feet in the vagina, and a doubling of the funis down between the thighs of the fœtus.

13. In this case the child had just been expelled, dead, when I arrived. It came footling, preceded by a portion of the funis.

Prolapse of Funis, occurring in Labours complicated with Pelvic Deformity.

14. Hand and funis in vagina; head also presenting naturally at the brim; promontory of sacrum encroaches on the antero-posterior diameter of the brim; rupture of the uterus; death.

15. Diminished antero-posterior diameter; funis in the vagina; head presenting naturally. Embryulcia.

16. Brim much contracted; head presenting; funis in the vagina. Embryulcia.

17. Antero-posterior diameter of the brim a little diminished, owing to a bony growth on the inner surface of the pubis at the symphysis; funis in the vagina; head forced by the pains into the cavity of the pelvis; rupture of uterus; death.

Concerning these seventeen cases, it is worthy of remark, that *in none did the presenting portion of the fœtus*, at the period when the nature of the presentation was detected, *occupy the brim in the ordinary manner:* either the head lay *at*, but not *in* the brim, there being no pelvic deformity, or the head could not enter the pelvis, owing to more or less deformity; or the presentation was preternatural (the feet, the hand, or the shoulder), and consequently unfitted, in reference to its bulk and figure, for filling the brim.

Again, it is further worthy of remark that, in addition to the above causes facilitating the premature escape of the funis from the uterus, in at least five of the cases I have related, the membranes ruptured while the patient was in a sitting or standing posture.

Of twenty labours complicated with prolapse of the funis, which have come under my notice within the last five years, in ten the head of the fœtus presented in a well-formed pelvis; in four the head presented, the inlet of the pelvis being more or less contracted; and in six the feet or the hand formed the presentation.

Of the fourteen labours in which the head presented, in two the uterus was

ruptured, causing the death of both mother and child; in two embryulcia was performed; in three delivery was effected by turning, two of the children being still-born, and one living; in three the funis was, by the hand of the accoucheur, lodged above the head, and the delivery trusted to nature, resulting in two children dead and one living; while in four, in which nothing was attempted to be done, all the children were still-born. In four of the six preternatural cases the children were living.

The mortality from prolapse of the funis, therefore, in my practice, has been great. Of twenty instances, in fourteen the children were still-born; and of the six children saved, two of these were footling labours.

The methods which I pursued with the view to saving the life of the child, were—

1st. *Turning.*—Mrs. B., extremely thin and delicate, the mother of several children, fell in labour at 2 o'clock in the afternoon. At 10 the same evening the waters escaped in a large gush, and a young surgeon, who was in attendance, on making his examination, found that the funis was in the vagina. Unthinkingly he suffered the patient to get out of bed and sit by the fire at times during the night. It was not till 5 the following morning that I was called, when I found the head presenting at the brim, and a doubling of the funis pulsating in the vagina. As the waters had entirely escaped, I was in some doubt what course to take; but finding there had been no pains since the rupture of the membranes, that the passage was relaxed and flabby, and that the woman was very patient, I introduced my hand into the vagina, and carried the cord upwards beyond the head. On discovering that this attempt did not excite pains, I thought it best to turn, which I effected with unexpected ease. The child, although feeble at first, in a few minutes revived perfectly.

Here the inirritable state of the uterus favoured turning, notwithstanding that the waters had escaped seventeen hours before. In general, turning, even so early as an hour after rupture of the membranes (the head presenting) is attended with great difficulty to the accoucheur, with much suffering and some danger to the mother, and commonly with destruction to the child. In such circumstances this method ought not to be resorted to. But should the accoucheur be in attendance when the waters give way, and immediately detect the funis, then turning is the best resource. In presentation of the funis with the shoulder, the operation may generally be expected to succeed.

2d. *Lodging the funis above the head.*—A patient of the hospital was the subject of this practice: the waters had escaped several hours before I saw her. I found the funis pulsating in the vagina in a great number of short turns; these passed down, in contact with the promontory of the sacrum, and, consequently, threw the head forwards on the pelvis. There was little or no uterine action. Having so generally failed in my endeavour to save the child in this kind of labour by turning, I resolved to try another method. Anointing my left hand with lard, I slowly carried the cord upwards, along the right side of the child's head, until it was fairly lodged above the head on the neck. Pains now came on, and every pain caused the cord to slip down a little, which I prevented with my hand as well as I could. At this period a dose of infusion of the ergot of rye was given, that probably helped to force the head down into the pelvis more rapidly than would otherwise have happened, whereby the funis was hindered from again descending. In an hour and a half a lively male child was born.

In two other instances I tried this same manœuvre, but without success. In one I thought I had succeeded in lodging the cord above the head, and out of the reach of undue pressure, but the child was born dead. In the other case, the waters having escaped, the head was so firmly fixed in the brim, that the cord ceased to pulsate during my persevering, but unavailing, efforts to return it. Nevertheless, with a moderate degree of dexterity, the funis may be now and then successfully reduced, as I learn from those of my colleagues with whom I have conversed on the subject.

3d. *Delivery with the forceps.*—I was called by one of the hospital midwives to a case of labour, described by her as "head, hand, and funis" presentation. Finding that the waters had passed off nearly ten hours before, and also that the parts were rather hot, I, previously to taking any step, introduced lard

freely. I now discovered that the vertex was presenting at the brim, towards the right acetabulum; that the right foot was down by the head in the right side of the pelvis, and a number of turns of the funis pulsating in the vagina down by the same side. As the pains were strong, and the waters had been so long drained off, I did not venture to bring down the feet, as I at first thought of doing, but pushed the foot up above the brim slowly between pains. The head soon began to enter the pelvis. I tried to pass the funis beyond the head, but did not succeed. I now left the patient for three hours; on returning, I found the funis protruding beyond the vulva, and still pulsating; the head had meantime descended a little, but more than half of it was as yet above the brim. I now again tried to reduce the funis, *i. e.* to lodge it above the head, but in vain, and therefore forthwith applied the long forceps, by which means, the head readily descending, the child was born alive.

In another case, which occurred about twelve months ago, I found the funis pulsating between the thighs of the patient, and the head of the fœtus pressing at the os externum. Not having the forceps with me, I was obliged to trust to the pains, aided by employing the fingers as an extractor. By the time the head was delivered, the funis had ceased to beat. With the help of the forceps or the lever, the child might easily have been saved.

Other modes of practice, besides those I have mentioned, have been recommended by various writers, some absurdly refined and fanciful, and others deserving attention. The reader will find them enumerated in most of the larger treatises on midwifery.

Manchester, Dec. 31, 1835.

ON THE "BRUIT DE SOUFFLET," OR BELLOWS SOUND, IN AUSCULTATION.

By Charles Cowan, M.D.

The "bruit de soufflet," or bellows sound, has been the subject of observation and experiment from a period very closely succeeding the discovery of auscultation; "but as to the connexion of this sound (says Magendie) with circumstances of plethora, whether general or local, with contraction of the cardiac orifices or nervous spasm, we know very little; we have not yet any sufficient data for determining the precise condition of the vessel in which the bruit de soufflet is heard."

It was at first regarded as almost pathognomonic of valvular disease, and considered to depend on friction, either from some morbid irregularity of the heart's surface, contraction of an orifice, or (as Laennec supposed) on a spasmodic condition of the muscular parietes of the heart. It has subsequently been discovered in the aorta and its ramifications, in cases of pericarditic inflammation, in certain examples of cerebral disease, and, what is still more interesting, as an almost constant phenomenon from the fourth month to the termination of pregnancy. These and other circumstances, such as bronchocele, aneurism, &c. in which the sound we are going to investigate is occasionally present, attach both an interest and importance to the subject which its first contemplation did not lead us to anticipate; while, at the same time, they afford materials for conjecturing its cause, and of attempting to generalize the conditions necessary for its production. It is for the elucidation of the last point that the present observations are more particularly intended.

And first it may be stated as a general principle, that the conditions requisite for the production of a physical phenomenon must necessarily be present wherever the latter may be found; and consequently all explanations, founded upon any peculiarity of structure or functions, must fail to be generally applicable, unless equally consistent with the phenomenon itself. Now since the bellows sound is known to exist elsewhere than in the heart, every explanation derived from the peculiar structure and functions of that organ can only be locally applicable, and is very possibly fallacious.

It results from the elaborate and ingenious researches of M. Bouillaud, in his admirable work on Diseases of the Heart, that, in nineteen cases out of twenty, where the bellows sound is observable, it depends on stricture of the cardiac orifices. The same pathologist has heard it where there have been

coagula in the ventricles; calcareous valvular vegetations or incrustations; a flabby and incomplete state of the valves; their adhesion to the cardiac or vascular parietes; dilatation of the ventricle and auriculo-ventricular orifice; considerable hypertrophy, with dilatation; in cases of copious hæmorrhage; and lastly, in nervous or chlorotic individuals.

Accompanying all these varied conditions, one common element may be traced—viz. *increased friction;* whether this is depending on constricted orifice, roughened membrane, increased rapidity of circulation, or on a want of normal proportion between the fluid and its containing tube. The variable intensity of the sound, its frequent subsidence and recurrence, and the uncertain morbid appearances discovered after death, are all easily explained, and satisfactorily prove that the mere presence of the bellows sound is of itself quite insufficient to indicate any certain morbid alteration with which it is invariably associated. Numerous accurate observations, particularly those of M. Bouillaud, seem to justify the conclusion that the rasping, sawing, whizzing, and hissing sounds, are only varieties of the bruit de soufflet; that they frequently replace each other, corresponding in their recurrence to modifications in the force and rapidity of the circulation. The correctness of these views is confirmed by the ingenious researches of Drs. Stokes, Graves, Law, and Mr. Main *, in cases of acute pericarditis. They clearly demonstrate that the deposit of coagulable lymph upon the surface of the heart and pericardium, distinctly gives rise to sounds closely imitating those taking place in the cardiac cavities, and presenting modifications which, as in the preceding instances, are evidently connected with the rapidity and impulse of the heart's contractions.

From the most gentle rubbing to the harsh and coarse rasping or whizzing sounds, corresponding variations in the number and force of the cardiac movements are observable, and the effects of depletion, either general or local, are immediately perceptible. The difference of friction produced by a fluid and that of the medium we have last adverted to, may naturally be expected to modify the nature of the sounds: this is, however, far less the case than might, *à priori*, have been anticipated; though we are inclined to the opinion that the peculiar rushing noise characteristic of the true bellows sound, whenever distinctly accompanying cases of pericarditis, depends either on valvular disease, unnaturally violent contraction, or hypertrophy, with dilatation of the heart;—at least such is the impression resulting from our limited experience; and in a case detailed by Dr. Stokes, where the bellows sound was unusually distinct, the morbid alterations to which we are alluding were found, after death, to exist *. Should this supposition be confirmed by additional experience, the power of distinguishing a sound produced by the motion of a fluid, from one depending on the friction of coagulable lymph, would not be either unavailable or uninteresting for increased accuracy of diagnosis.

In addition to the causes already enumerated, capable of exciting a bellows sound in the precordial region, there is one originating in conditions to which, we believe, we have first directed the attention of stethoscopists — viz. the action of the heart upon the lungs. The effect has been almost universally attributed to the pressure of the former on the thin layer of lung intervening between the left portion of the pericardium and the thoracic parietes; and the synchronism of the sound with the respiratory movements has been constantly mentioned as a sufficiently distinctive indication of its source. On the other hand, we believe it to depend on the mediate or immediate compression of one or more of the larger bronchi at the base of the lung, by an hypertrophied or dilated heart; a condition which, except in cases of extreme emphysema, is incompatible with the presence of the thin lamina of lung in the situation we have described. The bellows sound is also, in the circumstances we are supposing, synchronous with the heart's action, and cannot be distinguished from, or rather may be very easily confounded with, a bellows sound arising from some morbid peculiarity of the central organ of circulation itself. We

* Vide Dublin Journal, *passim.*

* Dr. Davies, in his Lectures on Diseases of the Chest, (MED. GAZETTE) by proving the frequency of valvular disease, and its sequel—hypertrophy, renders the dependence of the bellows sound on disease of the heart itself highly probable.

are satisfied, from two post-mortem examinations elsewhere detailed *, and from several living examples, that the seat of compression is one or more of the larger bronchi; and that when the obstruction is considerable, both the exit and entrance of the air are interrupted by the heart's action, and produce, as it were, a succession of short inspirations and expirations, synchronous with the pulse, and, when heard through the substance of the heart, closely resembling the bellows sound originating in the cardiac cavities. The similarity of these results should not excite our surprise, for the essential conditions of the bruit de soufflet are evidently present in the partially obstructed bronchus; where the air, entering with increased rapidity, in consequence of its volume remaining undiminished, impresses vibrations upon the bronchial parietes, precisely on the same principles as obstructed circulation reacts upon the heart or arteries.

The latter, when auscultated in their natural condition, manifest no other sound than a faint and distant murmur, depending upon the friction of the circulating fluid. By voluntary external pressure, this friction is increased, and we have a proportionate exaggeration of the normal noise, which may, at least in the larger arteries, be converted into a distinct bellows sound. The same effect would be the consequence either of accidental constriction or any increase of solid material, either from within or without, by which the calibre of the vessel is locally diminished.

If we auscultate an aneurismal tumor which involves some irregularity in the passage of the blood, a similar phenomenon is perceived; and if the internal arterial membrane becomes roughened by cartilaginous, osseous, or other deposit, the bellows sound is equally present.

The passage of arterial blood into a vein is accompanied with the same result. The bellows murmur, with some curious modifications, is also to be heard in chlorotic individuals; in cases where the heart's action is accelerated, or where the mass of circulating fluid has been suddenly lessened, in consequence of copious venesection. Dr. Fisher, of America (vide Medical Mag. No. 15), cites several cases where, by applying the ear to the head of individuals attacked with meningitis, a bellows murmur was distinctly to be detected; attributable, most probably, to the pressure of the larger arteries at the base of the brain, from the increase of fluid contents in a comparatively unyielding cavity; and perhaps also on some modification of the arteries themselves, which the researches of Dr. Alison prove to be dilated, when distributed to inflamed organs. We may mention, in confirmation of the correctness of this opinion, that the sound was diminished or suppressed, by either impeding or interrupting the circulation in the carotids. Dr. Graves (vide Dublin Journal, No. 16) details an interesting observation of acute pneumonia, where the bellows murmur was very distinctly heard over the portions of the chest corresponding to the affected lung: it no doubt originated in the larger arteries traversing the pulmonary parenchyma, since every precaution was taken, by that sagacious observer, to exclude sources of error in the diagnosis. In the increased impetus of the blood, the fact of capillary obstruction, and the improved conducting power of the lung, we may trace the necessary elements of the phenomenon.

A very superficial examination of the preceding examples, while it discloses various combinations of physical conditions, admits of their being classed under one common head—viz. of acting as obstacles to the circulating fluid; whether by presenting an unnatural outlet to the blood, producing irregularity of the lining membrane of the tubes, local alterations in their calibre, or a relative disproportion between their containing capacity and the volume of their contents.

The common effect would be increased vibrations of the arterial parietes, and the production of a sound more or less uniform in all.

The source of the bellows murmur in pregnancy is far less easily analysed, and has excited much greater diversity of opinion, than any of the preceding examples; but we think, that, by attention to the facts already adverted to, it may be proved to depend upon physical conditions equally simple with any to which we have previously alluded.

M. Kergaradec, who first directed attention to the subject, ascribed the bellows sound to the passage of the blood through the placental vessels;

* Vide Med. Gazette.

Laennec conceived it to originate in the uterine arteries; M. Paul Dubois, in the vascular system of the arteries generally; Dr. Kennedy, in the uterine arteries, aided, probably, by the circulation in the maternal placenta; and Dr. Forbes, in the uterine vessels corresponding to the portion immediately connected with the placenta.

These conflicting opinions point out the obscurity which has hitherto surrounded the investigation,—an obscurity to be ascribed, we believe, rather to the isolated manner in which the phenomenon has been studied, than to its intrinsic difficulties. One and all of the preceding hypotheses, supposing certain conditions in the uterine and placental circulation, which we cannot admit, might more or less satisfactorily account for the presence of a bruit de soufflet, but from their too great speciality, we do not hesitate to reject them, and to decide in favour of the opinion of M. Bouillaud, who conceives the placental murmur to result from the compression of one or more large vessels of the abdomen by the uterus charged with the produce of conception. In the first place, it is a general law of the circulation, that in proportion as the blood recedes from the heart, its progress becomes equal and continuous; and it does not appear probable that the impulse of the mother's heart should be propagated through the tortuous uterine or placental vessels, and still retain in their ultimate ramifications so distinctly an intermitting character as the nature of the bellows murmur would imply. Such a state of circulation would be different from any we are acquainted with in the structure of our different organs, and difficult to reconcile with the want of vascular continuity between the placenta and uterus, and with the now admitted independence of the fœtal circulation upon that of the mother.

Besides, under all other circumstances, with one doubtful exception, the bellows sound may be traced to some obstruction of the heart or arteries; and the nicest and most practised ear, as Dr. Montgomery says (vide Cyc. Pract. Med.; art. Pregnancy, p. 484), cannot detect any difference between sounds produced by these conditions, and those accompanying the pregnant uterus. This accurate observer has recorded a case of an enlarged sarcomatous uterus, where the phenomenon was heard in its most perfect form; also another example of an abdominal tumor compressing the aorta, where the murmur was equally distinct; and M. Bouillaud has observed it in the left iliac region in a female with a tumor in the ovarium, compressing the corresponding iliac artery. In addition to these facts, we may mention, that the same effect may be produced by external pressure with the stethoscope.

M. Bouillaud has succeeded in *displacing* the placental murmur, by making the patient lie alternately on the right and left sides; and in the only instance which we have had an opportunity of carefully examining since our attention was directed to this particular view of the subject, the same phenomenon was distinctly reproduced. These facts are incompatible with the hypothesis of the bellows sound depending on either the placental or uterine circulation, and to a certain extent invalidate the generally-expressed opinion, that the point of greatest intensity of the murmur is always identical in the same individual. But that this should literally be the case in the great majority of instances where the effects of change of position are not studied, would simply result from only the arteries of one side being compressed; and probability is greatly in favour of that pressure coinciding with the heaviest portion of the uterus, viz. that on which the placenta is attached.

The more diffused nature of the sound in the advanced periods of pregnancy, may be explained by the greater arterial surface exposed to the mechanical action of the uterus; and the fact of the murmur being occasionally audible for a short time after the placenta is detached, but instantaneously ceasing when uterine contraction has taken place, would in the first instance depend on the volume of the uterus not having materially diminished, and the cessation of sound, on the conditions necessary for pressure being removed. We may also advert to the fact, that the bellows murmur is never detected before the uterus has risen above the margin of the pelvis, or, in other words, until this organ would be capable of acting in the manner we have supposed; and all writers agree in describing the sound as most distinct over one or other of the iliac regions. The modifications in the nature of the murmur by the interrup-

tion of the fœtal circulation, by the removal of the placenta, by the death of the fœtus, may all be explained by accompanying changes in the volume and weight of the uterus. Lastly, as almost demonstrative of the opinion we are supporting, we have ascertained, after careful examination, that the bellows sound can be detected in the *femoral arteries*, immediately below the arch,—a fact in every respect analogous to what takes place in the large arteries of the neck, in cases of disease of the aortic valves. We have also found that the sound may, by change of position, be transferred successively from one femoral artery to the other, always corresponding with the side of the uterine murmur; and, lastly, that the sound is in every respect similar, though weaker and less diffused, to what is heard in the region of the uterus itself,—facts we should be unable to reconcile with any other hypothesis than the one we are endeavouring to establish.

The occasional presence of a bellows sound in cases of bronchocele of considerable size, is rather confirmative than opposed to the views we entertain,—the carotid arteries, in respect to the enlarged thyroid gland, being in exactly similar relations as the iliacs in reference to the pregnant uterus. The character and intensity of the sounds are in both cases modified by variations in the size and pressure of the tumor. We have recently examined a case of bronchocele, which placed the dependence of the bellows murmur on the conditions we are supposing in the clearest light. The central lobe was considerably enlarged, but gave not the slightest indication of sound to the stethoscope. The lateral lobes were less voluminous, but insinuated themselves on either side, beneath the sterno-cleido-mastoidæi muscles, producing a curved projecting appearance of the latter. On the right side, over a portion corresponding to the thickest portion of the lobe, and immediately over the carotid, a distinct and loud bellows sound could be heard, rapidly decreasing in intensity as the stethoscope was moved either upwards or downwards, but more especially in the latter direction. On the left side the phenomenon did not exist, the deeper portions of the lobe evidently not interfering with the calibre of the corresponding carotid. The absence of the sound over the heart was ascertained, and the possibility of artificially producing it by the pressure of the instrument on the carotid, not overlooked.

Dr. Forbes (art. Auscultation, Cyc. Pract. Med.) asks whether a bellows sound will be found to exist in active hypertrophies of other parts of the system? We would say *not*, unless conditions more or less similar to some of those we have insisted upon in the preceding examples are at the same time conjoined.

Having thus rapidly alluded to the various circumstances in which the phenomenon we are considering has been associated, we feel justified in concluding that, with one doubtful exception (pericarditis), it is every where depending on similar conditions, viz. on some impediment to the normal function of the heart, arteries, or air tubes, by which the circulating fluid is more or less obstructed in its course, and the parietes of the tubes thrown into unusual vibrations. A bellows sound is therefore indicative of obstruction, but alone is quite insufficient to point out the nature of the obstacle.

In medicine, as in every other science, it is always better to reason from the known to the unknown; and our present mode of viewing the question not only simplifies a hitherto somewhat obscure subject, but attaches it to a series of phenomena, the nature of which is both rational and easily understood.

M. Magendie, at one of his recent lectures, fitted a large syringe to elastic tubes of various diameters, and forced through them a quantity of fluid. A very distinct bellows murmur was produced, whence he concludes that probably the seat of the anormal sound resides in the parietes of the vessels themselves. In a portion of an elastic tube which had partially yielded to the impulse, the sound heard in the corresponding point exactly resembled that described by Corvisart as a symptom of aneurism of the ascending aorta. M. Piorry (vide Collection de Memoires sur la Physiologie, &c. 1831, p. 105, *et seq.*), to whose experiments M. Magendie makes no allusion, has also distinctly produced the bellows sound by means similar to the preceding, as well as by the forcible passage of fluid through the hand immersed in water,

and closed in the form of a tube. The sound was augmented by increasing the force of the current, and was rendered more intense by the presence of air mixed with fluid. In all his experiments there was a close analogy between the artificial sounds and those occurring in the heart and arteries. He was unable to imitate the rasping sound and its varieties, though Magendie appears to have partially succeeded, by increasing the roughness and hardness of the obstruction.

M. Cagniard Latour (says Dr. Williams, in his excellent work on Auscultation, third edition, p. 193), has succeeded in producing not only sounds, but musical notes, by the motion of liquids through tubes and apertures of different dimensions; and there can be no doubt of gaseous bodies acting in the same manner. Dr. Williams himself concludes that all the sounds in question depend upon "a certain resistance given to the blood moving with a certain force;" and this appears also to be the view of Dr. Hope and Dr. Spittal.

M. Raciborski (vide Man. of Auscult. Trans. p. 139) conjectures that the peculiar sound accompanying chlorosis may depend on the *quality* of the blood; but the argument in support of this opinion which he adduces, viz. that he has observed the sound artificially produced after very copious venesection, and again disappear when the blood had regained its qualities under the influence of tonics, cannot be regarded as in any degree confirmative, since *quantity* offers a far more palpable and natural means of explaining the effect, than the vague idea of change in quality. Whether this last condition is ever capable of modifying the sounds, we are unable to assert; the only analogous experiments we know of are in reference to gases, which produce different tones in their passage through glass tubes of similar diameter: but here the densities of aeriform bodies are far more disproportionate than can be imagined to exist in the most extreme modifications of the circulating fluid; and, moreover, both numerous experiments and observations on the living, prove that air, blood, and water, are all capable of originating sounds of a closely approximative, if not identical character. The possible influence of quality on the nature of vascular murmurs, is, however, yet undecided; and much obscurity still attends the precise explanation of the sounds characteristic of chlorosis.

Bath, Dec. 22, 1835.

THE DISCOVERY OF THE ARTERIES PRODUCING ERECTION OF THE PENIS IN MAN AND OTHER ANIMALS.

By Professor Mueller, of Berlin.

[From his "Archiv für Anatomie," &c.; the translation executed at Berlin, under the Professor's inspection.]

All that has hitherto been known of the anatomy of the penis is with reference to its general structure; its minute organization, producing erection, has been up to this period undiscovered. Neither Cuvier, Tiedemann, Moreschi, nor Panizza (although they have explained some details of the structure of the penis, viz. the venous arrangement producing the anastomosing chambers of the corpora cavernosa), have known the real and efficient part of the erectile tissue.

For a long time, continued investigations have given me more certain and important disclosures of the construction of this organ. Having already * treated of the fibrous substance which, in the horse, lies between the anastomosing veins, I will now communicate a more important part of my observations: it relates to those hitherto undiscovered arteries which, without doubt, are the proximate cause of erection,—a phenomenon which has hitherto remained so obscure and void of explanation.

The internal pudendal artery, after it has passed through the greater ischiatic notch out of the pelvis, and advanced through the lesser notch into its cavity again, gives off immediately the art. hæmorrhoid. externa; and then the arteria perinæi, its continuation, lies just within the inferior aperture of the pelvis, upon the tuber ischii and the ascending ramus of the os pubis; being accompanied by the vena pudenda interna and nervus pudendus, and covered by the lateral portion of the musc. levator ani. It then passes upwards, between the musculi ischio-cavernosus and bulbo-cavernosus. Whilst between these

* See J. Muller's Handbuch der Physiologie, p. 304.

two muscles it usually gives off the art. corp. cavern. urethræ, a branch which is almost as large as the continued trunk of the vessel. This continued trunk is the arteria penis; it passes between the before-named muscles to the back part of the root of the corpus cavernosum penis on its side; and, whilst extending over it, it divides into two branches, the art. *profunda* penis, and the art. *dorsalis* penis. The art. profunda penis penetrates the root of the corpus cavernosum from above inwards, and ramifies in it as the principal artery of the erectile tissue of the corpus cavernosum penis, carrying the blood which serves both for nutrition and erection. The art. dorsalis is continued over the corpus cavernosum of its side farther, even to the glans, lying on the outer side of the vena dorsalis penis, and having on its outside the nervus dorsalis penis. In this manner it sends, soon after the division of the original vessel, branches to the corpus cavernosum penis; and distributing itself to the fibrous envelope of the penis, its fascia, outer skin, and to the prepuce, it terminates by ramifying in the glans penis.

In the horse the erectile tissue of the corpus cavernosum penis is provided with double sets of arteries,—with a set from the arteria obturatoria, as well as that from the pudenda interna. The former, after it has passed through the foramen obturatorium, gives off a branch to the root of the penis, which not only supplies the musc. ischio-cavernosus, but also a twig which penetrates the inner part of the root of the penis, accompanied by a considerable branch of the nervus dorsalis penis. This vessel distributes itself upon the posterior part of the penis, whilst the continued trunk of the branch of the art. obturatoria, which supplies the penis, passes over its dorsum, and becomes the art. dorsalis penis, which sends additional twigs through the fibrous envelope of the corpus cavernosum into its erectile tissue. The arteria penis of the art. pudenda interna, therefore, of the horse, distributes itself in a totally different manner from the analogous vessel in man.

Its first division is the deep branch of the corpus cavernosum *urethræ*, from which also proceed twigs distributed to the corpus cavernosum *penis*, corresponding to the branches of the arteria profunda penis in man. The upper twig is very short, and unites itself with the art. obturatoria*.

The distribution of the principal branch of the arteria penis was all that was hitherto known of this important vessel: it is in the more minute distribution of its branches through the corpus cavernosum penis that the secret of the cause of erection has remained concealed. It is not a lucky accident, but a systematical arrangement, and investigations proceeding from certain combinations, that have guided me, in this instance, to an important discovery. It is generally thought that the arteria profunda penis, which nourishes the substance of the penis, is at the same time capable of filling the cells of the corpus cavernos. with blood during erection also; that the nutritive blood, as well as that serving for the purpose of erection, proceeds from the fine twigs of the same vessel into the fine (capillary) veins; that it passes inwards from these into the sinuous veins, and again from these to the vena dorsalis penis; and that the state of erection distinguishes itself from the ordinary state of circulation in this organ, partly in the quantity of blood circulating in its vascular net-work, and partly in an impediment offered to the return of the blood through the veins of the penis. The older writers, on the other hand, considered the sinuous veins of the corpora cavernosa to be cells;—imagined that the nutritive blood supplied by the ordinary circulation did not at all reach these cells, or pass through them, but was carried off by appropriate veins; and that still, during the state of erection, the cells of the corpus cavernosum were filled with blood. Both views are, as we shall see, incorrect. The opinion of the old writers is proved to be unfounded, as well by investigations on the dead body as by the vivisections of beasts. In many experiments performed on the living horse, dog, ram, &c., I have observed, that upon making an incision in the corpora cavernosa, these bodies were not, in their unexcited state, much charged with blood, but that blood is contained in their sinuous veins, although certainly in less quantity than in the corpus cavernos. urethræ, which bleeds freely when cut across. Farther, in the majority of human subjects blood

* See Gurlt Handbuch der Vergleichenden Anatomie, 2 Bd. p. 281.

is found in the sinuous veins of the corpora cavernosa penis.

With regard to other views, I had, through different experiments and anatomical investigations, brought it to the highest step of probability that the cause of erection did not lie in the mere checking of the blood in the vena dorsalis penis, but that it must be hidden in the tissue of the penis itself. This view had increased to that degree which involuntarily leads us to prosecute an idea through all difficulties of investigation.

I resolved now to apply to the arteries alone those injections which I had hitherto directed principally to the veins. The idea that the vessels which poured out blood during erection differed from those which passed it over to the veins forming the capillary net-work (serving the purpose of nutrition), had haunted me already a year. The want of success that had so long attended my injections had almost deprived me of all hopes, when the long-expected discovery was made. It was whilst drawing up a memoir on the erectile tissue and erection, for the Encyclopædial Dictionary of the Medical Sciences, published at Berlin, that this idea presented itself to me, and induced me lately to make new and finer injections of the arteries. It was one of the happiest days of my life when I repeated this injection on a human penis, through the arteria profunda,—it led me to the discovery of the wonderful difference in the distribution of the fine twigs of this artery. Both sets of branches, as well those which nourish the corpora cavernosa penis in their internal structure as those which form scrolls during erection, are derived from the art. profunda, and are given off from it during its course through the spongy tissue. These two sets of vessels differ from one another as much in their size as their course, their form as in their object.)

I. *The nourishing Twigs (Rami nutritii Arteriæ profundæ Penis.)*

When an injection of the arteries of the penis is made with size and vermilion, a considerable portion of the injected mass is always forced into the cavities of the corpora cavernosa, as well in the human organ as in that of the horse and dog. When this mass of injection (of which I am still uncertain as to the means by which it enters into the cellular structure) is washed out, the rami nutritii will become evident. The rami nutritii of the spongy substance (which, since they are upon the walls of the sinuous veins in the interior of the penis, may be also called the vasa vasorum) are found to be as minute as the arteries of any other part: they distribute themselves upon the pillars of the spongy substance, until they become too fine to be perceived by the naked eye. As in the arteries of other parts, they anastomose; and lastly, they form, as in other parts, the capillary net-work which is so difficult to be injected in the penis, owing to the facility with which the injection escapes into the cavities of the corpora cavernosa.

II. *Arteriæ Helicinæ Corporis cavernosi* (in Man.)

In order to see these arterial branches satisfactorily, an injection composed of size and vermilion must be thrown into a separated penis, through the art. profunda. (In the horse the pudendal and obturatorial artery are to be injected together.) As before mentioned, a part will escape into the cavities of the corpora cavernosa. When the injection has become cold, the corpora cavernosa must be cut open longitudinally, and that portion of the injection which has escaped into the cells is then to be washed out with great care. If a size of a greater degree of consistence has been employed, it will be found to have become solid on cooling. In this case the penis must be soaked in water, and the mass squeezed out softly and carefully, until the cellular tissue is emptied. When a thin size has been used, this will, of course, not occur; then washing alone will be sufficient.

If the tissue of the corpora cavernosa be now examined with a magnifying lens on its posterior third, it will be seen that in addition to the distribution of the arteries already described, there is another class of vessels, having an entirely different form, size, and distribution; these branches are short, being about a line in length, and a fifth of a millimetre in diameter; they are given off from the larger branches, as well as from the finest twigs of the artery. Although fine, they are still easily to be recognized with the naked eye; they come off from the artery mostly at a right angle, and projecting into the cavities of the spongy substance, they

Fig. 1.

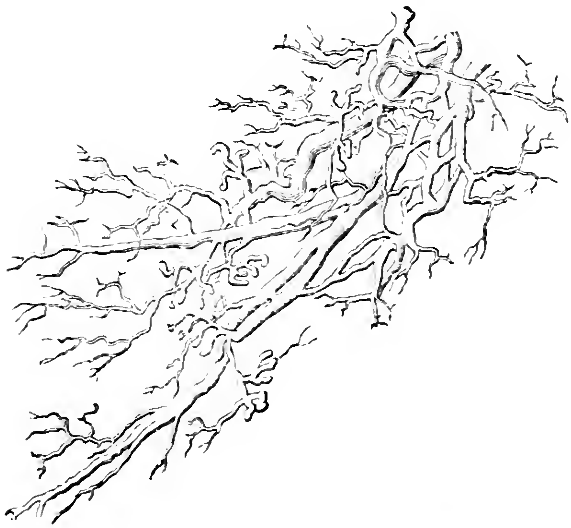

Fig. 3.

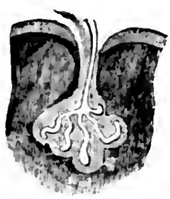

Fig. 2.

Fig. 4.

Fig. 1 is a representation of the arteriæ helicinæ, as they appear in the corpora cavernosa of the stallion. It will be observed here that in some instances nutritive twigs arise from these vessels, a circumstance which does not occur in man.

Fig. 2 is a drawing of a very highly magnified portion of the arteriæ helicinæ, taken from the posterior portion of the corpus cavernosum urethræ of man.

Fig. 3 shews a single bunch of these vessels, with its envelope.

Fig. 4 represents a portion of the art. profunda penis (in man), with its attached vessels, somewhat enlarged.

either terminate abruptly, or else swell out into a club-like process, without again subdividing.

In the penis of the stallion these vessels must be sought after in the root of the corpus cavernosum penis, in preference to the other parts, since, as the injection succeeds better here, they will appear entirely perfect.

In the dog they are evident in the entire structure, but they are at the same time very difficult to be sought after, on account of the innumerable tendinous fibres which pervade the whole of the spongy bodies.

In the horse they are likewise difficult to be discovered, with the exception of the corpus cavernosum urethræ; but in man they are the easiest; and the human penis also possesses the most peculiar structure.

The following description relates to the penis of man. These twigs branch off from place to place, sometimes alone, sometimes in greater number: little bundles will be seen, in which from three to ten twigs stand together; these, as well as the former, project constantly into the cells or venous cavities of the corp. cavern. penis. When the arteries thus form a tuft, they arise by a common stem, which immediately divides itself into the separate branches. Sometimes such a vessel, whether it proceeds from the artery as a single branch, or forms part of a cluster, divides itself into two or three parallel branches, which also end either abruptly, or else swell out near their extremity.

Almost all these arteries have this character, that they are bent like a horn, so that the end describes a half circle, or somewhat more. When such a branch so divides itself, there are formed doubly bent twigs, inclined one to the other. I have before observed, that many of these arteries enlarge towards their end; this enlargement is gradual, and is greatest at some little distance from the extremity, so that the end is somewhat conical. This cone, however, is rounded at the point, and, giving off no branches, terminates immediately. The diameter of these twigs, in their middle, is from the fifth to the sixth of a millimetre: they preserve a great similarity: thus those which branch off from the large trunk of the artery are not thicker than those which take their origin from the finest subdivisions. (See fig. 4.) Indeed, the finest twigs of the art. profunda often evidently give off branches of this kind, which are much thicker than the vessel from which they arose.

These remarkable vessels possess throughout a great similarity to the tendrils of the vine, except that they are much shorter in proportion to their thickness; therefore I have named them the "arteriæ helicinæ." Their termination may be also compared to a crosier. By a more minute examination of these vessels, either with a lens or a microscope, it will be perceived that although they at all times project into the venous cavities, yet they are not entirely naked; but it will be seen that they possess a delicate membranous covering, which, under the microscope, appears to be granular. (See fig. 3.) After a more forcible injection this envelope is not visible. When the arteries form a bundle, the whole is covered by a slight gauze-like membrane.

I found within the venous cells of the posterior part of the corpus cavernosum urethræ small clusters of these arteriæ helicinæ, in which the thickness of the soft mass surrounding each artery was much greater than the calibre of the inclosed vessel. (See fig. 2.) Perhaps this difference arises from the circumstance of these vessels not being sufficiently distended with the injection mass. In this case the arteries appear finger-shaped. The ends of the processes were less bent upon themselves than was the case in the art. helicinæ of the corpora cavern. penis. Still the arteries contained in these finger-form masses were invariably bent upon themselves within the soft substance, so that the somewhat pointed end came nearer to the surface, without having an aperture. I know not whether this covering consists of the inner, and here much attenuated, skin of the veins forming the cells of the corpora cavernosa penis; I doubt it, and conjecture that these coverings perform a very important part in producing the phenomenon of erection.

Our arteries have no openings which can be detected, either on their surface or at their extremities; and if the blood, as it is probable, proceeds from them during erection in greater quantity into the cells of the corp. cavernosa, so it

must either pass through invisible openings, or, at least, through openings which only become enlarged by the great extension of the vessels. If the great number of these tendril-like branches which are given off from the art. profunda penis be considered in comparison with the many fine nutritive twigs of the same vessel, it must be evident that when the former are filled, they must take up by far the greater portion of the blood conveyed by it. The diameter of the art. profunda, therefore, not only includes the nutritive twigs which arise from it, but also the tendril-like branches, which likewise deriving their blood from it, yet, it is probable, allow none to pass except during erection; therefore the blood in the unexcited state of the penis only pervades the nutritive branches, and thus only reaches the commencement of the venous cells in smaller quantities; whereas, during erection, it is probable that the blood passes in quantity through these tendril-formed vessels into the cells.

Many of our attentive readers will perhaps imagine that these vessels, instead of terminating, do more probably form loops, which, turning upon themselves, become venous, as those looped vessels which were discovered by E. H. Weber to penetrate the villi of the placenta, and project within the venous sinuses of the uterus. But this is not so. Our vessels are simple; they bend themselves at the end, but they do not return to their trunk as a loop, but are simply blood-containing processes of the arteries, which project freely into the cellular cavities of the veins of the corpora cavernosa. These veesels are most numerous in the posterior part of the corpora cavernosa penis; in the middle and anterior parts they occur but seldom. They are also present in the corp. cavern. urethræ, especially in the bulb; anteriorly here, also, they become less frequent, and, as yet, I have not clearly perceived them in the glans penis. They are much more difficult of detection, however, in the corp. cavern. urethræ than in those of the penis, where their demonstration, especially in the human penis, is very easy.

The greater development of these arteries in the posterior part of the organ corresponds with the fact of erection being always earlier evident there, as if the blood distributed itself from thence into the venous cells.

The preceding description is entirely confined to the human organ. It was in man that I first attempted to discover the anatomy of these vessels, and here they are most easily seen. The tendril-formed vessels are indeed present in the penis of other animals, but are in them much more difficult of demonstration than in man; their form corresponds mostly with those of the human penis, especially in the apes, where I have investigated them in the simia sylvanus. In other animals they are very irregular, as, for instance, in the dog, and in the corpus cavernosum urethræ, and the posterior part of the corp. cavern. penis of the stallion; for they frequently give off from their side many nutritive twigs; and we can only compare the free obtuse ends, or processes which remain undivided, with the tendril-formed arteries of man. (See fig. 1.)

The importance of this discovery in explaining the phenomenon of erection must be perceived by every one. In the meantime we will not enter into a theory concerning its cause, but leave the reader rather to pursue its further process himself, on the ground of our researches.

EFFICACY OF CHLORIDE OF ZINC

IN THE TREATMENT OF MALIGNANT ULCER.

To the Editor of the Medical Gazette.

SIR,

I HAVE much pleasure in presenting to the public, through your valuable journal, the following case of malignant ulceration, successfully treated by the chloride of zinc, on the principles explained in my two papers, published in the Medical Gazette of December 12th and 19th.

The patient, Ann Whaller, aged 55, of impaired constitution, has been, since the 21st September, 1835, under the care of Mr. Howship, at the Charing-Cross Hospital, for an ulcerated state of the integuments of the chin and upper lip, of an extremely obstinate nature. The affection had commenced about eighteen months previously, in the form of a small tubercle, or pimple, on the chin, which, from being irritated with

the finger, ulcerated, and showed no disposition to heal. She became, in consequence, an in-door patient of St. George's Hospital; from which, after some time, she was dismissed relieved. The disease subsequently returned in an aggravated form, which induced her to obtain admission into the Charing-Cross Hospital, at the above date. As arsenic and the other remedies which had been employed were productive of only temporary benefit, I applied the chloride of zinc, by the kind permission of Mr. Howship, on the 29th November, when the following appearances were observable. On the left half of the upper lip, extending to the ala nasi, there was an ulcer of the size of a half-crown. Its margins were hard, and somewhat elevated; its surface had a dirty-grey aspect, and emitted an offensive sanious discharge. There was another ulcer, which occupied the principal part of the chin, similar in character to the above, but with more extensive hardness of the surrounding integuments, which had a livid-red hue. Nearly the whole of the under lip was destroyed, causing a permanent exposure of the alveolar ridge, and a constant trickling of saliva; giving a hideous appearance to the patient. Both sores were the seat of acute pain.

November 30th.—Complained of considerable pain, in consequence of the application of the paste; but as she is habitually querulous, little stress was laid on the circumstance. On removing the chloride of zinc composition, the dry greyish-white eschars, characteristic of the chloride, were observed.

Poultices ordered.

December 2d.—Slough has come away from the chin. She acknowledges herself to be free from pain in the parts. Eschar on the upper lip getting detached round the edges.

Continue poultices.

7th.—The slough on the upper lip separated the night before last, and has left a very healthy granulating surface; all surrounding hardness has disappeared. As there still remained considerable induration round the sore on the chin, the application of the paste was renewed to this part.

Simple dressing.

10th.—Ulcer on upper lip quite healed.

12th.—Slough has separated from the chin, leaving behind a well-conditioned sore.

18th.—Ulcer much smaller; continues daily improving.

Strips of adhesive plaister to be applied.

24th.—Sore nearly cicatrized.

Continue the dressing.

The rapid amendment which followed the application of the chloride in this case, is the more remarkable when we take into account that the woman's constitution had been previously undermined by the inroads of vice and disease.

Several other cases are now in progress, of which the results will be duly reported.—I am, sir,

Your most obedient servant,

ALEXANDER URE.

13, Charlotte-Street,
Bedford-Square, Dec. 26, 1835.

ELECTRICITY AND THE NERVOUS INFLUENCE.

A Final Reply to Dr. Williams,

BY DR. WILSON PHILIP *.

[With this letter the discussion between our respected correspondents must be allowed to drop.—ED. GAZ.]

To the Editor of the Medical Gazette.

SIR,

DR. WILLIAMS is particularly unfortunate in accusing me of errors, in a reply in which (a result I was little prepared for) there is hardly a paragraph free from inaccuracies. Had I been sooner aware of such a possibility it would have saved us both a good deal of trouble†.

He quotes my observation as erroneous, that the electricity of electric animals is incapable of affecting the common elec-

* *Errata in Dr. Philip's last paper.* — Page 444, 2d column, line 15, *before* "which," *insert* "the functions of."—Page 445, 1st column, line 23, *for* "body," *read* "blood."— Ibid. line 42. At the end of this line there should have been a reference to the following note:—This observation applies equally to the organs of the nervous as to those of the other powers of the living animal. It is by their peculiar mechanism that they are enabled to supply and direct the agent on which all the nervous functions, properly so called, depend.

† Dr. Williams accuses me of having modified my expressions. He should have stated what the modification is that he refers to: I am not sensible of the slightest modification of any of them.

trometer*. The word "common" is omitted by Dr. W., which wholly alters my meaning. This may be an error of the press; but the following must be an error of Dr. Williams. He refers to Dr. Davy's papers on the Torpedo, in the Philosophical Transactions, for a refutation of my statement. Dr. Davy's words are,—

"In accordance with Mr. Walsh and my brother, I have in no instance seen the torpedo affect the common electrometer, or exhibit any, the slightest, indications of a power of attraction and repulsion in air." (Phil Transactions for 1832.) And in his last paper on the same subject, in the Phil. Trans. for 1834, in enumerating the resemblances between the electricity of the torpedo and that of inanimate nature, he does not mention the former affecting the common electrometer,—a proof that he still found the fact as he had before stated it. Here are all the latest authorities on the subject.

These facts are surely sufficient to prove my position, that even the electricity peculiar to what are called electric animals, respecting which there is no difference of opinion, is modified by the other powers of the living animal; which renders altogether inconclusive the arguments of Dr. Williams, derived from the properties of the electricity of inanimate nature.

Dr. Williams, I think, will hardly, on reflection, adduce such cases as he states, where so many chemical affinities are operating, as proofs that oxygen and carbon will combine by mere juxtaposition. Let him for a moment consider how much is necessary to make such cases serve such a purpose.

Be this as it may, it cannot, as far as relates to animal temperature, affect my position, resting, as it does, on direct experiments; from which it appears, that when the nervous influence is wholly withdrawn, the process which supports animal temperature ceases; and that when this influence is partially withdrawn, that process is impaired exactly in proportion to the degree in which it is withdrawn; and, lastly, that after it is wholly withdrawn, the temperature of living arterial blood is immediately raised several degrees by subjecting it to voltaic electricity, which has no such effect either on dead blood, whether arterial or venous, or on living venous blood, that is, living blood which has undergone the action of the nervous influence.

With respect to what is said in the latter part of the same paragraph, has Dr. Williams ascertained that all combustion is not the effect of electricity? On this head he will find Dr. Faraday rather a formidable opponent.

Dr. Williams could not have so mistaken me as he does in the paragraph marked 1, and the note appended to it, had he read my reply, in the Philosophical Transactions for 1829, to MM. Breschet and Edwards, whose authority he adduces against me. Above five years have now elapsed without any attempt on their part to maintain their former statements; and with regard to the latter part of the same paragraph, it must have been hastily written. Dr. Williams will not surely maintain that there are no circumstances but the feebleness of the power which can prevent electricity becoming evident to our tests.

The most unlooked-for inaccuracy of Dr. Williams, however, is, that even at this, the eleventh hour, he is not aware of the foundation on which my inference rests; although, I think, it will be admitted that I have explained it with sufficient care. No man, in the least acquainted with the subject, could infer the identity of the nervous influence and electricity, merely from the circumstances enumerated in the paragraph marked 2, as the foundation of my opinion.

Dr. Williams reminds the reader that the electric organs are peculiar to the animals called electric. For this, of course, we are prepared, as other animals do not possess their peculiar faculty; but in such a discussion as the present, he should have added, that these organs, in a great degree, consist of nervous matter; that is, of what belongs to all the more perfect animals. Dr. Davy, who examined them with great accuracy, says, in his first paper, page 265, "Their great proportional size, the vast proportion of nerve with which they are supplied," &c.; and in page 269, he says, the electric "columns formed of tendinous and nervous fibres, distended by a thin gelatinous fluid," &c. From

* Medical Gazette, for Dec. 12, p. 396.

both passages, it is a necessary inference that nervous matter is essential to their function.

But the most important fact relating to the peculiar faculty of electric animals, the electric nature of which is universally acknowledged, is that, like the other nervous functions properly so called, it is immediately dependent on the brain. Dr. Davy, in his last paper, observes, "When the brain has been divided longitudinally, the fish has continued to give shocks; when the brain has been entirely extracted, the fish instantly lost this power, though the muscles generally continued to act powerfully."—P. 546. That is to say, that the electric power is not, like the muscular, independent of the brain, but, on the contrary, immediately dependent on it,—proving that in this, as in the other nervous functions, as shewn elsewhere *, the nerves are the passive, the brain and spinal marrow being the only active, parts of the nervous system. I need not say how powerfully this fact countenances the inference which is the subject of the present discussion. It is gratifying to see that here, as generally happens, when truth is once arrived at, other facts, beside those which conducted to it, arise to give it their aid.

Dr. Williams states that in one of my experiments the animal died in consequence of the electricity in two hours and a quarter, and gives this as a reason for believing that it does not prove fatal by exciting inflammation of the lungs; but he neglects to state that in this instance the subject of the experiment was a dog, which is much less tenacious of life than the animal (the rabbit) the usual subject of my experiments, which sometimes survived the effects of the electricity nearly twenty hours; but the most material oversight here is, that he quite forgets that inflammation must be much more rapid in its fatal effects when the cause which produces it continues to be constantly applied, than where, as in ordinary cases, the operation of the cause of the disease ceases at its very commencement. The proofs of the inflammation from the effects of the electricity being the cause of death in the cases referred to, are very simple. The animals died of oppressed breathing; and on dissection it was found that the only cause of this symptom was the inflamed state of the lungs. But enough of the ungracious task of pointing out misconceptions.

For misconceptions all must be prepared who attempt to correct prevalent errors, which always tend, in proportion as they interest the feelings, to impede the judgment. We do not readily forego opinions for which we can quote a list of authorities; but having so fully explained myself on the present occasion, I did not expect such misconceptions as some of those I have pointed out.

Dr. Williams, in our long discussion, has given dissertations on electricity, and on some of the powers of the living animal; and he has referred copiously to the prevalent opinions of the day, without much discrimination respecting his authorities. He calls my inference an hypothesis, and very inaccurately compares it with the systems of Brown and Broussais, to which it has, neither in its nature, or its object, nor in the foundation on which it rests, the slightest resemblance. All this may make an impression on the ill-informed; but I ask any one who has seriously considered the subject, if he can extract either from Dr. Williams's original strictures, which produced the present discussion, and which, the reader must perceive from many parts of the discussion, were adduced with a very imperfect knowledge of my publications on the subject,—or from his numerous replies, one sentence fairly bearing on the points at issue, and to which I have laboured in vain to direct his attention.

I have been allowed to maintain without contradiction the following positions:—

1. That a living power, properly so called, can exist in no texture but that to which it belongs in the living animal.

2. That none of the powers of inanimate nature are capable of the functions of a living power, properly so called; and lastly (a position equally supported by the properties of a living power properly so called, and direct experiment),

3. That no analogy can be observed between the functions of a living power,

* See the first of my papers in the Philosophical Transactions for 1829.

properly so called, and the operations of the inanimate world, except where some of the agents of that world co-operates with it.

To my original inference I therefore recur, namely, that the result of the experiments being admitted, the identity of the nervous influence and voltaic electricity is a *necessary* consequence; because if this be denied, we must admit that there are two distinct principles, the one of which possesses all the properties of the other; and I believe that those, whose minds are practised in such subjects, will allow that no more conclusive proof of any inference can be adduced; and that none of the positions which have been brought against it can in the least degree affect it.

I am, sir,
Your obedient servant,
A. P. W. PHILIP.

Cavendish Square,
Jan. 6, 1836.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

St. Thomas's Hospital Reports. By JOHN F. SOUTH, Assistant Surgeon. No. I. *Nov.* 1835.

Guy's Hospital Reports. No. I. January, 1836. Edited by GEORGE H. BARLOW, M.A.; and JAMES P. BABINGTON, M.A.

THE medical staff at each of the Borough hospitals has come to the determination of publishing "Reports;" and we are glad to find that the results of an extensive field of practice are thus likely to be open to the profession—that is, so long as such reports continue to be published; a period, however, which we suspect will not be very long, unless the undertaking be merged in the general concerns of the hospitals respectively, and the disbursements supplied from their funds. We do not mean by this to imply any unfavourable opinion of what the parties have done, or are capable of doing, but simply that the experience of the nearest approximation to such works, namely, the Transactions of the Medical and Chirurgical Societies of London and Edinburgh, afford no room for flattering anticipations of the result. The former of these was supported by the chief persons engaged in the present works; to whom were added not only their metropolitan brethren, but the practitioners of all England; yet we refer to the accounts of the society in proof of the fact, that they never have attained a remunerating sale. Again, as to the Edinburgh Transactions, they died a natural death, having reached only the second volume. We must hope that a better fate awaits the "Reports" before us.

With which hospital the idea originated we know not. The gentlemen of St. Thomas's appeared first in the field; and we have heard that on this their rival neighbours immediately took the alarm, and got up opposition "Reports." Be this as it may, we shall be perfectly candid and impartial between them, and, in fact, as will be seen, can bestow unqualified approbation upon neither.

In the Reports from Guy's Hospital (of which, though published last, it suits us to speak first), we have too many old cases—cases, indeed, which have been already published, particularly those of aneurism, not really by Sir Astley Cooper, as the advertisement led us to expect, but taken from Sir Astley Cooper's notes. It is true that accounts of two dissections are added by Mr. Cock, but the papers contain nothing else of any importance not to be found in Sir Astley Cooper's papers in the Medico-Chirurgical Transactions on these very cases; and we are reminded by their reappearance of a remark made by Sir Astley Cooper (in his paper in the fourth volume of the Medico-Chirurgical Transactions, page 434); viz. that

"The iliac artery has now been so frequently tied for aneurism of the femoral artery at the groin, that no useful purpose can result from the narrative of a case of that kind offering no uncommon circumstances."

Had the cases been recent, their publication would have been very right; but, under the circumstances, their introduction is, we think, injudicious. Now we have no doubt that our friends at Guy's will say this is very ill-natured; but if they will be advised by us, they will not give room even for a suspicion of paucity of materials by go-

ing back more than twenty years for cases, the chief details of which have been long before the public.

We also doubt the propriety of introducing into "hospital" reports so many cases which occurred in private practice, as we find in the brochure before us; this, at all events, is at variance with the appellation of the work, which ought rather to be called, "Reports of Practice in Guy's Hospital and elsewhere." Thus, in an interesting paper by Dr. Bright on diseases of the arteries of the brain, of eight cases given in illustration, *six* are from his private practice, *two only* having occurred in the hospital! Again, there is a want of statistical information. Indeed, we have no information as to prevalent diseases or general results, except with regard to midwifery by Dr. Ashwell, and revaccination by Dr. Babington. Of the general business of the hospital, as to admissions, recoveries, deaths, &c. &c. we have not a word. All these objections, however, so far as they may be found valid, will be easily remedied: meantime, we shall endeavour to farther the objects of the work by laying before our readers, from time to time, some of the most important observations it may contain.

Dr. Bright on Fever.

Dr. Bright's views have undergone very essential modification regarding the treatment of fever. We shall first give a case in illustration, and then proceed to some farther remarks.

" *Case of Simple Fever, protracted by Irritation of the Bowels, and attended by Relapse.*

Henry Grant, aged 14, was admitted under my care August 12, 1835, on the fourteenth day of fever. He stated that he was attacked with headache, shivering, and pain in his back, while at work; and the symptoms he described were, in all respects, those of a simple continued fever. At the time of admission, he was greatly oppressed with symptoms of acute fever; skin very hot, and covered with miliary eruption; pulse 130, feeble, but sharp. He said he had no heachache, but had a tendency to wander. His bowels were relaxed, and there was some tension of the abdomen; tongue dry, brownish in the centre, white at the edges; no cough.

Radatur caput, et applicetur Embrocatio communis. Habeat Sodæ Carbonatis, gr. x. ex Julepo Menthæ, sextâ quâque horâ.

13th. — Aspect improved; tongue moister, and less furred; skin pungently hot; pulse 120. He has not the least cough, nor any pulmonary symptom. Three watery yellow stools, sinking to the bottom of the vessel.

Adde Vini Opii, ♏iij. singulis dosibus misturæ. Applicetur Cataplasma Lini ampl. abdomini. Injic. Enema Amyli c. Syrupi Papap. alb. ℥ss. horâ somni, urgente diarrhœâ.

14th.—Pulse 108; abdomen less tender; motions very loose and gritty, but only two in number.

Rep. medicamenta.

15th. — Pulse 92; skin hot; one loose stool of the same character.

Rep. medicamenta.

17th.—Pulse 96; skin hot; bowels open as before.

Rep. medicamenta.

19th.—It appears that the injection has been omitted the last two or three nights.

Injic. Enema Amyli c. Syrupi Papav. alb. ℥iij. omni nocte.

20th. — Dejections still very loose; but a portion of the fæces has changed its character, and floats on the surface. Pulse 92; skin less hot; tongue gradually cleaning.

21st.—A slight feculent motion, with some tenesmus. Pulse 80, rather weak; skin temperate. He has been sick, after taking nourishment.

Habeat Sodæ Carbonat. gr. x. Spir. Lavend. comp. ℥ss. ex Julepo Menthæ, sextâ quâque horâ. Omitt. Enema.

22d.—Motions to-day nearly natural. He is better in all respects.

Rep. medicamenta.

24th.—Motions not quite so healthy.

28th.—Omitt. Cataplasm. Lini. Repetantur alia medicamenta.

Sept. 4th.—Habeat Infus. Cascarillæ c. Sodæ Carbonat. gr. xv. ter die.

He now appeared to be quite convalescent; and as he expressed a great de-

sire for animal food, a small quantity was allowed.

18th.—He has suffered a relapse; bowels relaxed; the dejections not natural; skin hot; pulse 120; tongue furred; some cough.

> Hydrarg. c. Cretâ, gr. iij. statim. Sodæ Carbonat. gr. x. c. Vini Opii, ♏iij. ex Julepo Menthæ, sextâ quâque horâ. Low diet.

> 19th.—Applicetur Cataplasm. Lini abdomini. Hydrarg. c. Cretâ, gr. ij. horâ somni. Rep. mistura.

22d.—The irregular and unnatural state of the bowels remains.

> Hydrarg. c. Cretâ, gr. ij. Pulv. Ipecac. comp. gr. iij. horâ somni. Rep. mistura.

28th.—He has improved decidedly, and his febrile symptoms are gradually diminishing; but he complains of a pain and tenderness in the left iliac region.

> Applicetur Emplast. Cantharidis parti abdomini dolenti. Habeat Liquoris Calcis, ℥iv. quotidie c. Lacte. Omitt. alia omnia.

He was kept enirely in bed, and strictly upon a mild milk diet, taking liquor calcis with his milk, till the 19th of October; during which period he took two doses of three grains of the hydrargyrum c. cretâ, each followed by two drachms of castor oil. His motions having now gradually acquired a healthy appearance, he was allowed two eggs and some beef-tea daily; and on the 30th he left off all remedies, but continued in the hospital a week longer, to protect him from the danger of relapse; and left it at that time, full of flesh, and healthy in his appearance."

Now, it will be perceived that the treatment here adopted differs from that of most practitioners, and from that formerly recommended by Dr. Bright himself; the difference consisting in the almost complete abandonment of mercurial preparations; for it will be seen, that during the first attack of fever not one particle of this medicine was used, and during the second only a few grains of the hydrar. c. cretâ. The difference between Dr. Bright's previous and present practice will, however, be best seen by contrasting parallel passages from his works. In his "Report of Medical Cases" Dr. Bright says,—

"In fevers in which the irritation of the lining membrane of the intestines is most prominent, we often have a relaxed state of the bowels from the very commencement of the disease: but more frequently the contrary has been found to be the case; and then it is a matter of the highest importance to remove any accumulations which may have taken place, and prevent them for the future. For this purpose it is necessary to have recourse to efficient purgatives; but the less irritation we produce, the more likely we shall be to afford permanent relief. If we can obtain the effect by calomel followed by castor-oil, we cannot possibly do better; or there are some advantages in the employment of a combination of calomel and rhubarb, which may render that a more convenient form of remedy."

Dr. Bright, in the Guy's Hospital Report, says,—

"Here, again, my practice has undergone modification; for it is but in very rare instances that I use calomel as the remedy to precede the administration of the castor-oil, and I still more rarely now make use of the calomel and rhubarb. As regards purgatives in the confirmed stages of fever, I almost entirely confine myself to small doses of hydrargyrum c. cretâ, varying from two grains to five, and followed, in about four hours, or on the next morning, by a dose of castor-oil, often limited to two drachms, seldom increased to half an ounce, and never exceeding six drachms, to which I very often add from two to six drops of tincture of opium."

We quite agree with Dr. Bright that this modification is "far from unimportant;" for in the one case we are told that "we cannot possibly do better than" use calomel with castor-oil or rhubarb, where those produce the desired effect; and in the other the author informs us that he follows the practice he himself recommended "but in very rare instances."

Again, as to the means of removing the irritation which is so often set up in the mucous membrane of the great intestines, even the hydra. c. cretâ is discarded as too stimulating;—witness the early part of the case detailed above; and still more strongly what follows:—

In the *Reports of Medical Cases*—"To obviate this state," I have formerly said, "I find that the combination of the hydrargyrum c.cretâ, the ipecacuanha, and

the compound chalk powder, in different proportions, is almost always the most applicable remedy; and, in many cases, I have scarcely used any other combination throughout the disease. Under this treatment, simple as it may appear, with the mildest nourishment, I have seen the stools gradually change their character, the febrile symptoms regularly retire, and a state of complete convalescence succeed to the most threatening symptoms."

But in the *Guy's Hospital Reports*— "Now, though I have had many opportunities of knowing that this practice has been followed by others, since the above was written, with great satisfaction in the result, yet it is upon this passage chiefly that the foregoing case is intended to form a comment and a stricture. I conceive that even this mild combination is very generally too irritating for the diseased intestines; and I have had reason to think it has, in many cases, kept up the irritation, or, at all events, allowed it to continue longer than would have been the case had a still milder and more soothing practice been adopted: and, in point of fact, I have for some years very seldom had recourse to this combination, except for temporary purposes, scarcely ever now continuing its use in the way I once recommended."

Dr. Bright need not have been apprehensive that the change of his opinions would be deemed as slight; to us they appear to involve a total abandonment of a principle—we mean that of the alterative effect of mercury in cases of fever. In the former passage we are told, as the result of observation and experience, that a certain remedy "is almost always most applicable;" and in the latter, he says that he "scarcely ever" uses it as he had recommended.

The opinions of Dr. Bright deserve attention, and cannot fail to be treated with respect; but we venture to ask whether, even in the case he has given, it may not rationally be conjectured that the recovery would have been more rapid (for the patient was not declared to be "quite convalescent" till the fifth week), and the subsequent relapse avoided, if moderate doses of mild mercurials had been administered. At all events, the case is not calculated to prove any superiority in the treatment adopted; and we think, that to justify so great a change, the results of comparative trials on a very large scale ought to have been [illegible] us.

For the mild mercurial plan Dr. Bright has substituted the administration of small doses of carbonate of soda (see the case above given), a practice which he attributes to Dr. Addison, but which is really nothing else than the "saline treatment" of Dr. Stevens. Where the bowels are very irritable, clysters consisting of a few ounces of starch or gruel, with from one to three drachms of syrup of poppies instead of laudanum, are strongly recommended. Having once more expressed our opinion that Dr. Bright greatly under-rates the power of cautiously regulated mercurials over fever, we shall conclude this notice with the following summary of his present views:—

"Thus, then, were I briefly to state the few rules and remedies on which I am accustomed chiefly to depend in that class, and that stage of fevers to which the present observations refer, I should say—Keep the patient as tranquil as possible, and, on the slightest indication of cerebral affection, whether primary or secondary, let the head be shaved, and a cooling embrocation be applied. In nine cases out of ten, supposing there are no obvious bronchial or catarrhal symptoms, great comfort, and great alleviation of the general febrile state, will be the result of this measuse. Let the only food be fresh barley-water, or toast-water; or if, occasionally, a little more support be required, arrow-root, and mild animal broth. Let the soda be given regularly; and watching the condition of the bowels, regulate the evacuations with the hydrargyrum c. cretâ and the castor-oil: if the bowels need restraint, use injections of poppy-syrup and starch: if abdominal tenderness come on, apply the linseed-poultice; or apply leeches, if the tenderness be urgent. In all cases, let the condition of the bladder be frequently ascertained, by placing the hand over the lower part of the abdomen: for, more particularly when the head is much affected, the bladder is apt to become distended, owing to the unconscious or powerless state of the patient; and this aggravates greatly all the cerebral symptoms. If the strength begin obviously to fall, the infusion of serpentaria, with a few grains of the subcarbonate of ammonia, is generally the best stimulant tonic: and when wine is administered, it should be with great caution, and measured by the ounce, not trusted to the judgment of attendants."

St. Thomas's Reports. No. 1.

We should have noticed this *brochure* before, had the copy (which *of course* was sent) ever reached us: as it is, having only procured it at our publishers after the preceding had been some days on our table, we have neither time nor space left to do more than say, that it is premised, by some judicious remarks on clinical instruction given by Mr. Travers, that it is less pretending, and a more bonâ fide "Hospital Report," than its rival. We shall recur to it, if we find any thing in it likely to entertain our readers, or deserving publicity.

A Series of Botanical Tables, with four coloured Medico-Botanical Maps of Europe, Asia, Africa, and America, shewing the geographical situation of all the plants in the Pharmacopœia. By W. K. Toase, &c. London. 1835.

That a work upon the plan of that here described, if ably executed, would be a useful companion to the student, can scarcely be doubted; but however much we may feel inclined to commend Mr. Toase for the idea of his work, we cannot extend our commendations to the execution of it.

The object of the work is, however, very different from what would be conceived by a perusal of the title-page,—it is to be learnt from the colophon at the end—viz. the table of fees which Mr. Toase expects from those whom he professes to assist in their studies previous to presenting themselves for examination at Apothecaries' Hall. Now we very much fear that the four preceding tables and maps will neutralize the last, inasmuch as an inspection of them, from the incompetence they exhibit, will inspire a doubt—nay, more than a doubt—as to the competency of the author to execute the matters stated in the last. Table No. 1 consists of a Key to the Linnæan System, with a corresponding arrangement of medical plants. In this table, of 198 names of plants, there are at least 20 which are incorrectly spelt. The author seems to make no account of the practice of all botanists of spelling the second or trivial name of certain plants with a capital,—we presume from ignorance of the reason of such practice. Old exploded names are retained, in the year 1835, which have been disused for a quarter of a century, while the locality assigned to others is not the most modern; nor are the localities assigned as the place of growth of certain plants, in the maps, more happy or conformable to the knowledge of the present day. Mr. Toase is, however, entitled to the praise of originality in some cases; for he has not only ascertained that there is such a plant as that called by Linnæus *Amyris elemifera*, but also that it grows in three different parts of the world, viz. in Arabia, in Brazil, and North America. Another of his discoveries, the merit of which no one will contest with him, is that the *Quassia Simaruba* grows in North America. But enough of this abortion. We would recommend Mr. Toase to circulate his advertisement apart from the tables and maps, which would be a more honest proceeding, inasmuch as he would thus not make others pay 4*s.* for it, and it might answer the object better,—as no prudent individuals, looking at the preceding pages, will resort to him for assistance in their studies.

MEDICAL GAZETTE.

Saturday, January 9, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

THE HUMANE SOCIETY AND ITS EXERTIONS.

We have read with much interest the report of the Committee of the Humane Society appointed to inquire into the circumstances of the late loss of life at the Serpentine river.

We had previously noticed several points in the evidence given at the inquest which seemed to require explanation. The chief of these points were—the time which elapsed previous to drawing the bodies out of the water—and, secondly, the number of persons actually employed by the society, and on the spot at the time of the accident.

At the Serpentine, seven lives were

lost out of fifteen in a state of suspended animation. The proportion is very large, when we consider what has been done on other occasions by the officers of the society; and if the result cannot be attributed to time lost before assistance was administered, or to a deficiency of hands at the receiving-house, we can only say that in this instance the efforts of the medical men were peculiarly unsuccessful and unfortunate.

Appararent death by drowning, we are aware, is a subject attended with much mystery and difficulty. The chances of resuscitation depend upon several circumstances connected with the period and the mode of submersion, the state of the drowning person, &c., which cannot be immediately appreciated, but which always render the prognosis more or less doubtful. Some worthy old authors have handed down to us histories of recovery where the submerged had lain sixteen hours, three days, and even seven weeks, in the water. But as we believe that the age of miracles did not extend to the seventeenth century, when these *facts* are related to have happened, and as we have reason to think that human beings were not differently organized formerly from what they are at present, we must respectfully decline giving credence to the marvellous histories aforesaid. We can believe, however, that there have been some rare cases of recovery where individuals have remained above an hour in the water. In the Paris arrangements for the resuscitation of the apparently drowned, *twelve* hours are taken as the *maximum*, beyond which there is no hope. Where it is ascertained, for instance, that a person has been eleven hours submerged, no means of probable succour are left untried: and the result of this practice is highly satisfactory. We learn from the official returns made by M. Marc, that of 1849 persons apparently drowned (between the years 1821 and 1826), 576 had been submerged for less than twelve hours; and of these 430 were saved.

But still more than the time of submersion, the mode in which it has been effected, and the state of the party at the moment, influence the chances of resuscitation. If submersion be preceded, or immediately followed, by apoplexy—as in the case of people falling into the water in a state of drunkenness—the instances of recovery are extremely rare. Not so where syncope occurs, as where a person faints with terror at the instant of submersion, or immediately before; for in this state, as is known, respiration may remain suspended for a period more or less considerable; and probably the greater number of those cases, which seem most anomalous, where recovery has been effected after a sojourn of two hours or upwards in the watery element, have been of this description. There are, besides, a number of other accidents which must enter into the calculation, if we want to estimate the amount of the chances of recovery: there may be sudden palsy, concussion of the brain, a shock received at the pit of the stomach, or the like, rendering all human assistance fruitless.

Yet, when the bodies are dragged up, there is no mode of distinguishing the hopeless from the hopeful: the mere appearances, unless when putridity is present, teach us nothing as to the chances of success. Some authors, it is true, have attempted to show the contrary: but perhaps there is, after all, nothing better known than what Hippocrates himself has told us,—that it is a fatal sign when foam is observed about the mouth of the apparently dead. M. Piorry in a great measure confirms this remark of the father of physic, in his valuable memoir on respiration, in which

he says, "Where there is no froth in the air-passages, the person not having respired between the moment of submersion and that of asphyxia, the case is very different from that in which there is froth—the result of struggling and agony in the act of drowning: the hope of rendering assistance in the former case is far greater than in the latter. There is reason to believe, that where a man has been asphyxiated without being terror-struck, has made no efforts to keep himself afloat, and, in fact, has not reappeared on the surface of the water after the first plunge, such a person will more readily be resuscitated than one who has sunk under different circumstances. It is even probable that where an abundant foam is found in the mouth and air-passages, recovery is impossible."

Making every allowance, however, for the difficulties of estimating the chances of success, we are still at a loss to account for the large proportion of lives lost on Christmas day, at the Serpentine. There must have been either time lost, or there was a deficiency of proper assistance. The drummer of the Fusilier Guards, who acted with such intrepid energy on the occasion, deposed, in evidence at the inquest, that much time was lost; that ten minutes elapsed before the first boat was brought up, and probably twenty minutes, or half an hour, before the third made its appearance. It seems probable, however, that the witness did not take a correct note of the lapse of time; he was immersed up to the shoulders in freezing water, while making his observations, and it is certain that the first boat which came to the succour (for there were four) escaped him altogether, being hid by the intervening crowd. The weight of evidence, as the matter now stands, is to the effect that not many minutes were lost in dragging up the bodies; that the first body, in fact, was in a warm bath in two or three minutes after submersion, and the whole fifteen were under treatment in no very long time.

Thus, then, we are necessarily driven to the inference that there was a lack of *medical* assistance in the work of resuscitation. It must not for a moment be supposed that we mean to impute the slightest deficiency of skill to the gentlemen employed, nor, we believe, can there be pointed out the least material defect in the means and appliances of the Society, in regard to the apparatus and things needful for the recovery of persons in a state of suspended animation. But the fact is undeniable, that here were *fifteen* bodies all at once, and under one roof, to be attended to by *seven* medical men. In this view of the case, perhaps, many will rather be surprised that so many as *eight* were recovered.

The conclusion at which we arrive, after a very deliberate survey of all the circumstances, is, that the means of the Humane Society were inadequate on the occasion of the late accident. For all *ordinary* emergencies, we believe the resources of the Society to be admirable, and to leave nothing to be desired; but in such circumstances as those occurring on Christmas day last,—when there were thousands of persons on the ice, and above seventy fell in when it broke—*extraordinary* arrangements should have been adopted, in order to provide against the possible accidents. What were *eight* or *ten* men (if there were so many), to guard the banks and to be ready with ropes?—and what, as we have just now observed, what were *seven* medical men when there were *fifteen* dying persons simultaneously to be attended to?

We have no fault to find with the Humane Society: it is an institution

beyond all praise. It is a glorious example of what the British nation is capable of doing through a pure impulse of benevolence. That it is not *all*-efficient is not to be wondered at: that it exerts itself, however, to the utmost extent of its means, is beyond a question; and as its managers, therefore, very properly say, "if the public expect more from us, the public must contribute more: no expense is spared, within the limits of our funds."

This leads us to another point, of no small importance. We want to know why, or on what reasonable principle, it happens, that government lends *no* assistance to the Humane Society? Not even on an extraordinary emergency, like that which lately occurred, and when so many accidents might have been anticipated, was there the least provision made by the Home Office for the prevention of dangerous casualties. A police force, we understand, was on the spot; but however efficient that body might be in the way of catching thieves, or removing the riotous, it could be of little or no use in saving persons from drowning, much less in rendering assistance in the work of resuscitation. Why have we not, particularly on such marked occasions, a regularly organized band of *secouristes*, under the direction and control of competent officers? Why is there no annual grant of money in aid of the funds of the society? The government surely ought to do more for a national body of this sort than merely giving it a charter, and the title of Royal. How it would excite the surprise of continental states, were they to know that our Royal Humane Society, whose reputation is European, receives not one farthing from the British government in furtherance of its benevolent objects!

On what a very different footing those things are managed (we will not, however, say *better* managed, in any other respect than as regards the co-operation of government) in other countries—especially in France—may be gathered from a circumstance of no very distant date. About two years since, the French Minister of Commerce and Public Works wrote to the Prefect of Police to inquire—whether their means of rendering succour to the drowned were as complete as they ought to be, considering the advanced state of science; and whether the institutions of other countries in this respect possessed any advantage over those of France? The consequence of this inquiry was the immediate reference of the question to a commission of the Council of Health, by whom, with the assistance afforded by the Foreign Ministers and other official functionaries, a body of information has been procured, and offered to the public, in the highest degree valuable. It is not, however, the actual result on which we set such a price, but rather the principle by which the French government was actuated in setting such an investigation on foot: it shows a praise-worthy interest on the part of the governing body in behalf of the governed. And so we hold it ought to be among us; there should at least be manifested a far greater care of the subject than our official people seem disposed to entertain.

We hope, however, not to be misunderstood. Great latitude must be allowed for the difference of manners between this and other nations; and that it is the feeling in this country not to be dictated to, nor interfered with, in matters of benevolence, is a thing of common notoriety. The government know it well: but, as it strikes us, they sometimes take advantage of it rather unfairly,—in fact, to do nothing, even where imperatively called upon to lend

their assistance. That such is the case in regard to the supply of adequate succour for the apparently drowned, we have already expressed our opinion: and we repeat, that though the Humane Society is abundantly and admirably adapted for all ordinary emergencies, it is unworthy of the government of a great nation like this, not to be prepared for extraordinary occasions, which may often arise in so great a metropolis, but to be dependent, for the safety of the liege subjects of the realm, on the resources of a private Society, which is without *power* to enforce its regulations, or *funds* ample enough to render it as efficient as it ought to be.

MEANS OF SAFETY IN THE MINES.

In our last remarks on this subject, it was mentioned that the Parliamentary Committee laid the chief stress on ventilation and the use of proper safety-lamps, as the best means of preventing the occurrence of disastrous accidents. We extract the following from the Report, as it contains some observations curious both in a historical and scientific point of view. The guarded and very tender manner in which the reputation of Sir Humphry Davy is handled by the Committee, is, perhaps, on the whole, the most satisfactory, as it is certainly the safest, mode in which a matter of so much national interest can be treated. Had Davy lived, we have no doubt but such modifications would have been adopted in his lamp as would have rendered it, what all the world is convinced now that it is not, a safe and efficient instrument.

"Your Committee have endeavoured to investigate with strict impartiality the merits of the different lamps which have been brought under their notice. In the course of the evidence many varieties will be found described. The invention claimed by the late Sir H. Davy, on principles demonstrated by that able philosopher, may be considered as having essentially served the mining interests of this kingdom, and through them contributed largely to the sources of national as well as individual wealth. Many invaluable seams of coal never could have been worked without the aid of such an instrument; and its long use throughout an extensive district, with the comparatively limited number of accidents, proves its claims to be considered, *under ordinary circumstances*, a safety-lamp.

"The attention of your Committee has been drawn by different witnesses to contingencies in mining, under which the lamp of Sir H. Davy ceases to afford adequate protection. Of the possible existence and nature of those contingencies, your Committee have ascertained that the inventor was well aware, and they regret that the cautions he gave to some of his immediate friends were not made more public. Accidents have occurred where his lamp was in general and careful use; no one survived to tell the tale how these occurrences took place: conjecture supplied the want of positive knowledge most unsatisfactorily; but incidents are recorded, which prove what must follow unreasonable testing of the security of that lamp; and your Committee are constrained to believe, that ignorance and a false reliance upon its merits, in cases attended with unwarrantable risk, have led to disastrous consequences. The proofs collected in support of this opinion may be considered as so many warnings to the miners of England. The prejudices which exist in many districts against the employment of the Davy-lamp are not occasioned by doubts of its protective character: the complaints made are of too little light, and the difficulty, in comparison with the use of the common candle, in bringing that light to bear with precision on the work, particularly in the thicker seams which are found in Warwickshire, Staffordshire, and other counties. Notwithstanding these prejudices, your Committee conceive that no employer of miners can be justified in allowing caprice, or inconvenience to certain individuals, to interfere with a due protection to the lives of his workpeople. In some mines, now lighted by the ordinary means, the use of the lamp ought, in the judgment of your Committee, to be compelled by the owners.

"Many improvements, calculated to lessen the number of dangerous contingencies already alluded to, have been suggested; all these may be considered as extensions of the principle: such are the lamps produced by Messrs. Upton and Roberts, Mr. Newman, Mr. Martin, Mr. Douglas, Mr. Wood, and Mr. Dillon. The lamps of Dr. Clanny and Mr. Ayres are provided with additional mechanical contrivances, intended to exclude danger which might overcome the safety principle, and at once warn the miner of the in-

security of his situation by the extinction of his light.

"In the experiments made before your Committee at the London University*, it may possibly be remarked, that the tests applied were not such, in nature or mode of application, as the known actual condition of the mines would point out as satisfactory. It must not be forgotten that the object of those experiments was to ascertain which of all the lamps produced was, when exposed to the severest trial, best entitled to the name of a safety lamp. In these experiments the explosion of the gases within the lamp was effected in every one, and similar explosions produced externally, save Messrs. Upton and Roberts's. Your Committee are therefore decidedly convinced that its construction possesses paramount merit.

"Your Committee cannot admit that these experiments had any tendency to detract from the character of Sir H. Davy, or to disparage the fair value placed by himself upon his invention. The improvements are probably those which longer life and additional facts would have induced him to contemplate as desirable; and of which, had he not been the inventor, he might have become the patron.

"With the sole exception of unexpected destruction of the instrument, Messrs. Upton and Roberts's lamp appears to your Committee to provide against all, or nearly all, the contingencies attending the Davy-lamp. Mr. Buddle states that tin shields, and a partial concealment of the lamp under their dress, constitute the prudential precautions taken by the miners in dangerous situations, to prevent the flame passing the gauze when the lamp is agitated. The glass chamber does all this with greater certainty; its sudden fracture leaves the instrument a perfect lamp on Sir H. Davy's construction. The introduction of the glass is not new; the novelties are, the shape of the glass, the collar which regulates the admission of air or gas to the cotton wick, and the double tissue of gauze beneath the wick, which prevents firing backward. If no practical objections are discovered, and your Committee do not contemplate any which may not be readily overcome, Messrs. Upton and Roberts's lamp will supply a grand desideratum, especially if extensive experience should prove that the lamp and area of the gauze may be so increased as to allow of more light with safety."—*Report: Accidents in Mines.*

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* See an account of those experiments, as performed before the Committee by Mr. Pereira, in the MEDICAL GAZETTE, vol. xvi. p. 664.

LECTURES

ON THE

DISEASES OF THE NERVOUS SYSTEM.

BY M. ANDRAL.

As published in the *Gazette des Hôpitaux*, with the approval of the learned Professor.

INFLAMMATION OF THE SPINAL MARROW.

The subject of Myelitis continued—Anemia of the Nervous Centres—Anatomical Characters—Symptoms—Treatment — Hæmorrhage of the Nervous Centres—State of the parts in and around the effusion—General state of the Brain—State of the Cerebral Membranes—Causes—Influence of Sex and Age—Affections of motion—Paralysis resulting from Hæmorrhage of the Cerebral Hemispheres.

Myelitis continued.—In the *Bulletin de la Société Anatomique* is related the case of a female who became paraplegic and died. It was supposed that the symptoms had indicated myelitis, but, on post-mortem examination, nothing whatever to justify this idea was found about the spine. The only morbid phenomenon was that all the bones were extremely brittle, and it was satisfactorily ascertained that the appearance of paraplegia might have resulted merely from the destruction of the head and neck of both femurs, which had taken place in such manner as to interfere with their movements.

Of late years an attempt has been made to generalize certain facts, and various writers, first in England, and afterwards in Germany, have described a peculiar condition, the seat of which is laid in the vertebral cord, under the name of spinal irritation, and which is supposed to display itself by producing disease in parts of the system more or less distant. The name of "Spinal Irritation" was proposed by Mr. Griffin, and adopted by M. Ems, who has also published on the subject. According to these authors, this condition of the spinal cord is not inflammation, but, as the name implies, an irritation, *sui generis*, which leaves no appreciable change of structure. According to these views, every part of the body is in relation or connexion with some portion of the vertebral cord, and pressure at this point would act upon the part connected with it so as to produce pain; and therefore, in order to establish their diagnosis, these gentlemen follow a course different from that usually adopted. Let a patient have cough, or palpitations, or gastritis—in fact, any thing—they begin by examining the spinal column, by applying pressure to

it with the hand, and they assert that when they arrive at the spinous process of a particular vertebra, pain is instantly felt in the part which is the seat of the diseased manifestation— in the epigastrium for instance, if it be the stomach which is deranged. It is required, in order to detect a disease by pressure on the vertebræ, that such part should receive its nerves from the spinal cord; they hold that many cases of headache originate in this same spinal irritation, and that various forms of deafness, amaurosis, morbid affections of the sense of smell, and even mental hallucinations, are attributable to the same cause; they even pretend that in some persons affected with blindness, pressure on the spinal column causes pain of the eye. The same observations apply to the chest, many of the diseased conditions of which are attributed to the spinal marrow; which, however, is much more feasible than the former. Neuralgia of the heart is regarded by those authors as usually deriving its origin from the spine, as well as certain painful conditions of the integuments over the sternum. Nevertheless, it is but the other day that a man presented himself to me, having an acute pain in this situation, and which was aggravated by touching the sternum or ribs. This patient had also habitual dyspnœa, but he was in other respects in good health, and had no lesion of the heart or lungs. I was not able, however, to obtain any of the results promised by these authors, whose opinions I am now submitting to scrutiny, and the whole theory may be regarded as an extension of certain well-known facts from which they have generalized prematurely and fallaciously.

Myelitis sometimes lasts but a few days, while in other instances it may endure for several years, and I have known so many as ten elapse between the supervention of the symptoms, and their fatal termination.

Myelitis may be cured, but the prognosis ought always to be guarded. The disease often proves fatal, and it may do so in various ways, viz. 1. By extending to the brain. 2. By the lesion of respiration. 3. By lesion of the heart's action. 4. The brain, respiration, and heart, remaining undisturbed, the patient may become gradually weaker, and lose each day something of the power which resists the disease; or sloughs may form, and life be slowly extinguished without any important functions being greatly affected.

The *treatment* is to be conducted on principles essentially the same as that of encephalitis. Leeches are to be applied in the form of a line along the whole course of the spine; cupping is also required. If the inflammation be chronic the leeches must not be laid aside, but revulsives are to be more particularly employed. *Douches* of different kinds have been recommended, such as the waters of Plombieres. Derivation to the intestinal canal by means of purgatives, is also to be practised.

ANEMIA OF THE NERVOUS CENTRES.

This disease is remarkable, inasmuch as it presents more than one trait in common with the symptoms of hyperemia, and yet it is very important to establish the diagnosis between the two affections, inasmuch as they require diametrically opposite methods of treatment.

The anatomical characters consist in discolouration of the nervous pulp, particularly the grey, which appears no longer to contain the vessels which are apparent in its natural state; its aspect approaches to that of the white substance; one would suppose it had undergone an actual maceration. Sometimes the anemia is attended with induration, sometimes with softening. It may be associated with a state of general anemia, in consequence of violent hæmorrhage, or it may succeed to chronic diseases, which have produced an impoverishment of the blood. It is not uncommon to meet with it after acute diseases, such as certain forms of gastro-enteritis, particularly in children. Sometimes it comes on without having been preceded by any disease which could have had an impoverishing effect upon the blood, but exists alone and independently of any other affection. If an animal be copiously bled, so as to produce death from hæmorrhage, we observe that in proportion as the brain becomes deprived of blood, convulsions supervene, and this symptom has also been met with in men dying from loss of blood. It may be established as a general principle, that the too great diminution of blood in the organs of the body is productive of disorder as well as is the opposite condition; with too little blood digestion is badly performed, or not performed at all, and palpitations occur in chlorotic patients as well as in those affected with hypertrophy of the heart.

Symptoms.—With regard to the intellect, delirium has been observed in a certain number of cases. M. Papavoine has published some cases in which children were rapidly carried off by an acute disease amidst violent delirium; on examination after death, the brain was found to be remarkably pale. This delirium is analogous to what arises from too spare a diet, particularly in infants and nervous persons. The same effect is produced if the brain be deprived of stimulants to which it has been accustomed. An individual addicted to the use of spirituous liquors, is put in prison, and there fed on bread

and water; he falls into a state of extreme weakness, becomes pallid, and has the nervous system deranged; he experiences insomnolence, disturbance of intellect, and some degree of delirium. So far from regarding these symptoms as dependent on congestion of the brain, the medical attendant restores to his patient his brandy and his former diet, and this has no sooner been done than his equilibrium returns, and the delirium disappears. A German physician named Osbund, has made some observations strongly corroborative of this.

With regard to motion, there may be convulsions, or various other disturbances.

The sensibility is increased in a very remarkable manner. Blisters or sinapisms applied to the skin, produce intolerable pain, and hence the precept not to have recourse to cutaneous stimulants in individuals who have been too copiously bled; in the same manner as it is imprudent to employ them when violent re-action is still present.

Treatment.—If by investigating the previous history and habits of the patient, we come to the conclusion that the symptoms are dependent on a state of anemia, the treatment must be founded on this circumstance. In patients arrived at a very advanced stage of typhus fever, in whom all that remains is nervous depression, the inflammatory symptoms having now disappeared, the further abstraction of blood could only be productive of evil, and the equilibrium becomes restored in proportion as aliment, and consequently strength, is cautiously supplied.

HÆMORRHAGE OF THE NERVOUS CENTRES.

Hæmorrhage of the nervous centres has long been known under the name of apoplexy; but this denomination cannot in the present state of science be regarded as synonymous with hæmorrhage; for, on the one hand, the symptoms of apoplexy may occur without there being any hæmorrhage, as in certain kinds of softening; and on the other hand, hæmorrhage may take place without the phenomena which characterize the apoplexy of nosologists. In a word, apoplexy is a term representing certain symptoms connected with very different conditions of the organ.

Hæmorrhage may have its seat in almost any part of the nervous centres. We see it most frequently in the hemispheres of the brain, and the part of those hemispheres where it is most common is that which is situated externally to, and on a level with, the optic thalami and corpora striata; it is also common in the two great ganglions themselves. The corpora striata and optic thalami may alone be implicated, but this is less common than it is to have the cerebral mass which surrounds them also affected. It may also happen that the disorder is seated just externally to these bodies, and leaving them intact. We also meet with hæmorrhage in the centrum ovale, and neighbouring points of the brain; in the anterior, middle, and posterior lobes. Sometimes, again, the convolutions only suffer, the rest of the cerebral substance remaining quite healthy.

Such are the different parts of the hemispheres of the brain where hæmorrhage has been observed; but there are other points of the encephalon into which blood may be effused. Thus, examples have been met with in the mesocephalon, or the prolongations which run from the protuberance to the hemispheres of the brain and cerebellum has been the seat of the extravasation.

The cerebellum is much less frequently affected with hæmorrhage than the brain, but this is occasionally seen either in the lateral lobes of the organ, or in that portion which has been called the median lobe.

The spinal marrow may be the seat of hæmorrhage at any point from its origin to its termination.

Sometimes the effusion takes place into the ventricles themselves, although much more frequently the blood which may be found in them comes from a laceration of the nervous substance surrounding them. M. Montault has related a case of hæmorrhage into the fourth ventricle. The septum lucidum may be ruptured, and an effusion which has taken place originally into the lateral ventricle of one side may pass into the other.

Hæmorrhage may take place into the membranes themselves, and it is to this form that M. Serres has given the name of *apoplexie meningée.* It is an extremely rare disease, and frequently, by a more careful examination of the brain, blood is found in its substance, which has made its way through the membranes by a kind of transudation. As to size, the hæmorrhagic effusions into the brain are sometimes very small, scarcely exceeding that of a pin's head, while at others they may give rise to one vast cavern occupying the hemispheres. With regard to their number, there may be but one point of hæmorrhage, or the brain may appear as it were riddled by an immense number of such deposits, either of the same date, which, however, is rare, or produced at periods more or less distant from each other, which is much more common than the former. Some effusions appear to take place only consecutively of others; thus, hæmorrhage into the cerebellum is rarely seen without the same affection of the brain, which renders the symptoms of apo-

plexy of the cerebellum obscure and difficult to recognize.

The colour of the blood is the same as that of hæmorrhagic effusions in other parts of the body; at first it is liquid, but afterwards acquires the appearance of jelly; finally it takes an almost solid consistence, and remains in this condition as long as it resists the absorbents.

There are two principal sources whence the blood is derived. 1. It is furnished by an exhalation from the capillaries, without the laceration of any considerable vessel; or 2, it comes from a vessel of appreciable size, the rupture of which has produced the hæmorrhage. The blood once effused, may increase in quantity, by continuing to flow from the same source which originally furnished it. The effusion may continue the same, or it may be reabsorbed; a very remarkable operation, which the physician may perhaps favour, but which he cannot excite. Complete reabsorption of the blood which has been effused leads to a radical cure of the diseases: a cyst is produced; the blood is surrounded by a cellular membrane, the internal surface of which exhales a serous fluid; this becomes mixed with the blood, divides it into numerous grumous portions, and thus facilitates its absorption. At length there arrives a time when the blood having entirely disappeared, or nearly so, there remains nothing in the cyst except a little serum, and cellular bridles running from one side of it to the other.

The blood being absorbed, the work of nature does not terminate here, and now an effort is made to effect the disappearance of the cyst; this has a tendency to become effaced, and having served its purpose, it leaves in its place only a linear cicatrix. All the stages of this reorganization may be traced, and it is particularly to modern writers that we are indebted for the most precise observations on this subject, among whom we may mention the names of M. M. Riobé, Rochoux, Cruveilhier, &c.; but we find in Bonnetus, Wepfer, and Morgagni, certain passages which clearly show that they had been aware of the process of cicatrisation of the cerebral tissue: witness the following explicit phrase:—" *Connivebat cavernula et jam coalescebant inter se parietes.*"

M. Foville has pointed out two modes in which cerebral hæmorrhage may take place. The labours of some anatomists, as you know, have led to the belief that the medullary matter may be unfolded in such manner as to show that it is formed of superimposed plates. M. Foville, taking this as a starting point, says that hæmorrhage may take place, 1st, in the midst of lacerated brain; 2d, into the interstice, between the cerebral plates above-mentioned, effecting their separation.

We have here to consider a difficult and interesting question. When the nervous pulp is destroyed can it be reproduced, or is the cicatrization constituted solely by the intervention of cellular texture, without the formation of cerebral matter? M. Serres thinks that in certain cases the nervous pulp is susceptible of a complete regeneration. The cicatrization takes a longer or a shorter time to be accomplished: thus, in some persons, a simple linear cicatrix alone is found within six months after the apoplectic seizure, whilst in others the cyst remains entire many years after the attack. In this case the process of cicatrization has been arrested. As to the state of the vessels immediately around the effusion, they may present no appreciable breach of their continuity, nor any other change, but they may also be the seat of various lesions, which act an important part in the production of hæmorrhage; such as ossifications, cartilaginous deposits, remarkable friability, &c.; or we may even find some vessel ruptured within the cavity containing the coagulum, or at some distance from it. M. Michelin has related a case of hæmorrhage into the optic thalamus, wherein a vessel running to join the choroid artery at the base of the skull was ruptured within the apoplectic cavity, the orifice being plugged up by a clot. The hæmorrhage is sometimes owing to rupture of the basilar artery, or the branches connected with it, and indeed there is no artery carrying red blood on the external surface of the brain, the influence of which may not be productive of hæmorrhage.

State of the parts in and around the effusion.—The nervous pulp may present various alterations surrounding or in the centre of the apoplectic effusion. Within the nucleus the cerebral substance may be wanting, or if present may be altered and softened; while in the midst of this softened tissue are found the cellular bands of which we have spoken. Is this softening preceded or followed by the hæmorrhage? Sometimes the softening has evidently preceded the effusion, as we find only some very small coagula in one vast mass of softening; and in certain cases hæmorrhage may be the means of breaking down the cerebral substance before it, this being merely a mechanical effect of blood being poured out in an organ having little power of resistance.

There are cases where close to the nucleus and within a fraction of a line from the point of hæmorrhage, the brain regains its natural state. Sometimes it remains injected, and more or less deeply coloured; in other cases it presents the appearance

of simple imbibition, without genuine injection of vessels; or the nervous tissue around shows an appearance of ecchymosis, and we may then have the different shades of colour peculiar to that state. Under more than one circumstance the softening may precede the effusion, as M. Lallemand has abundantly proved. It may also happen as a secondary consequence, either a very short time after the hæmorrhage, or not until a very remote period. If the effusion be old, we may find the nervous substance round the nucleus either healthy or variously discoloured, softened or indurated.

General state of the brain.—Great congestion is often observed, and a knowledge of this fact is important, because frequently the symptoms depend less upon the apoplexy itself than upon the cerebral congestion. In the process which follows the hæmorrhage, changes may arise which do not depend in any degree upon the extravasation, but upon the hyperemia. If the hæmorrhage be considerable, it may happen that the hemisphere which is the seat of the extravasation is not the only one to suffer, whether this arise from congestion of the general mass, or from the bursting of the blood from one ventricle into the other by the rupture of the septum lucidum; a circumstance which may act a more or less important part in the production of some of the symptoms which accompany apoplexy.

State of the cerebral membranes.—These may be completely healthy, or show appearances of congestion, or of an infiltration of blood. When the hæmorrhage has taken place long before, and a sanguineous nucleus remains, the membranes of the surface as well as of the ventricles may be filled with serosity, and in some cases individuals in their progress of recovery from the hæmorrhage have been carried off by these serous infiltrations of the meninges, or into the ventricles, the cause of death being, in fact, genuine chronic hydrocephalus. It thus appears, that in the disease of which we speak, we must keep in mind not only the hæmorrhage, but all the above circumstances which may be complicated with it.

Causes.—There is little to be said on this subject; the causes which produce congestion are the same, which being present in a more intense degree, or occurring in individuals otherwise predisposed, give rise to hæmorrhage; there are, however, some remarks relative to the etiology of this affection, which it may be proper to make. From certain investigations instituted with a view of determining the influence of different temperatures, it has been ascertained that at Paris, attacks of true cerebral hæmorrhage are more common during the winter than the other seasons. In a calculation founded upon 177 cases, I have arrived at a similar result; thus there were,

During Winter	60	Cases.
——— Spring	42	do.
——— Autumn	40	do.
——— Summer	35	do.

It has been inquired whether apoplexies have been equally frequent at different periods, and it would appear from the register compiled in London by Heberden, as quoted by Bateman, that from the beginning to the end of the eighteenth century, the number of cases of apoplexy had obverved an increasing progression. M. Falret instituted a statistical investigation into the number of cases of apoplexy which occurred between January the 1st, 1794, and December the 31st, 1823. The number proved to be 2297. The above thirty years, divided into three periods of ten years each, give the following proportions: first period, from January, 1794, to December, 1803, only 399 cases; second period, from January, 1804, to December, 1813, 979 cases (that is to say, nearly three times more than the first period) from January, 1814, to December, 1823, 919 cases, being rather less than the preceding period.

As to the causes derived from the abuse of spirituous liquors, and of too stimulating food, I have only to repeat what I have already said in speaking of congestion. Apoplexies have also been known to occur as the immediate consequence of violent mental emotion, or great bodily pain; as for instance, during the operation of lithotomy. Let me remark, however, that these moral perturbations and exquisite sufferings more frequently give rise to simple congestion than genuine apoplexy.

With regard to the digestive system, there must exist a predisposition, in order to enable its derangements to act any part in the production of apoplexy.

The influence which the different conditions of the apparatus of circulation have on the production of cerebral hæmorrhage is the same as with respect to congestion. Some authors, indeed, have endeavoured to make out that a considerable narrowing of the aorta beneath its arch had a great influence in the production of apoplexy. In order to decide this question, let us take certain cases in which there has been not only narrowing, but almost complete obliteration of this vessel. Now, of four such cases, in one only (published by M. Raynaud) did symptoms referable to the brain occur. This took place in a man 92 years of age, in whom there was hemiplegia of the right side, of very long

standing. On opening the body, numerous apoplectic cells were found. The patient had the head habitually hot, and with a sense of weight; the pulsations of the temporal artery were remarkably strong. On the other hand, nearly complete obliteration of both carotids has been known to accompany the occurrence of apoplexy, although the blood could only reach the brain through the vertebral arteries. This case, related by M. Cruveilhier, neutralizes that of M. Raynaud. We must not judge from an isolated fact, but carefully avoid precipitate and premature conclusions.

The venous circulation exercises the same influence in apoplexy as in congestion; only that an embarrassment of this portion of the circulation produces the former much less frequently than the latter; one does not easily produce apoplexy by holding the head down. It has been attempted to produce hæmorrhage into the brain in animals by ligatures which prevented the blood from returning to the heart through the veins, but this has not always been easily accomplished.

Plethora and the sanguineous temperament predispose to apoplexy, but less conspicuously than to hyperemia. As to the rest, apoplexy may take place in very opposite conditions of the system. Thus, cases have been met with in which persons became apoplectic just after the loss of a large quantity of blood. The thesis of M. Portal contains instances of this nature.

Influence of age and sex.—The particular kind of plethora of pregnant women has been adverted to, and cases of apoplexy quoted as having occurred during gestation and parturition; I regard these, however, as mere coincidences; and hold that we can scarcely enumerate pregnancy or labour amongst the causes of the disease. Apoplexy has been seen to occur in women some time after their confinement, but the cases of this kind given by M. Leloutre were certainly quite accidental.

With regard to the relative frequency of apoplexy in the two sexes, Joseph Frank says, "*Inter decem apoplecticos unam enumerare soleo fœminam*;" but this cannot be admitted, for it is obviously an exaggeration. Peter Frank found that, of 1241 patients who died of apoplexy at the Hospital of Vienna, between 1787 and 1804, there were 637 men and 604 women. M. Falret, however, has arrived, as the result of his researches, at a conclusion which comes nearer to that of Joseph Frank, having found 1670 men and only 627 women in 2297 cases of the disease.

As to the question of age, M. Falret, calculating from the number above mentioned, found that the period most obnoxious to apoplexy is that between 55 and 65 years, then between 45 and 55, and then between 35 and 45; while, in other periods of life, the disease becomes more and more uncommon; a result in keeping with the aphorism of Hippocrates, in which he points out the diseases most frequent in advanced life. (Aphor. 31, sect. iii.) M. Rohoux, on a calculation of 69 cases, has found,—

From	20 to 30,	2	cases.
—	30 to 40,	10	—
—	40 to 50,	7	—
—	50 to 60,	13	—
—	60 to 70,	24	—
—	70 to 80,	12	—
—	80 to 90,	1	—

The result to be deduced from these calculations is, that apoplexy is at its maximum of frequency during a period of fifteen years, commencing at 55, and finishing at 70. Thus, above 70 years of age the tendency to cerebral hæmorrhage diminishes; and it is also rare before 30: there are examples of it, nevertheless, between 10 and 15, and even between 5 and 10. And what is remarkable, one of the rarest forms of apoplexy, that affecting the cerebellum, has been seen in an infant seven years old. Some cases of apoplexy in patients from 5 years to 1, have been published by MM. Durnet and Tonellé. M. Serres has observed one case in an infant only three months old; and M. Billard saw another very shortly after birth.

The symptoms, to facilitate their study, must be divided into several series:—1st. Some depend upon lesions which may precede the hæmorrhage, such as a congestion, the symptoms of which are altogether independent of actual extravasation. It is to these that the names of precursory symptoms, hæmorrhagic effort, or *molimen hæmorrhagicum*, have been given. These symptoms may be entirely absent. 2d. Others depend upon the sanguineous effusion itself, whether it have acted by compressing or lacerating the cerebral substance. 3d. In another series of symptoms are ranged the phenomena which the alterations coincident with the effusion (such as softening, inflammation, or hyperemia of the brain) may have produced. 4th. We have those symptoms which became developed at a longer time after the occurrence of the hæmorrhage, as encephalitis, consecutive softening, &c. It will be perceived, however, from the above enumeration of these four series of symptoms, that many of them are not constituted by the disease itself.

Before we enter into a detail of the phenomena by which the disease of which we speak is announced, there is one ques-

tion to be asked: is there any external manifestation of such occurrence every time that an extravasation of blood takes place into the nervous centres? We must answer in the affirmative as so a great majority of cases; nevertheless there are examples on record, wherein considerable hæmorrhages into the brain have been unaccompanied by any appreciable symptom. M. Lenormand has related (Journal Hebdomadaire, tom. i. p. 435) the history of a woman, 30 years of age, who suffered under a hæmorrhagic diathesis: she frequently lost blood by the mouth, nose, and intestines; she had numerous hæmorrhages from the skin, which was covered with ecchymoses; and she had that pale yellowish complexion which announces excessive loss of blood. This woman died, and, on examination, the mucous membranes were found spotted by ecchymoses; besides which, there was an enormous effusion in both hemispheres of the brain. There had been considerable debility, but none of those symptoms which usually accompany even slight hæmorrhage on the brain. This case, and another which I shall mention by and by, ought to be kept in mind, in order to be associated with other analogous facts, should such present themselves.

Let us now examine the symptoms of hæmorrhage of the nervous centres, commencing with those which result from disturbances of the functions of relation.

Affections of motion.—There is one lesion of the motive functions, which, when it supervenes suddenly and remains persistent, is perfectly characteristic of hæmorrhage of the brain: this is paralysis; but, as I have just said, it is not sufficient that it should merely show itself, nor even that it should do this suddenly; it is further necessary that it should be persistent. This diminution of muscular power exists in different degrees, in almost every case, but not actually in all. We have seen the case of M. Lenormand, above-mentioned; and in another, published by M. Suretin, in 1829, he describes a patient who died without having had any paralysis; on opening the body, a clot, as large as a hen's egg, was found at the posterior part of the right cerebral hemisphere, having implicated the extremity of the corpus striatum. This case, and that of M. Lenormand, are almost the only examples of this nature which have been recorded.

Does paralysis, when once produced, always remain alike? It appears, from many facts, that paralysis may cease for a time and then return; which circumstance may be attributed to a kind of remission in the action of the effused blood upon the cerebral mass. With the exception of such cases, which, although not very uncommon, are to be regarded as exceptions, paralysis, as a general rule, only ceases with the reabsorption of the blood.

As to the period at which the paralysis makes its appearance, this coincides with the time when the blood is effused; it becomes increased, and suddenly acquires a great degree of intensity if a second attack of hæmorrhage be added to the first. The after course of the paralytic affection may be divided into four stages—first, that of its increase; second, that in which it is stationary; thirdly, that of alternate increase and diminution; fourthly, that of constant and gradual diminution.

The intensity of the paralysis differs. In some individuals we see but a slight numbness, with a diminution, though but inconsiderable, of muscular power. They grasp less firmly; so that if they have a cane or other object in the hand, they may perhaps let it fall: this is the slightest degree of paralysis. In a more marked degree, the movements are performed with greater difficulty, till at length comes entire loss of all power of motion. It may be that this diminution of muscular contraction has not been preceded by any other accident; and under such circumstances it marks the onset of the disease. Or there may have been some muscular weakness, arising not from hæmorrhage, but from a certain degree of congestion. Some disorders of the motive powers, such as convulsions or spasm, are occasionally met with, not depending upon the hæmorrhage, but upon the lesion which has preceded it.

This paralysis, so variable in degree, by which apoplexy is characterized, presents itself in different situations, corresponding to the situation where the effusion of blood takes place. I shall consider in this point of view the hæmorrhages of, 1st, the cerebral hemisphere; 2d, the mesocephalon; 3d, of the cerebellum; 4th, the different parts of the spinal marrow.

Paralysis resulting from hæmorrhage of the cerebral hemispheres.—This may be general or partial. It is general when it occupies both sides of the body at once. I speak here only of paralysis of the limbs, intending to advert afterwards to that of other parts.

Hæmorrhage of the hemispheres produces general paralysis, under the three following circumstances:—1st, when each hemisphere is the seat of effusion; 2dly, when the hæmorrhage, although but on one side, has been very considerable, and the irruption of blood has been sufficiently violent to destroy the substance as far as the corresponding ventricle, and to penetrate this, lacerating the septum lucidum, and producing strong compression of the

other hemisphere; 3dly, when the hæmorrhage, limited to one hemisphere, and not effecting an irruption into the opposite side, is yet sufficient to compress the other hemisphere of the brain, while perhaps there may be at the same time some congestion, which assists the generalization of the paralysis. In this state the four limbs, on being lifted up, fall down again quite inert. This paralysis may persist till death, or else, having been general at the commencement, it may at a later period become transformed into simple hemiplegia: this is met with, and is even very common, in the third form of hæmorrhage which I have pointed out. Paralysis of both sides at once indicates hæmorrhage much less certainly than hemiplegia does; the former is most frequently dependent on simple congestion. Paralysis of one side only constitutes hemiplegia; and this, when it comes on suddenly, and remains persistent, shows the existence of cerebral hæmorrhage. This point must be insisted on, because it is fundamental. When hemiplegia exists, it is most common for both limbs of the affected side to be paralyzed; but, nevertheless, it sometimes happens that only one is implicated. When both limbs are seized, it is very common to have at the same time palsy of the corresponding side of the face; and the antagonist action of the muscles of the affected side being destroyed, their opponents pull the mouth to the side which is not paralyzed. But although it be a law that the hemiplegia as well as the paralysis of the face takes place on the side opposite to the hæmorrhage, yet there is an exception as regards the tongue, of which I shall speak more particularly in the sequel.

It has been attempted to ascertain the cause of the paralysis and the effusion existing on opposite sides; and it has been attributed to the interlacing of the fibres at the upper part of the spinal cord. This explanation, which appears a very natural one, has been generally admitted; we must observe, however, that at the same time that the limbs of one side are paralyzed, the corresponding side of the face in many cases is similarly affected. Now the seventh pair, which gives movement to the face, becomes detached from the spinal marrow above this interlacing; and hitherto no fibres of this nerve have been traced beneath it. This fact is a great blow to the theory. However it may be, the general law as to the paralysis of apoplexy being on the opposite side from the effusion, must be regarded as established; remarking, however, that the explanation of this is yet to be discovered.

But there are various perplexing cases, which I shall mention, and which appear contradictory of this law. There are 14 cases which have been published, and 2 others which have not, making in all 16, in which the paralysis took place on the same side as the cerebral lesion. Let us rapidly examine those cases which are of unquestionable accuracy, and separate them from those which, being deficient in the necessary details, may perhaps be referred to the general law.

The first is very old, and is to be found in the work of Bonnetus. It relates to an individual who received a blow on the left temple, and was seized with paralysis of the right side; on examination after death, effusion was found on the right hemisphere. This case is certainly not of much value, for there had been external violence applied directly to the left side of the head, being that opposite to the hemiplegia. The second is more important, and is due to Forestus, whose observations are too little read. In this, softening was found in the right hemisphere of a child, and there had been hemiplegia of the right side. At the time when this case occurred, attention had already been directed to the law of *crossing* observed by paralysis; and Forestus takes good care to remark that he looked very attentively for some change on the opposite side, but could find no trace of any such; and let me remark, in passing, that the existence of *softening* was not altogether unknown to Forestus, since in this very case it is of "softening" that he speaks. Let me remark, also, that in the examples I am about to quote of direct paralysis, I shall not only speak of the hæmorrhages with which it has been coincident, but I shall include in my analysis all the cases with which I am acquainted wherein any anatomical change of the brain has produced paralysis of the same side of the body.

EFFECT OF MARRIAGE ON THE DURATION OF LIFE.

Some very curious facts on this subject are stated by Dr. Casper, in a paper of his lately published at Berlin. It had been long ago vaguely asserted that bachelors were less long-lived than married men. Hufeland and Deparcieux were of this opinion; and Voltaire observed that there were more suicides among those who had not given hostages to fortune than among those who had. Odier, however, was the first who set on foot the inquiry with exactitude, and he found (*Bibl. Britannique*, 1814) that, in the case of females, the mean duration of life, for the married woman of 25, was above 36 years; while, for the unmarried, it was but $30\frac{1}{2}$. At 30 there was a difference of four years in

favour of the married; and at 35, two years; and so on. It may be said, perhaps, that married females ought to be considered as *picked* lives; but, as Dr. Casper observes, this is far from being generally the case, especially in the middle and upper classes of society: it is chiefly among the lower orders, where a livelihood is procured by labour, that importance is attached to the bodily health and vigour of the female. With regard to men, we gather from Deparcieux's and the Amsterdam tables, that the mortality of those from 30 to 45 years of age is 27 per cent. for the unmarried, while it is but 18 for the married; and that for the 41 bachelors who attain the age of 40, there are 78 married men. The difference becomes still more striking as age advances: at the age of 60, there are but 22 unmarried men alive for 48 married; at 70, eleven bachelors for twenty-seven married men; and at 80, for the three bachelors who may chance to be alive, there are nine benedicks. The same proportion very nearly holds good with respect to the female sex: seventy-two married women, for example, attain the age of 45, while only fifty-two unmarried reach the same term of life. M. Casper, in conclusion, considers the point as now incontestibly settled, that, in both sexes, marriage is conducive to longevity.

NOTE FROM DR. MACLEOD.

To the Editor of the Medical Gazette.

Sir,

I request that the following error of the press, in my letter to you last week, may be corrected. In reference to the reports of the proceedings of the Phrenological Society, instead of "are furnished by a common reporter," read "are *not* furnished by a common reporter."

Your obedient servant,
R. Macleod, M.D.

Henrietta St., Cavendish-Sq.
Jan. 4, 1836.

NEW MEDICAL WORKS.

Remarks on the Unity of the Body, as illustrated by some of the more striking phenomena of Sympathy. By George Macilwain. 8vo. 6s. bds.

A Practical Treatise on Urethritis and Syphilis. By W. H. Judd. 8vo. with 23 coloured plates, 25s. bds.

British and Foreign Medical Review. No. 1. 8vo. 6s.

Brief Memoir of Sir William Blizard. By W. Cooke, M.R.C.S. 8vo. 3s. 6d. bds.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

January 7, 1836.

John Russell, Inerthyr Tydfill.
Thomas Baskerville, Canterbury.
Joseph Seaton.
Peter Norcliffe Roberts, Denbigh.
John Bunce Samuel.
John Keen Maurice, Smethwick.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Jan.* 5, 1836.

Abscess	3	Hooping Cough	4
Age and Debility	78	Inflammation	33
Apoplexy	9	Bowels & Stomach	4
Asthma	48	Brain	1
Cancer	2	Lungs and Pleura	11
Childbirth	3	Insanity	5
Consumption	85	Liver, diseased	4
Convulsions	41	Measles	4
Croup	1	Miscarriage	1
Dentition or Teething	11	Mortification	2
Diarrhœa	1	Paralysis	5
Dropsy	25	Rheumatism	1
Dropsy on the Brain	10	Small-pox	9
Dropsy on the Chest	2	Spasms	1
Erysipelas	2	Stricture	1
Fever	4	Thrush	2
Fever, Scarlet	5	Tumor	1
Fever, Typhus	4	Worms	7
Gout	1	Unknown Causes	1
Heart, diseased	5		
Hernia	1	Stillborn	21

Increase of Burials, as compared with the preceding week . . . 153

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Dec.	Thermometer.		Barometer.	
Thursday . 31	from 20 to 33		30·22	Stat.
Jan. 1836.				
Friday . . 1	25	33	30·21 to	30·40
Saturday . 2	7	29	30·53	30 52
Sunday . . 3	22	41	30·12	30·31
Monday . . 4	36	49	30·15	30·11
Tuesday . . 5	41	51	30·13	30·19
Wednesday 6	41	47	30·15	30·11

Prevailing winds, S.E. and S.W.

Except the 31st ult. generally cloudy; a little snow on the morning of the 1st and evening of the 2d inst.; a little rain on the mornings of the 4th and 6th.

Rain fallen, ·025 of an inch.

Charles Henry Adams.

NOTICE.

We cannot give insertion to the long rejoinder of Mr. F. Winslow to Mr. Munk. Any brief explanation we might have found room for, but not for a letter containing nothing important—except, perhaps, where the writer states in strong terms that he had no intention whatever to treat with disrespect those physicians whose characters he is charged with having attempted to asperse.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 16, 1836.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XVI.

I PROPOSE in this lecture to examine our common blistering beetle (the *Cantharis vesicatoria*), the only coleopterous insect employed in medicine in this country.

COLEOPTERA.

The order *Coleoptera* has derived its name from κολεαs, a *sheath* or *scabbard*, and πτερον, a *wing*, because, when the insect is not flying, the posterior is covered and protected by the anterior pair of wings, which are horny, and constitute what are termed *elytra*, or wing-cases. The animals of this order undergo complete metamorphoses, and have a masticating mouth, or at least visible mandibles and palpi.

Divisions.—Latreille divides this order into four sections, according to the number of the joints of the tarsi.

Sect. 1. *Pentamera* has five joints to all the tarsi.

Sect. 2. *Heteromera* has five joints to the four first tarsi, and at least one to the two last.

Sect. 3. *Tetramera* has four joints to all the tarsi.

Sect. 4. *Trimera* has three joints to the tarsi.

The only one of these interesting to us pharmacologically, is the second (*Heteromera*), which includes four families, namely, *Melasomata*, *Taxicornia*, *Stenelytra*, and *Trachelidia*. The last contains those animals in which the head is connected to the thorax by a kind of neck, and includes six tribes, one of which is called the *Cantharidia* or *Vesicantia*, embracing those insects commonly employed for the purpose of blistering.

Cantharis vesicatoria.

History.—It is difficult to determine at how early a period this animal was known or first used in medicine. It is, indeed, true that Hippocrates employed an insect which he calls κανθαρις, and that the effects which he, as well as other old writers, attributed to it, are such as we know our cantharis produces. But we must bear in mind that many coleopterous insects operate in a similar manner, and that the word κανθαρις merely signifies *a small beetle*, and would be equivalent to the Latin, *Scarabæus parvus*. Dioscorides speaks of several kinds of cantharides, but says the most powerful have transverse yellow lines on the wings, while those which are of one colour are weak and inert. Pliny makes use of the very same words. So that it is tolerably clear neither Dioscorides nor Pliny were acquainted with our cantharides.

The insect used for blistering in China, and some parts of Hindostan, agrees precisely with the characters mentioned by these old writers. It is called the *Mylabris cichorei*: in these specimens [showing some] you will observe on the elytra the transverse yellow lines or bands alluded to by Dioscorides and Pliny.

Synonymes and etymology.—It is to be regretted that entomologists are not agreed as to the zoological name of the Spanish fly: by Linnæus it is termed *Meloe vesicatorius*; by Fabricius, *Lytta vesicatoria*; by Geoffroy, *Cantharis vesicatoria*.

Several insects evolve a yellowish liquor (like honey), which is acrid, and even vesicating; and hence the term *Meloe* has been derived from μελι, *honey*. The word *Lytta* probably has reference to the supposed specific power of these animals to cure hy-

drophobia. I have already mentioned that *Cantharis* is derived from the Greek κανθαρις, a diminutive of κανθαρος, *a beetle*. As they were formerly brought to this country from Spain, they are commonly termed Spanish flies; it would, however, be better to call them *green blistering beetles*.

External characters.—As the animal undergoes perfect metamorphoses, I shall examine it in its different forms, beginning with—

1. The *ovum*, or egg.—This is of a sulphur yellow colour, of a cylindrical form, and slightly curved. In about three or four weeks the larva is developed, and lies almost straight in the shell, the two layers of which are sufficiently transparent to allow the dark parts of the animal to be seen through them.

2. The *larva* comes out of the shell, according to Brandt, with the hind part of the body first. It is of a citron-yellow colour, but is darker posteriorly. In twenty-four hours it changes colour, becoming brownish. It is formed of thirteen rings, and has three pair of feet. The head is flattened and rounded, and supports two short filiform antennæ. The mouth is formed of two maxillæ, furnished with four palpi. The nervous system presents more ganglia than in the perfect insect; but the stomach and biliary vessels are similar in both forms. The larvæ are very lively and active; and Zier says they are nourished on rotten wood and leaves of *Lonicera*, and on sugar. Applied to the skin, they excite itching.

3. The *pupa* state has not been described; in fact, the metamorphosis of the larva has not been observed.

4. The *imago*, or *perfect insect*, is the most interesting of all the forms, being that in which we employ the animal medicinally. In this state it has an elongated or almost cylindrical shape; its length is from six to ten lines (rarely longer); its thickness one or two lines: it has a golden green colour, and a nauseous unpleasant odour. The body is covered with whitish grey hairs, which are more numerous on the thorax than on the head, abdomen, or legs. The characters of the different parts are best studied under separate divisions.

(*a*) *Head.*—You will observe that the head is as broad, or rather broader, than the thorax, and inclined downwards; its shape is triangular or cordate: along the top there is a longitudinal furrow or depression. The eyes are large, and of a dark-brown colour; immediately in front of them the antennæ are attached,—they are filiform, and composed of eleven pieces or articulations, the two first of which are green and blue, the others black; the first is the thickest, the second the shortest piece. The mouth is composed of the same number of parts as other coleopterous insects; but there are some peculiarities noticed by Audouin. The mandibles terminate in a cutting edge, and have no teeth, though there is at the base a circular tubercle, somewhat resembling a molar tooth. The maxillæ are in part horny, in part membranous, and support palpi, each of which has four joints. The upper lip (*labrum*) is bilobed and narrow; the lower lip supports palpi, which are shorter than those of the maxillæ, and have only three joints.

(*b*) The *thorax* presents no characters essentially different from those of other coleopterous insects. The protothorax is not so large as the abdomen. The elytra are flexible, from four to six lines long, and from three quarters to one line and a half broad. On their upper surface they are of a beautiful golden or cupreous green colour, but beneath are brown: when viewed by transmitted light they are also brown. On their upper surface we observe three longitudinal ribs or veins, one of which is often confounded with the outer margin; and on examination by a magnifying glass we observe wrinkles. The wings are very thin, membranous, veined, transparent, and of a pale brown colour. The feet are from four to six lines long, smooth, and thin: there are five joints to the first pair of tarsi, and four only to the last. Audouin has described the following peculiarity connected with these parts: in the female, two small moveable spines are found at the junction of the leg with the tarsus, on all the limbs; whereas in the male this arrangement is found only on the two posterior pair, the anterior pair having but one spine; which, however, is strong, compressed, cutting, and placed on the median line. In this pair of legs, in the male, we observe a peculiarity not met with in the female: the first joint of the tarsus is hollowed out, so that when the spine is applied to it a hole is found. Now in the act of coitus, the male seizes the antennæ of the female, and locks them, as it were, in this hole. The last joint of the tarsi of all the feet, both of males and females, is furnished with a pair of claws, each of which is split, and forms in reality two.

(*c*) The *abdomen* is very soft; in the female it is broader than in the male. Superiorly it is of a greenish violet colour; at the sides near the stigmata it is black. Near the anus of the female are two articulated caudal appendages, somewhat similar to palpi.

Internal organization.—Let us now examine the internal structure of these animals, commencing with—

1st. *The nervous system.*—In this, as in other insects, the nervous system is two-

fold; namely, one part analogous to the cerebro-spinal axis of vertebrata, and another regarded as a kind of sympathetic system.

(*a*) The part which we may call the *cerebro-spinal axis* of insects, consists of a double nervous cord, running along the ventral surface of the body, and re-united at intervals by ganglia. In the cantharis there are nine ganglia; namely, two cephalic, three thoracic, and four abdominal. The first cephalic ganglion, or the bilobed brain, gives off the optic branches, the nerves of the antennæ, and two branches which may be regarded as the commencement of the single sympathetic system: posteriorly two twigs connect the brain to the second cephalic or subœsophageal ganglion: the œsophagus is placed behind the brain, before the second ganglion, and between these two twigs, so that the nervous system forms a kind of collar around it.

(*b*) The *sympathetic system*, or the nervous system of the œsophagus and stomach, has been recognized in the cantharis, both by Audouin and Brandt. It consists of two portions, a single and a double.

The *single* sympathetic system is composed of a single nerve. It commences at the two branches from the brain, as before mentioned. These, at their union, form a triangular ganglion (*ganglium frontale*), from which the nerve proceeds along the surface of the œsophagus to the stomach, where it divides into two, forming a small ganglion at the point of its division.

The *double* sympathetic system consists of four ganglia placed on the œsophagus, immediately behind the brain, two on either side of the single nervous cord just described, with which, as well as with the brain, they are connected by nervous twigs.

2. The *circulatory system* is formed by a simple pulsating dorsal vessel, extending from the head to the extremity of the abdomen.

3. The *respiratory system* is composed of ten pair (three thoracic, seven abdominal) of small openings, called spiracles, or *stigmata*, placed on the sides of the body. These open into the air-tubes, or *tracheæ*, which ramify and are distributed to all the organs of the body.

4. The *digestive system* must be next noticed. The before-described mouth terminates in the pharynx; this contracts into a long muscular œsophagus, which terminates at the metathorax, in the elongated fusiform stomach. On the external surface of this last-mentioned viscus we observe a number of transverse bands, formed by its muscular coat. At its termination in the intestine is a valve formed by the union of several small floating kidney-shaped bodies, attached by their external sides. The small intestine arises abruptly from the stomach, and is at first tolerably large, but soon contracts: it forms two or three curvatures, and then proceeds directly backwards. The last portion of the intestine being somewhat swollen, is considered as the cæcum, which terminates in a very short and narrow rectum.

The biliary or hepatic vessels consist of six very long, filiform, convoluted tubes, terminating anteriorly at the stomach, near the pylorus, and posteriorly at the intestine near the cæcum.

I may here notice the *adipose tissue*, or *rete*, of the cantharis. The function of the part which bears this name in insects is imperfectly known; some have regarded it as a kind of liver. It envelopes the alimentary canal, and is usually of a white or yellow colour.

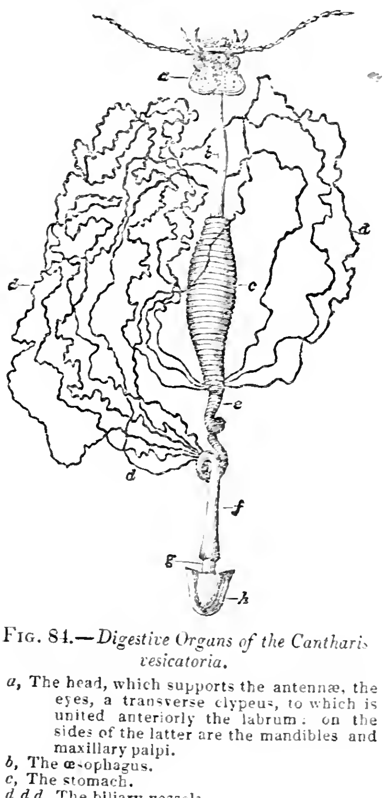

FIG. 84.—*Digestive Organs of the Cantharis vesicatoria.*

a, The head, which supports the antennæ, the eyes, a transverse clypeus, to which is united anteriorly the labrum: on the sides of the latter are the mandibles and maxillary palpi.
b, The œsophagus.
c, The stomach.
d d d, The biliary vessels.
e, The intestine.
f, The cæcum.
g, The rectum.
h, The last ring of the abdomen.

5. *The sexual apparatus.*—In the *male* we find a pair of spherical testicles, granulated externally; two vasa deferentia, which have a ringed appearance, and enlarge as they proceed towards their termination; three or four pair of tubes, the functions of which are imperfectly known; a common spermatic duct; and a penis. The three or four pair of tubes just alluded to are described differently—Brandt saying they are three, Audouin four pair: the latter writer calls them seminal vesicles, while others term them epididymoid vessels. The penis has three barbs or hooks at its extremity, and is enveloped by a sheath composed of two pieces, each hooked at its termination.

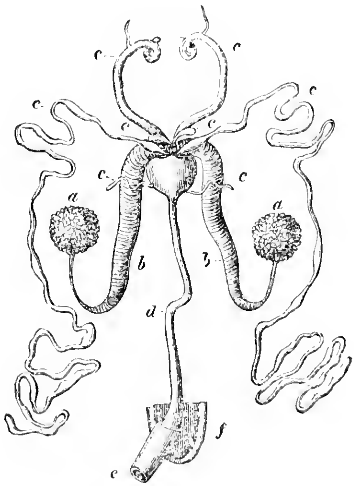

FIG. 85.—*Male Genital Organs, from Audouin.*

a a, Testicles.
b b, Vasa deferentia.
c c, *c c*, *c c*, *c c*, The four pair of vesiculæ seminales, or epididymoid vessels.
d, The common spermatic tube.
e, Portion of the intestinal tube inverted.
f, Last abdominal ring.

The *female* organs consist of two large, hollow, egg-shaped, ovaries: the cavities of which are called *calyces*. On their external surface is placed an immense number of fusiform egg tubes. From each ovary (or calyx) arises an oviduct; and the two ducts joining form the common oviduct, or egg canal, the lower portion being termed a vagina. Into this common oviduct passes a tube from a vesicular bag, called *spermatheca* (the *vesicule copulatrice* of Audouin), and also of other appendages (called sometimes sebaceous glands.)

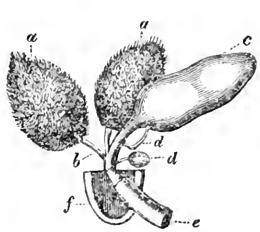

FIG. 86.—*Female Organs of Cantharis vesicatoria.*

a a, The ovaries covered by the egg tubes. Each ovary sends out an oviduct, *b*. The two ducts unite to form the common oviduct, which receives the excretory tube of the *spermatheca*, *c*, and of other appendages, *d d*.
e, Portion of the inverted intestine.
f, Last abdominal ring.

I must refer those who feel interested in the amours of these animals to Audouin's "*Recherches*," in the "*Annales des Sciences Naturelles*." Suffice it here to say, that the male loses his penis, and shortly afterwards dies.

Geography.—It is probable that this insect was originally a native of the southern parts of Europe, especially Italy and Spain. It is, however, now found in France, Germany, Hungary, Russia, and other parts of Europe. As its food is vegetable, it is found on trees, particularly those belonging to the families *Oleaceæ* (as the ash, the privet, and the lilac), and *Caprifoliaceæ* (as the elder, and several species of Lonicera.) It is easily recognised at a considerable distance, by its disagreeable odour, which is exceedingly unpleasant, and even dangerous. It occasions public walks and promenades, near which it may be found in large quantities, to be deserted.

Mode of catching them.—In the south of France these animals are caught during the month of May, either in the morning or evening, when they are less active, by spreading large cloths under the trees, which are then strongly shaken, or beaten with long poles. The catchers usually cover their faces, and guard their hands by gloves. Various methods have been recommended for killing the insects; such as exposing them to the vapour of vinegar (the practice mentioned by Dioscorides), or of hot water, or of spirit of wine, or of the oil of turpentine. Geiger states, that if destroyed by dropping oil of turpentine into the bottle in which they are contained, they are not subject to the attack of mites. But I believe they are more frequently destroyed by immersing the cloths containing them in hot vinegar and water, and then drying on hurdles covered with paper or cloths.

Preservation.—The active principle of cantharides being volatile, they ought to be preserved in glass vessels well stoppered, as by long exposure to the air their activity must be in some degree impaired.

Cantharides are liable to be attacked by several insects, which reduce them to powder. These are, a species of mite (Geiger says the *Acarus domesticus*), a moth (*Tinea flavifrontella*), and two coleopterous insects, namely, the *Anthrena museorum*, and *Hoplia farinosa*. Notwithstanding what Limouzin-Lamotte has asserted to the contrary, it seems well established that worm eaten cantharides are less active than those which have not been attacked, and that the deterioration is in proportion to the number of mites present, the dust and excrements of which are not vesicant. Various proposals have been made to guard our blistering flies from these parasites. According to Farines camphor will not answer. Some recommend placing little bags of the chloride of lime in the vessel containing them; or dropping into the bottle a little spirit of wine, or oil of petroleum, or pyroligneous acid.

Commerce.—Cantharides are imported from St. Petersburgh in cases, each containing 160 or 170 lbs.; and also from the port of Messina, in Sicily, in barrels or cases holding about 100 lbs. The largest quantities are brought over towards the latter end of the year. By reference to the Trade List published at the Custom House, I find that the quantity of cantharides on which the duty of one shilling per pound has been paid is as follows:—

	lbs.
For the year 1834	20,085
For the year 1835	17,468

However, it must be remarked that this statement refers to the ports of London, Liverpool, Bristol, and Hull only; but it will give a tolerably good notion of the quantity annually consumed in Great Britain.

Varieties.—In commerce the cantharides from Petersburgh are considered the best; they are larger and more copper-coloured than some which I have received from France; the latter being greener and smaller.

Chemistry.—Very little advantage can be gained by discussing the notions entertained by the old chemists, respecting the active principles of cantharides. Suffice it to say, that by some the operation of these insects was referred to the sharp, needle-like, and pointed form of their particles; by others to an imaginary acrimonious salt; by Neumann to resin; by Thouvenal to green wax; and by Beaupoil to various constituents, namely, to a green oil, a yellow and a black matter. The following tables shew the results of Thouvenal and Beaupoil's analyses:—

Thouvenal's Analysis.

1. Extracted by water, a reddish, yellow, bitter extractive	216
2. Extracted subsequently by alcohol, a green, acrid, odorous wax	60
3. Extracted subsequently by æther, a yellow wax	12
4. Insoluble residuum, or parenchyma	288
	576

Beaupoil's Analysis.

		Gros.	Grs.
1. Black matter, insoluble in alcohol, but soluble in water		1	2
2. Yellow matter, soluble in water, alcohol, and æther		1	2
3. Green oil, insoluble in water; soluble in alcohol and æther		1	8
4. Parenchyma	Animal matter	4	36
	Phosphate of lime	0	12
	Carbonate of lime	0	2
	Sulphate and muriate of lime	0	4
	Oxide of iron	0	2
5. Acid (phosphoric ?)	quantity undeterminable.		

In the year 1810 Robiquet announced the discovery of a new principle, to which the active properties of the blistering beetles were referrible, and to which Professor Thomson, of Glasgow, applied the name of *Cantharidin*. The other substances which Robiquet obtained from these animals he found had no vesicating property.

Robiquet's Analysis.

1. *Cantharidin.*
2. Green fixed oil, soluble in alcohol.
3. Fatty matter, insoluble in alcohol.
4. Yellow viscid substance, soluble in water and alcohol (osmazome?)
5. Black matter, soluble in water, insoluble in alcohol.
6. Yellow matter, soluble in æther and alcohol.
7. Free acetic and uric acids.
8. Phosphate of lime, and phosphate of magnesia.

I may remark that there are probably other principles in cantharides than those just mentioned. The strong and fœtid odour of these insects, no doubt, depends on a *volatile oil*; and Orfila is of opinion that this oil is one of the active principles. Odier has obtained from the elytra, and other parts of many coleopterous insects, 25 per cent. of a substance which he has designated *chitine* (from χιτωὶν, a coat, or envelope); because it forms the basis of the hard parts. Though cantharides are not particularly noticed, yet they must possess it in common with other beetles. Lastly, it would appear there must be a *colouring matter* unnoticed in Robiquet's analysis; for Chereau has found that the green and gold matter composing the elytra is insoluble in water, alcohol, æther, and oil.

Let us now examine the most important constituents of Spanish flies separately.

1. *Cantharidine.*—I have already mentioned that this substance was discovered by Robiquet, in 1810. In addition to the name it usually bears, it has also been termed *vesicatorin*, or the *vesicating principle of cantharides.* In Leopold Gmelin's *Handbuch der Theoretischen Chemie,* it is called "*cantharidcn camphor*;" being classed among the solid volatile oils, which some of the German chemists denominate camphors (Berzelius terms them *stearoptenes*, Bizio, *steurisin*). This principle is not peculiar to the cantharis vesicatoria, but has been found both by Dana and Robiquet in the *lytta vittata*, or potatoe fly of America, and by Brettonneau in the *mylabris cichorei.* Most probably it (or some analogous substance) exists in all blistering insects.

Extraction.—There are two methods of procuring cantharidine; one proposed by Robiquet, and another by Thierry.

Robiquet's process. — (*a.*) *Prepare a watery extract* by repeatedly boiling roughly powdered cantharides in water until nothing soluble is left. Filter and evaporate to the consistence of a soft extract. A brown mass is by this means obtained. I would here remark, that, by the boiling, a portion of the cantharidine must be volatilized; for the vapour strongly irritates the eyes and affects the kidneys and bladder, causing pain in the loins, with frequent desire to pass the urine.

(*b.*) *Digest the watery extract in alcohol,* which takes up the active part. Robiquet calls the insoluble residuum a *black* matter; but I find it to be, when moist, dark brown, and when dry, light brown. The alcoholic tincture is to be distilled until it has acquired a thick consistence. Robiquet calls the residuum a yellow substance: that which I obtained has the appearance of treacle.

(*c.*) *Digest the alcoholic extract in æther.* The slightly-coloured solution thus obtained is allowed to evaporate spontaneously. By this means a yellowish brown extract is obtained, intermixed with very small crystals.

(*d.*) *Wash the ætherial extract with alcohol,* which dissolves the colouring matter, leaving the cantharidin. This is to be removed, and dried between folds of blotting-paper.

This process is very expensive, and gives very little product.

Thierry's process. — Macerate cantharides in alcohol, æther, or a mixture of the two (*alcohol éthéré*), for several days; then distil the tincture. The residuum, when cold, consists of two layers: the lower one a brown liquor, the upper one a green oil; on the surface of which are seen crystals of cantharidin. These two liquids being separated, the oil is to be placed on a filter, and, by the assistance of a gentle heat, it passes through the paper, leaving the cantharidin. To purify the latter from any oil, press it between folds of blotting-paper, and dissolve in boiling alcohol: as the liquid cools, the cantharidin is deposited.

The *properties* of cantharidin next deserve attention. This substance crystallizes in the form of micaceous plates, which are fusible, forming a yellow oil, which by a still stronger heat is vaporizable, forming white vapors, which condense into cantharidin in small needles, unchanged in its blistering properties. Dana regards it as an organic alkali, but without any just grounds; for it will not restore the blue colour of litmus paper reddened by an acid. Gmelin's opinion, that it is a solid volatile oil, seems to be correct. When isolated, it is not soluble in water, but, combined with the other constituents of cantharides, it is; the yellow matter probably being the principal agent in rendering it so. This, then, is the reason why an aqueous infusion of the flies contains cantharadin in solution. Cold spirit, digested on can-

tharides, extracts cantharidin; which it can only do by the agency of some of the other principles of the flies. It is easily soluble in æther, oils (volatile and fixed), and hot spirit of wine; and from the latter it separates as the liquid cools. Two or three practical points here suggest themselves, to which I shall have occasion hereafter more particularly to direct your attention. 1st. The solubility of cantharidin in oils, points out the impropriety, and even danger, of using these liquids as antidotes, and also why a blistering plaster acts equally well through paper or muslin. 2dly. In medico-legal inquiries, connected with cantharides, we may employ æther to extract the vesicant principle (that is, the cantharidin). Concentrated boiling sulphuric acid dissolves cantharidine: the solution is slightly brown; when diluted with water, it deposits small needle-like crystals of cantharidin. Boiling nitric and muriatic acids dissolve it without changing colour; the solution, by cooling, deposits it. Cantharidin is dissolved by potash and soda; but when concentrated acetic acid is added, it is precipitated. Ammonia is without action on it.

No analysis of cantharidin has yet been made.

2. The *odorous volatile oil* is asserted by Orfila to be one of the active principles of cantharides. I have already mentioned that cantharidin is to be regarded as a solid volatile oil; and it is not at all improbable there may be also a liquid one. That there is a volatile odorous matter is certain; but the doubt is as to the activity of it on the system: if it be active, it is curious that its action is of the same kind as cantharidin. If water be distilled from cantharides, we obtain a strongly odorous milky liquid: the vapour of this will affect the eyes like cantharidin, and also the kidneys. Now I suspect that this active volatile oil, or toxic principle of Orfila, is merely the volatile cantharadin combined with some odorous matter. That cantharadin is volatile, even at low temperature, is proved by the following fact. One of Robiquet's pupils, who was watching its crystallization, felt acute pain in the conjunctiva, which was followed by inflammation accompanied with small phlyctenæ, and loss of sight for many days. Robiquet, who was not so near, suffered but slightly.

This volatile odorous oil of cantharides is regarded by some as the principle to which these animals owe their action on the nervous system, and some have added the urinary organs also.

The cantharadin and odorous oil of Spanish flies appear to reside principally in the sexual organs. Both Farines and Zier tell us, that the soft contain more active matter than the hard parts. It appears also that the posterior is much more acrid than the anterior portion of the body; and Zier says the ovaries are particularly rich in this active matter. If so, it is evident we ought to prefer large females to males. It is a well-known fact that the odour of these animals becomes much more powerful at the season of copulation than at other periods; and that persons sitting under the trees in which these insects are, at this season more particularly, are very apt to be attacked with ophthalmia and ardor urinæ.

3. *The other constituents* of cantharides will require a very brief notice.

a, The *black*, or, as I should call it, greyish brown, matter left behind by digesting alcohol in the watery extract, though soluble in water, is not so in alcohol. Robiquet proved by experiment that it had no vesicating property.

b, The *yellow*, or brownish, *osmazome*-like substance is left undissolved when æther is digested on the alcoholic extract. Berzelius says it is an extractiform substance reddening litmus paper, and appears to contain lactic acid.

c, The *green fixed oil* is obtained by digesting alcohol on cantharides which have been repeatedly boiled in water. A green tincture is by this means obtained, and by spontaneous evaporation the alcohol is got rid of, and a green non-vesicating oil left behind. Barbier states that a drachm of this had no effect on dogs.

d, The other fatty constituent of cantharides is obtained by allowing the ætherial tincture of these insects to evaporate spontaneously; then wash the oily residue with hot alcohol, to dissolve the cantharidin. This fatty oil is distinguished from the green one by its insolubility in alcohol.

e, The yellow substance soluble in æther, and separable from the impure cantharadin by alcohol, has not been much examined.

f, The free acid found in the aqueous infusion, or distilled water of cantharides, was taken by Beaupoil for phosphoric acid; and it is not I think improbable that a portion of this acid may be present; but Robiquet has shewn the existence of acetic (or is it lactic?) acid even in the living flies; and in fresh flies uric acid also. In noticing this last substance, Robiquet remarks that it is curious that these insects, which have such a marked action on the urino-genital system, should present in their composition many points of resemblance to urine. The metallic brilliancy is probably in part owing to the mechanical arrangement of the constitu-

ents, and is, therefore, to a certain extent, independent of their nature.

Tests for cantharides.—On many accounts it is highly desirable that we should have some chemical means of recognizing cantharides both in substance and in solution. I much regret such means are still wanting. You will find in Orfila's Toxicology the effects of various chemical re-agents on solutions of cantharides. In my opinion, none of the results yet obtained are worth much; and I say this after having examined the action of a large number of substances on these flies. That this is also Orfila's opinion is evident from the following remarks:—"If the characters furnished by the re-agents should differ from those I have described, the practitioner must pay no regard to them in deciding on the presence or absence of cantharides, for animal substances present in the different menstrua very complicated phenomena, which are little understood, and consequently liable to lead into error." In the absence, therefore, of chemical proofs, we may resort to the following:—

1. *Zoological characters.*—It is very unlikely, however, that the flies will be met with in a sufficiently perfect form to judge of their zoological characters.

2. *Physical characters.*—In all powders of cantharides you may distinguish golden green particles; these may be separated from the other contents of the stomach by immersing them in boiling water: the fatty matter rises to the surface, while the cantharides powder falls to the bottom. Orfila has recognized these particles in a body nine months after interment; so that they do not readily decompose even when mixed with decaying animal matters. Recollect that there are many insects which have the same golden green colour, but are without vesicating properties; and *vice versâ*, there are many insects which vesicate, but which have not a golden green colour.

3. *The symptoms* hereafter to be mentioned.

4. The *lesions* also to be spoken of in the next lecture.

5. The *physiological effects* of the suspected matters. In some cases this is the only mode of detection that can be adopted. The best method of proceeding to determine these effects is the following:—If the suspected matter be a liquid, evaporate to the consistence of an extract; then digest in repeated quantities of sulphuric æther. The ætherial solutions are to be mixed, and allowed to evaporate in the air: the vesicating properties of the residuum may be determined by applying it to the inside of the lip or to the arm. If the suspected matter contain solid particles of cantharides, these will be recognizable in part by their colour, and in part also by digesting them in æther, and proceeding as just described. In the *Annales d'Hygiène publique* for April, 1835, you will find an illustrative case, in which Barruel demonstrated the non-existence of cantharides in a suspected liquor, and the presence of powdered cantharides in some suspected chocolate.

Adulteration.—The goodness or quality of cantharides may be recognized by their odour, and freedom from insects of all kinds. Sometimes the powder, but more commonly the plaster, is adulterated with powdered euphorbium. I have been informed by persons well acquainted with the fact, that it is a common practice amongst certain druggists to mix one pound of euphorbium with fourteen pounds of powdered Spanish flies. I regret that I have no means of detecting this adulteration in the plaster, except by observing its diminished vesicating power. I have on several occasions strongly suspected this practice, by finding that the plaster from certain druggists did not vesicate so powerfully as that which I knew to be genuine. If you digest alcohol on powdered cantharides mixed with euphorbium, and then add a few drops of this tincture to a glass of water, there will be a greater milkiness than if no euphorbium were present; but it is not a test on which much reliance can be placed. I have been informed that the dregs of the tincture of cantharides bottle have been sometimes used in the manufacture of blistering plaster.

CLINICAL LECTURE

ON

COMPOUND FRACTURE OF THE FEMUR.

Delivered at the Middlesex Hospital School, Jan. 4, 1836.

By Sir Charles Bell.

Gentlemen,—I wish I could speak to you familiarly and confidentially. You may perceive that what I am addressing to you goes abroad to the world; and instead of speaking to the half-educated, (if you will permit me to say so without offence) my unpremeditated words, when printed in the journals, appear to be ambitiously addressed to the learned part of the profession. I beg, then, to say they are not.

But this is the misfortune of the times in which you enter upon your studies, that every thing is printed; and it becomes an injury to the profession, and

especially to the schools, because it is placing in the hand of the journalists the character and reputation of the teacher; giving this power to men who may, or who may not, be the best judges of what is right and sound in doctrine, or honourable and correct in conduct.

There are professors acknowledging that their receipts have been trebled through the influence of the press: this is a new thing among us. The professors, or the teachers, of London, now marshal themselves to go up to the Ministers of State, who are understood to be in the act of imposing laws on the profession, and these gentlemen say, "Look you here, what a school we have at our back, and therefore give us privileges—make us a college." Had I the ear of these Ministers, I would certainly pray them to look to the recent means of obtaining popularity, and the consequent degradation of the schools of the metropolis.

You are the sufferers in this state of things; for instead of the teacher recollecting what he was when in your circumstances, remembering the errors and the misconceptions which he entertained at your age, and therefore bringing himself back, if you will allow me the expression, to your condition, he is thinking of another tribunal altogether, and you lose the advantage of that friendly footing that you ought to be on with the gentleman under whom you enter in the hospital, and especially the free and unembarrassed communications on the questions of practice. However, we must endeavour to forget all this, and enter the best way we can upon our duty.

Once more, then, I call your attention to one of those dreadful accidents which bear upon the great questions of surgery. You are not unprovided with books on stricture, fistulæ, and lues, for these works are published daily; but they do not touch upon the great principles of surgery, which ought to be with you at present the paramount objects.

Observe, gentlemen, that there is some force of imagination necessary to the study of surgery. "What!" you say, "poetical imagination?" No; but it is necessary, by force of imagination, to anticipate much—to place yourselves in that condition which, sooner or later in your professional career, you must be in; this is almost as necessary as it is to have a precise notion of the condition in which your patient shall be, for without it what avails the aphorism of practice, since you cannot apply it? By anticipating the terror and confusion of friends who crowd around a patient who has received some terrible accident, you are prepared to act your part,—which is, to be collected whilst others are distraught and palsied with terror—which is to be decided while every one else is jostling in ineffectual efforts to afford relief. This is your professional character; not a mock gravity, with the head of the cane at your chin, but a firm and manly aspect, and full of resources which you have long contemplated, and have anticipated the occasions for putting in practice. More especially in regard to the question which is to occupy us to-day, it is necessary that you should have a distinct notion of the case. You must not only know the nature of the wound, but you must have studied the effects of a severe accident on the constitutional powers—how the patient will be found lying hardly alive, pale, breathing anxiously, incapable of speaking, his pulse feeble, and his extremities cold. When you have formed, I say, by force of imagination, a correct notion of the patient's condition in such kinds of wounds, you have gone far to determine the question of what is proper to be done. I give you this as an instance why you ought to be looking forward in anticipation of scenes in which you are to be engaged.

I wish to draw your attention, then, to the case of a lad with fracture and laceration of the thigh, close to the hip-joint. At four o'clock on the morning of the 24th December I am brought here, and I find a lad desperately hurt. There is a deep wound in the groin and thigh; there is a great ragged flap formed of the integuments on the inside of the thigh and adductor muscles; there is a more irregular wound, more bruised, on the outside of the thigh; there is a separate wound just below the groin, and in front of the thigh; the femur is broken, and its upper portion stands out three inches, and its sharp extremity is cocking directly up out of the wound. There has been great hæmorrhage; the tourniquet is around the upper part of the thigh, but it is not now screwed up. The lad is very pale; the pulse feeble and easily compressed; the extremities cold; the breathing anxious. Fourteen hours have elapsed since the accident. It is one of the accidents from the railway; the heavy sharp edge of a wheel has passed over the upper part of the thigh, near the groin. You will, of course, at once, in these circumstances, anticipate the determination of the consultants, that amputation must be performed.

But before I proceed to consider this case, and all the questions that arise out of it, I wish you to regard these observations of mine not as speculative, but as the observations of one who has seen, perhaps, as much as any man can boast of—if it be

a thing to boast of at all—certainly as many cases of desperate fractures of the thigh-bone as to enable me to have an opinion on these cases. The late Inspector-General, Mr. Knight, employed me in giving lessons of anatomy to the young surgeons going out to our armies abroad; and if the pupil and brother of John Bell had not been excited early to attend to the questions of military surgery, this was an occasion which called for him to be particular in studying that department; in fact, I lost no opportunity of doing it.

During our long war, after the engagements of our ships at sea, or of our armies, our men were brought home in bands, and lodged in hospitals here, and I assiduously attended to them, examined their wounds, heard their narratives, and sometimes assisted at operations. Then came the disastrous battle of Corunna: history now tells you that the men who were wounded were carried to the transports without arrangement, in different vessels from their surgeons, and consequently they came to the shores of England just as they left the field. On that occasion I left lectures and practice (which people would say I ought to be most interested in), to wait the landing of these men. I marked their condition, followed them from the strand to the hospital, attended assiduously upon them there, and studied the rules which decided the practice of the army and navy surgeons; for both were in attendance.

On some occasions I doubted the expediency of the practice; and after-experience confirmed my objections. Another occasion offered in the battle of Waterloo. When I heard of that great battle I set off but ill prepared, and reached the field of our operations several days before the surgeons despatched by government. There was, indeed, sufficient field for observation. The newspapers said 30,000 were *hors de combat*. I can only tell you that they lay by thousands. I was engaged with my note and sketch books until I found that numbers were brought into Brussels in the most desperate condition, and without surgical assistance. There was no blame: the regimental surgeons had marched on with the army to Paris, and the wounded of all nations were brought in hourly. I could not, therefore, deliberately study the individual cases any longer; I was forced to offer my services; they were accepted, and I laboured with these men for three successive days and nights with little intermission. I mean by this, gentlemen, first to impose upon you my conviction of the importance of the subject, and then to convince you that I am not speaking here as a speculative person sitting at home, but that I have witnessed all the difficulties the army surgeon has to contend with, and have felt the responsibility of acting in the varieties of cases the most desperate.

After all there is not so much difference as you might imagine between the practice of the hospital and the military surgeon. Gunshot wounds, certainly, is a subject of itself, in so far as you must take up distinct principles when you have to speak of a ball, a round or obtuse body sent with incalculable velocity through the body—the thigh, for example. This is undoubtedly a distinct subject, but it is not the question with which we have to do to-day. When the sides of a ship are struck with round shot, and splinters scattered; or when cannon-shot tear up the gabions and the temporary breast-works in the field; or when men are wounded by the bursting of shells, the great questions of surgery present very much as they do in the hospital—as, for example, in this lad. Such wounds I have long since considered; and it may some time or other become my duty to my office, if not to myself personally, to take a leaf out of some of the volumes of Military Surgery, and to place it in my portfolio for lecture; and if I be challenged, I will just say as a northern Duke did in respect to his boroughs, "may I not do what I choose with my own?"

In the case of this lad the operation is determined upon; and when you contemplate the patient in the exhausted condition I described, I think the first idea that will occur to you is the correct one, as it is the natural one—that it is our duty to save him, as far as possible, from a second shock. It was with this idea that I placed the amputating knife within the irregular flap which was on the inside of the thigh, and carried it round on the outside, inclining the knife, and cutting as far as I dared venture, making a flap there also. That this attempt was imperfectly accomplished was owing to the bruised condition of the wound. The whole mass was then drawn back with the split cloth, but could not be so effectually drawn back as you might imagine, being so high in the thigh; and the bone was sawn across. I then looked to the ragged face of the stump, and those parts which hung out I cut clean off. You might have observed, that as I took hold of the muscle, and cut it across with the sweep of the knife, the lad felt no pain, and there was no additional disturbance from this practice. The artery bled freely although the tourniquet was applied; and upon this I shall make some remarks presently.

It is our duty to avoid, if possible, in these desperate circumstances, a second shock to the constitution. Now we use

that term metaphorically. No doubt there is such a thing as a mechanical shock—as, for example, when a man, standing firmly upon his feet, has a limb carried off by a cannon-ball. There is then a great mechanical shock to the whole nervous system, such as you are familiar with in respect to the brain in concussion. That most admirable surgeon, Mr. Cline, mounting a young horse, was thrown, and on his recovery he found that he had received a shock all over, and said, "Now I recollect the observation of John Hunter, that to kill an eel, you must not tread upon its head, but you must take it by the tail and smack it upon the ground, and it dies, because the shock is then given to the whole nervous system." He added, "I feel that the shock was not in my head, but over my whole body." There is certainly such a thing as a shock, a vibration, a mechanical influence over the whole body; but when I use the term *shock* now, I do not mean that, but an influence upon the life. It would be in vain to endeavour to ascertain what this was, unless we knew something more than we do of the life itself. We see, however, that great pain, that the lopping off of a large portion of the body, produces an influence upon the living powers. Neither the brain, nor the heart, nor the nerves, nor any thing you can mention of vital parts, are hurt, yet the constitution, or the powers of life, suffer. You know, for example, that in the case of the lower animals, you may cut them into portions, and yet they survive. But when you come to animals higher in the scale, and especially man, you find you cannot cut off a quarter of the body, and yet leave the body in life and health. Whatever this influence may be, it is that I now mean. When a whole limb, almost a quarter of the body, is cut off, however dexterously, however suddenly, whether removed by an engine or by the surgeon's knife, there comes to be an influence upon the life of the remaining body. The greater part of the thigh having to be taken away, instead of putting the knife round and cutting through all the parts again, I put it within the flap, and brought it along the original wound, so as to diminish, as it were, the severity of the second operation—the second shock upon the system; because I think every thing else yields in importance to this. The patient is in that condition, that if further and extreme violence be done, and if, for example, the whole parts are cut through again, he will probably sink under the operation.

But you will say that this flap was a lacerated wound; and we find it stated in our books of surgery "that contused and lacerated wounds must suppurate or slough." I do not hold that opinion to be correct. I have found a lacerated wound adhering most kindly. I have seen a man tossed by a bull, the horn going in at the side, and coming out at another hole, at which the bowels protruded; the bowels were reduced, and the lacerated and torn wound healed kindly by the first intention. So upon other occasions, when people have fallen on spikes and been torn, I have found a lacerated wound adhere just as well as if it had been made by a surgeon's knife; and accordingly, in the case of this lad, on the fifth and sixth days of dressing, you saw no sloughing.

I then put the flaps together, and dressed the stump; but this practice is objected to: it is at least a contested point. I consider it in this light: here is a large open surface; you look around for something to apply to that surface—something soothing. Surely you would like something warm, and soft, and moist; and if it were possible, something alive, that there might be sympathy between the surfaces. What, then, is better than the opposite flap of the same limb?—for by this two objects are gained: the most soothing application that you can put to this extensive lacerated surface, and probably, in addition to this advantage, adhesion; and if there be adhesion, just in proportion to its extent there is a diminution of the excitement and inflammation; and so much is removed of that influence which you dread on the constitutional powers. What, then, can be the meaning of a rule of some surgeons, not to bring the flaps together, lest they should adhere, but rather to dress them separately? Undoubtedly it arises from this; that if a man has his limb amputated in the field, in the hurry of passing from tent to tent in the rear of the army, and if the surgeon puts on strapping, dressing, compress and roller, and the patient be left, perhaps, for a whole day, or indeed much longer, there arise great pain and serious mischief: the stump swells, with pain, spasm, inflammation, and fever. What does this proceed from? Not from the manner of dressing, but from neglect,—necessary neglect, it may be. You have only to mark the necessity of an early visit; that if you have operated on a patient, and dressed him, brought the surfaces and the edges into contact, applied the straps, nicely put down the compresses, brought your cross cloth and roller over the face of the stump, and made all comfortable and firm, you impose upon yourself the duty of visiting the patient in a few hours, and asking him if there be pain and tension of the stump. If there be pain and increased

throbbing, you must loosen your cross straps,—you must cut across some of the rollers,—you must even divide the plaister straps, or you will have tension, following that inflammation, and great distress,—perhaps spasm. But when such mischief ensues, it is owing to the neglect of the evening's visit, and not to any thing wrong in the operation or dressing.

Again, some others say, I shall not dress this patient till two or three hours after the operation. There is reason in this; and we have a parallel case when a bone is broken. A patient comes into the hospital with a fractured limb; perhaps there is a new house-surgeon, who has not been attentive to his previous duties as a dresser. He rolls up the limb as soon as he is called, and applies the splints, and over these splints the bandages. The man suffers in the evening in an excessive degree; and if he be left till the morning there is vesication between the bandages, and perhaps mortification of the foot or hand. The reason of this is, that the young surgeon has done up and bandaged this limb before the coming on of the tumefaction. To save appearances with the patient's friends, take the trouble of explaining what will follow, and show your care for the limb by placing it properly, and defending it against any accidental movement; but see that you do not bandage before tumefaction has begun, and, indeed, before it has subsided; or if you must make a show of *setting* the limb, as the vulgar expression has it, mind that you return to divide your bandages, and free them under the increasing tumefaction. Now this is a parallel case. If you have that sort of patience, and if you have time to spare, you may wait and dress the stump in the afternoon; but if you dress it immediately, you must recollect that tumefaction from the stimulus of the knife must come on, and you must manage accordingly. Such I apprehend to be the reason why great inflammation and bad suppuration come on by immediate dressing, and therefore it is that some of the continental surgeons will not bring the flaps together,—will not take advantage of the two soothing surfaces meeting together,—will not take the chance of a certain portion of adhesion which is in some measure to stop the inflammation, but put in *charpie* between the surfaces, and allow suppuration to be established before they bring the flaps together.

Some might have thought in this case of taking off the limb at the hip-joint. I hope and trust that the desire of distinction by doing this operation has passed away with the novelty of it. I remember going round my collection in the museum of Windmill-street, many years ago, with Assalini, the celebrated Italian surgeon, to whom you are indebted for some neat surgical instruments, and who was certainly one of those who thought more of the operations of surgery than of the great principle we are considering. We came at last to a diseased bone—the femur—when I found my Italian friend expressing himself in a language familiar to all nations, but suprisingly so to the natives of the South. He paused; he stood erect, put the palms of his hands together, and mounting his eyes, said, "Ah! I let the opportunity pass of taking off a young man's limb at the hip joint!" I remarked that it was just as well that he had not; that his conscience was just as clear as it might have been then. Now that disposition pervaded all the surgeons of the continent; they had a notion, that by dexterous execution with the amputating-knife, they would establish an unfailing character for surgery. That is what I mean when I say that I hope the desire for distinction has passed away with the novelty of the case. Now would that lad have been in as favourable circumstances had I cut out the bone? Here was a return to the principle under consideration, viz. the extent of the injury to the powers of life. The surfaces must have been very extensive indeed, the wound longer, and the pain protracted. Then, to dissect the head of the bone from the articulation, is easy enough to any man educated in a dissecting-room; but how far does that consideration influence the question as regards the patient? The time spent in the disarticulation, and the extent of surface exposed, would, I believe, in this case, have been attended with death: I have not a question about it. I do not deny that a case may present in which the operation shall be required. A portion of bomb-shell may so shatter the head of the bone, and tear away such a mass of the thigh, that we can do no other than dissect the articulation, in order to make a flap: that case, however, is a very desperate one. But I venture to say that there is no form of musket-shot, nor is there any condition of diseased bone, that will make a case for the amputation at the hip.

I hope you have looked at this boy whilst he was dressed. I hope you looked to the state in which the wound is found about the seventh day; for that is ten times more important to you than any thing which could come from my lips.

You observe that the amputation was an irregular one; and in these cases it is necessarily irregular. You may take the general principle of saving skin to cover the muscle, and of saving muscle to cover

the bone; but when you come to a case of this kind, you must apply the principle differently from what you do in the circular amputation;—you must find the skin where you can, and the muscle wherever the mass is; but what I wish you to attend to is, that as a large portion of the surface in this case was made by the original wound, it presents to you now an expanded efflorescent mass of granulations. It does not exactly represent, but it reminds me of those cases which I saw that came from Corunna. When the limb has been carried off, and the stump left without assistance, in seven days the whole is as a part of this large stump, red and soft, with exuberant granulations. Such was the condition of the stump of Sir William Maxwell, whose arm had been carried off by a cannon-ball. There was extraordinary efflorescence, a cauliflower-like excrescence of the interior of the ragged flaps, the muscles were granulating, and the detached bones were adhering to the muscle. This, however, is the effect of the want of pressure, of proper dressing. You must attend to this, giving sufficient pressure without bringing your patient into that condition of pain and excitement which comes from tight bandaging. In dressing this boy the difficulty is to bring forward the mass of flesh, and to give it sufficient compression, so as to prevent it from expanding and efflorescing in the manner I have described.

I think that even in your day, in your experience in this hospital, we have found, that after dreadful accidents, giving occasion to amputation, our patients have sunk about the fifth day; so let me hope that this lad has passed that period of danger—I mean the danger from the shock and the rising inflammation on the constitution: if he sinks now, it will be from the extensive discharge and debility.

The next point in this case to which I beg your attention is, the remarkable erection of the bone,—it stuck straight up. As anatomists, you know how this must happen. As we sit there is a great weight upon the femur; and not only so, but all the posterior muscles are counteracting the anterior: but when the bone is broken, the weight that should depress it is gone, and the posterior muscles are detached at their insertions. There is a lesson to be learned here. The dresser will tell you that he has the greatest difficulty in pressing down the stump: as he presses it down, the muscles and integuments cover the end of the bone; but as he ceases to press, the bone jumps up again. Here is the demonstration [alluding to several specimens of fractured thigh-bones]. I think, by the by, that it has been in anticipation of this remark that they have placed these bones before us to-day, and I am very glad of the hint. I once made up a case of twelve fractured thigh bones, in all of which the upper extremity was cocked up; and you see in this specimen that the upper portion is above the lower portion of the bone. I did not know that we had so many examples of fracture. The fact is important, because it is a clear demonstration to you of the necessity, in amputation, of raising up the thigh, and keeping the saw horizontal whilst you saw it across, otherwise you are deceived. The bone appears at first pretty deep among the flesh, but you no sooner let the stump loose than up it goes, and the bone starts out from the flesh. The principle is, of course, applicable to the fracture of the thigh. You try in vain to keep down the bone; and failing, you may have distortion and retraction. You see, then, the object in the inclined plane; the inclined plane allows you to raise the lower part of the limb, and to bring it into that position which the muscles give to the upper part.

The next reflection regards the artery, and the application of the tourniquet. The tourniquet was applied here high in the thigh, but I was sure that it would not be effectual; and the fact is, that when I cut across the artery, you saw that it bled freely. Why then, you will ask, did I wish the tourniquet to be applied at all? Would it not have been better to have trusted to the assistant's thumb to compress the artery at the groin? Yes, in regard to the artery this would have been effectual; but still there would have been blood lost, which this youth could ill bear; for, as you have seen, the lad was pale and cold, and low, and must have sunk had much blood flowed. By compressing the groin you would have commanded the main artery; and when it was cut across there would have been no jet from it; but, in the meantime, the blood would have been poured out from the face of the stump, coming round by the vessels of the hip, the gluteal, the ischiatic, and obturator arteries.

Remember that the notion has been that the pressure you use at the groin is ineffectual, because, on proceeding to the operation, the artery bleeds notwithstanding. But it does not bleed because it is uncompressed, but because of the freedom of the inosculation coming round; and therefore, if at any time you are to operate by compressing the artery, your assistant's hands must be large and powerful to grasp all the flesh of the top and back part of the thigh, otherwise your patient will lose a great deal of blood. I was wont to give an instance of the freedom of inosculation here; but I happened this morning to be in consultation with Sir Astley Cooper and Mr. Copeland: the subject was, the tying

the main artery, to suppress the growth of a tumor,—when Sir Astley gave us an instance in point. A tumor had formed upon the leg; and he had taken up the idea, that by tying the main artery of the thigh the vascularity of the tumor would be so diminished that it would fade. The operation failed to have this effect, and amputation was necessary. Amputation being performed below where the artery was tied, the blood, notwithstanding, spouted out directly from the artery, as if it had never been tied. Now that exactly corresponds with a case which I saw under the hands of Mr. Lynn, of Westminster, the first year that I came to London. The artery had been tied for aneurism; amputation became necessary, when the blood jetted out from the main artery as if no ligature had been previously placed upon it. When, then, you are operating very high in the thigh, you can, if you choose, compress the artery at the groin, but do not trust to it; either have the tourniquet, as I had, round the hip, upon the back part of the thigh, so as to compress the collateral vessels, or see that the hands and fingers of the assistant are long enough and strong enough to compress the whole limb, and more especially the arteries which come round from the back of the pelvis.

You see here a large open wound, and no blood flowing from it. This you understand to be the character of a lacerated wound. No doubt the patient had lost much blood, and therefore the force of the circulation was diminished; but yet you will observe that the moment I cut off the surface of the muscles, that moment the arteries began to bleed; and therefore it is clear that stemming of the blood was not altogether from the weakness of circulation, but from the nature of the wound. It is an instance of what I have often spoken to you of, the relation between the blood and the containing vessels, on which principally the spontaneous stopping of hæmorrhage depends.

I hope I have now drawn your attention to all the circumstances of these desperate cases of fractured limbs and lacerated muscles; as far, at least, as I am authorised by the circumstances of the present case. From the extent of railways, and the extraordinary increase of machinery, such cases will present themselves to you many times, in the active life to which you are destined. It is on this consideration that I thought it incumbent not to permit this case to escape you without comment.*

* The lad is recovering.

EXTRACTS

FROM

DR. DAVY'S ACCOUNT OF SOME

EXPERIMENTS AND OBSERVATIONS ON THE TORPEDO *.

[These extracts are inserted at the request of Dr. Williams, who is satisfied that any other reply to Dr. Wilson Philip is unnecessary.—Ed. Gaz.]

The experiments which I have detailed on the electricity of the torpedo, confirm those of Mr. Walsh made in 1772, showing its resemblance to common electricity. They moreover show that, like common electricity and voltaic electricity, it has the power of giving magnetic polarity to iron, and of producing certain chemical changes. In these its general effects, it does not seem to be essentially peculiar, but as much allied to voltaic electricity as voltaic electricity is to atmospheric, or atmospheric electricity is to that produced by contact or friction.—*Phil. Trans.* 1832, p. 272.

Mr. Faraday, in the third series of his experimental researches on electricity, states, that he has little or no doubt, were Harris's electrometer applied to the torpedo, the evolution of heat would be observed. I have made very many experiments on this subject, completely establishing Mr. Faraday's anticipation.—*Ibid*, 1834, p. 542.

This instrument was strongly affected by active fish, and even distinctly by weak ones.—*Ibid*, p. 543.

The tests or indications of the electricity of the torpedo at present known, are six in number, namely, the physiological effect, as the sensation it imparts is sometimes called; the chemical effects, as the precipitation of iodine, the decomposition of water, &c.; its effect on the thermometer, on the galvanometer, and on steel in the spiral.—*Ibid*, p. 544.

It having been stated, on high authority, that a spark has been obtained from the Gymnotus electricus†, I have thought it right to renew the attempt to procure a spark from the torpedo. I

* Phil. Trans. 1832 and 1834.

† Vide Bloch's Ichthyologie, p. 1020; and M. De Humboldt, Ann. de Chimie et de Physique, tom. xi. p. 427.

have tried the method which it is said succeeded with Mr. Walsh in the instance of the Gymnotus, namely, dividing with a penknife gold-leaf attached to glass, and connecting the divided parts with the contact wires. Using an active fish in this way, I could neither observe a spark in the dark, nor in the light detect the slightest indications of the passage of electricity, &c. Reasoning on the subject, this, perhaps, is what might be expected, considering that the surface of the fish is a better conductor than air.—*Ibid*, p. 545-6.

The electricity of the torpedo, theoretically considered, offers a wide field for speculation. Is it, it may be asked, merely a form or variety of common electricity, or a distinct kind, or not a single power, but a combination of many powers?

The first opinion, which is commonly received, and which has been ably advocated recently by Mr. Faraday, is supported by the majority of the facts contained in this paper. The circumstance principally hostile to it, at least in appearance, is the interruption of the torpedinal electricity by the smallest quantity of air, and its want of the power of attraction and repulsion in the air.

These peculiarities are seemingly in favour of the second opinion, that the electricity of the torpedo is specific and peculiar. But till the opposite surfaces of the electrical organs can be perfectly insulated, so that no easier mode of communication is afforded than through air, they can hardly be considered as deserving of much weight.—*Ibid*, p. 548.

This, for the present, is a sufficient explanation why the electricity of the torpedo does not affect the gold-leaf electrometer.—C. J. B. W.

Jan. 11, 1836.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

Annales d'Hygiène publique et de Médecine légale. Tome XIV. Deuxième partie.

THE articles in this number are diversified and interesting. In the department of HYGIENE we have, in the first place, a translation of the paper of Dr. Casper, of Berlin, *On the Influence of Marriage on the Duration of Life*, of which we gave the substance in a paragraph in our last number (p. 591.)

The second article is on a curious as well as homely subject—the fattening of pigs: it originated in a question proposed by the French government,—

Whether the sale, slaughter, and consumption, of pork fattened with horse-flesh, were likely to be prejudicial to the public health? MM. Adelon, Huzard fils, and Parent-Duchâtelet, have decided in the negative, and their answer is in the paper before us. The circumstances which led to this investigation were briefly these:—Various localities about Paris, where knackeries and carrion butcheries were established, had for above ten years constituted a nuisance intolerable, yet irremediable, when at last the expedient was adopted on a great scale of steaming the flesh of the dead animals. This prevented putrefaction, and rendered the bones perfectly pure, and free from ill odour. Some intelligent speculators then hit on the plan of giving the boiled flesh to pigs, and it proved very successful: so much so that large herds of swine, from 400 to 500 together, are now kept and fattened in the environs of Paris; and it is said that some of them will presently be increased to the number of 1000 or 1200. The carrion is steamed on the premises in some of these porkeries; in others it is given to the pigs raw; but whether cooked or raw, the pigs thrive wonderfully on the diet, so as, in fact, to become from 15 to 18 francs a head more valuable in the course of six weeks or two months.

But the swinish multitude—the lovers of pork—soon raised an outcry against the new system; they could not stomach the idea of pig fattened with dead horse, and accordingly raised several scandalous rumours:—1. That pigs thus fed must be dangerous—perfect cannibals—eating all the children they could catch hold of. 2. That the flesh of such pigs must give rise to disorders in the human body, particularly to leprosy. And 3. That pork-butchers in certain places sold sausages made of the produce of carrion, and therefore ought not to be dealt with. The pork-butchers suffered accordingly; but the council of salubrity took the matter up, as we have just now mentioned.

Nothing can be more methodical than the proceedings of the reporters: an application is made in the first instance to M. de Blainville and M. Desmarets, the eminent zoologists, to ascertain whether the hog is naturally carnivorous. According to these gentlemen the hog ranks, by reason of his organization, between the carnivorous and the great herbivorous mammalia. Like the omnivorous animals, such as man, the bear, and the rat, his molar teeth are adapted for variety of food. The stomach of the hog, also, is membrano-muscular, without any complications or divisions like those in the stomachs of the ruminants, which are herbivorous *par excellence*. In short, the conclusion is that the hog is omnivorous, and that nature intended animal matter to form a portion of its food.

Observation also shews that swine will not fatten on mere grain or vegetable matter; on the contrary, they become pot-bellied, meagre, and measly, on such fare; while the regular addition of a little gelatine to their daily allowance has been proved by experiment to restore them to health, and to give them a proper degree of *embonpoint*.

In the Veterinary School at Alfort, there are generally kept from 100 to 150 pigs of all sorts and sizes; and for the last seven years they have been fed on nothing but the *débris* of dead horses which have been dissected. This refuse matter is thrown to them in a raw state, and they eat it greedily. Now there are no better pigs, nor better pork for eating, than are found at Alfort. The pupils eat it without scruple, and it has never had the least bad effect upon them. Nay, MM., the reporters, are highly pleased with the food—they tell us it is excellent. "*Nous avons assisté* (say they), *à un repas de cette école, et avons goûté le petit salé qu'on servait ce jour-là, et nous l'avons trouvé parfait.*" This was putting it to the pudding-test, than which we all know there is none better.

As to the assertion that this sort of food renders animals ferocious, it is not borne out by the fact. At Alfort the children of the swineherd are constantly about the pigs, pursue them, and make them readily obey the word of command; yet, though thousands of swine, fed entirely on *raw* carrion, have been reared in this establishment, not an accident has ever occurred which could be attributed to ferocity arising from that cause. It is not likely, therefore, that *boiled* flesh should infuriate them.

There is no evidence whatever of any disease (particularly leprosy) being traceable to the animal nutriment of pigs. The herd at Alfort are always healthy, as are those who feed on them.

The question of possible danger from eating of swine fed on the flesh of animals which had died of *disease*, is also negatived. Dogs and cats have been fed for some time on cancerous flesh, and they have grown fat upon it. Desgenettes and Larrey have seen the dogs and jackals at Jaffa rooting up and devouring the bodies of those dead of the plague,—eating their very buboes, without appearing to suffer the least inconvenience. In the course, also, of the first French revolution, the starving wretches of the faubourg St. Germain and the vicinity of Alfort, ate perhaps from 700 to 800 distempered and farcied horses which were sent to those places by government for purposes of experiment: yet, so far from doing mischief in any instance, the supply saved the lives of many unfortunate people.

In conclusion, both on the score of administrative economy and that of hygiène, the reporters strongly recommend the encouragement of this new branch of industry. It puts an end utterly to the disgusting business of the former carrion butchers: the flesh of dead animals is turned to a most useful purpose, and should there be a greater quantity of it than is wanted for immediate consumption, the steam process, by drying it, renders it fit for keeping and for service in various ways.

There are two other papers in the hygiène department, of which we can only give the titles. One is a very long article, by MM. Labarraque, Chevallier, and Parent-Duchatelet, *On the Means of Improving the Common Sewers of Paris* (illustrated by several engravings); and the other, *An Account of the Sick Prisoners in the Maison Centrale at Nismes*, by M. Boileau-Castelnau.

The MEDICO-LEGAL contributions are four in number:—1. On the strangulation of new-born infants with the umbilical cord, by M. Tauffliεb; 2. A report by MM. Chevallier, Orfila, and Barruel, on certain blood-spots found on clothes, &c., in connexion with an accusation of murder; 3. M. Leuret, on a case of alleged burning to death; and 4. A set of cases of homicidal monomania.

Strangulation with the Umbilical Cord.

In M. Tauffieb's paper the question is discussed, whether there is any distinctive character by which strangulation with the umbilical cord can be known? In ordinary infanticide by strangulation, medical jurists have generally asserted that there is found a livid red circle round the neck, while in accidental strangling by the navel-string this circle is wanting. But some of the most eminent modern authorities have shown that this characteristic is fallacious. Dr. Schwartz, of Fulda, has actually seen the livid furrow produced by the umbilical cord; and Wildberg, in examining an infant which had lived a quarter of an hour after birth, but had suffered severe compression by the cord in the delivery, found that there was still present a narrow red circle round the neck, though without depression or ecchymosis. Carus, also, met with a similar case. And the result of these several observations is, that the marks of accidental strangulation by the navel-string are not to be distinguished from those produced by any other cord applied with a criminal intention.

M. Tauffieb, however, suggests that there are two circumstances which may guide us in our medico-legal diagnosis. In the first place, when there has been strangulation by the umbilical cord, the placenta may generally be expected to be found expelled along with the child; and secondly, there will be detected no indications of respiration having commenced; of course, none of the signs of death by asphyxia. Any exception to this, as in Wildberg's case above-cited, must be looked upon as exceedingly rare.

Analysis of Blood-spots.

There is nothing very important in the report of MM. Chevallier, Orfila, and Barruel, at least of a positive kind: the results were principally negative. The questions proposed by the legal functionaries were complicated and difficult, not generally admitting affirmative answers. A gamekeeper had been murdered, and certain persons were in custody charged with the crime. Spots were found on the garments of all the parties, and it was sought to determine whether these were blood-spots—whether they were of human blood, or that of a sheep or hare, or the like; also, of what date were these spots—whether they were of some weeks or some months standing; in short, whether the traces of blood on the victim corresponded with those on the alleged murderers. The medico-legal examiners were able to show that most of the spots were of blood, and probably of human blood; but they could determine little more with any degree of probability.

Burns, before or after Death.

In M. Leuret's paper on distinguishing burns as inflicted before or after death, there are some very important points; particularly where allusion is made to Dr. Christison's experiments and observations on the same subject.

The case which forms the staple of the article, is given at considerable length from the *Gazette des Tribunaux*. The following seem to be the principal particulars:—

On the 10th of July, 1834, the woman Berenger was found dead in her sleeping apartment. Her body, which was quite naked, was found on the hearth, with the head in the fire-place. Both head and neck were burnt to *carbonisation*. The medical man called in to examine the remains thought it possible that murder had previously been committed by strangulation, and that the burning was but an expedient to hide the proofs of murder. The circumstances of the woman and her husband made it not improbable that the latter committed the crime. On the night of the 5th July, cries like the howling of a dog were heard issuing from Berenger's house. Berenger himself, on the following day, appeared at the door of his wife's chamber, demanding admittance; and on not receiving any answer to his repeated knocks, he was heard to use the expression, "that he feared all was not right." He then pushed in the door, hastily threw himself on the floor beside the body, and said, "Ah! I knew it—I thought it would come to this—she is burnt to a cinder." The man was arrested; and M. Seguy, the medical witness above-mentioned, made his formal report. It was to the effect, that he found Madame Berenger lying on her left side, with her head in the middle of the fire-place. The legs were in an easy state of flexion, and neither hands nor feet betokened struggling. Beneath the body were some remains of a chemise, which had evidently been consumed by fire. The habit of body

was spare and delicate: the limbs possessed considerable cadaveric stiffness. None of the parts of the face could be recognized, they were so completely burnt. The hair of the head was all burnt off, except what belonged to the left temple, where it was copiously matted with blood, not yet quite dry. The burnt parts were no where encircled with redness; nor were there vesicles observed, except on the lower part of the abdomen and on the left knee. On opening the head there was found to be strong adhesion of the membranes to the cranium on the left side; those on the other side were strongly injected; the sinuses were filled with black blood, and the substance of the left lobe of the brain was reduced to *bouillie* about the temporal region and base of the skull. The ventricles of the brain contained a bloody serum. The lungs were hepatized, gorged with blood, and the bronchia were filled with a reddish mucosity. Such were the chief appearances. M. Seguy infers from them apoplexy (!) *caused by violence*, and producing death previous to the burning. That it was not an *accidental* apoplexy he concludes from the habit of body of deceased, and from her history: but, on the whole, the examiner declines saying any thing positive.

Another medical authority was brought forward by the friends of the accused, who contradicted or neutralized most of M. Seguy's report. We shall only extract what the new referee says regarding the appearance of the burns:— "The absence of vesicles and circles of redness in the upper parts of the body is owing to the continuance of the process of burning. The traces of their appearance on the lower parts show that the fire commenced at the bottom of the chemise; by which accident the apoplexy was probably caused, and the consequent fall on the hearth. In conclusion the reporter states, that, in his opinion, the woman Berenger was not strangled, but died of sudden apoplexy." Notwithstanding this, the husband Berenger was condemned to hard labour for ten years.

We have not room to give the observations of M. Leuret on this curious case—with the result of which he appears dissatisfied; but we must not omit what he says in the way of qualifying Christison's diagnostic remarks:— "The experiments of Christison show that vesicles filled with serum were not formed after death in those subjects on which he tried to raise them; but this does not prove that they may not be formed in subjects placed in different circumstances. I have seen large and numerous vesicles filled with serum raised on a dead body, at least twenty-four hours after death. It happened in this manner: I had placed a brasier, full of burning charcoal, near the legs of an infiltrated subject (*cadavre infiltré*); the epidermis hardened a little, then rose, and underneath was found an abundant collection of a reddish serosity. On removing the brasier to other parts of the body the same phenomenon was observed. I have since tried the experiment on various subjects not infiltrated, but have not succeeded in producing the same effect."

MEDICAL GAZETTE.

Saturday, January 16, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri: potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

A WORD OR TWO ON THE NEW UNIVERSITY.

It is very amusing to watch the antics played by the *one-faculty* men on the eve of the establishment of a Metropolitan University. They have gone through various tricks of ground and lofty tumbling, but seem to have so addled their brains by the performance, that they know not whether they stand on their heels or their heads. First we had the announcement of a grand spectacle (this was some time ago), called the "OneFaculty-*instar omnium*," which was to embrace a multitude of *dramatis personæ*, and to be the most splendid thing the world ever saw. But it went lamely off, and was soon shelved. Next came what was expected to be a more popular entertainment, under the imposing title of "The Tri-partite Faculty," the performers to be selected from among the stars of the three kingdoms; and the prime attraction, the display of an educational chimera or cerberus—a sort of

"three gentlemen at once," its uppermost part fire and smoke, &c.—but it all turned to smoke, and vanished in thin air. What was the next expedient of our "managers in distress"? They found the public taste strongly in favour of something more like the *legitimate* drama than the non-descripts hitherto offered them, and a rival House having got up with considerable pains a new piece, to be called the "University of London," and which is still in preparation,—our rival managers, at their wits' end, are endeavouring to bring out the same thing with the addition of various crackers, rockets, and explosions, which they feel assured will quite captivate the genuine lovers of noise and confusion.

But in sad and sober earnest, we cannot but commiserate our speculating and evidently much disappointed contemporaries; they find themselves reduced to a very awkward pass. After all their high-flown promises and anticipations, they are obliged to settle down to the contemplation of a simple University in London for giving degrees—those degrees, too, to be granted as nearly as possible on the same terms as are required in other Universities, with this special exception, that here there shall be no religious tests. What a falling off for those who had calculated on, and assured the world of, the downfal and utter destruction of all the medical institutions in the empire, in order to make room for One Faculty! Poor creatures, we can well understand how much they are beside themselves: they must be restrained from doing mischief in their madness, however.

The occupation at present of the parties referred to is to endeavour if possible to pull the scheme of the new University to pieces. But they must have a shrewd guess that their destructive efforts will be of no avail. They affect to be intimately acquainted with the details of the new structure, while, in reality, they know nothing about them. Failing in their endeavours at demolition, their next resource is to suggest alterations and improvements. Let us see what some of these are.

It seems to be the notion of our *ci-devant* "one-faculty" people, that the "University of London" ought to be put upon a footing different from that of any other similar establishment in Europe: it ought to be thrown open, they say, for the benefit of certain individuals of a poorer grade, so that every one who deems himself competent for obtaining a degree shall be allowed to present himself for examination: there should be no test of a regular education required, but the examiners should determine in the first and last resort on the sufficiency of the candidate. What a pitiable ignorance does all this exhibit of the object and working of a University! But what else can be expected of the parties? They are not aware that this sagacious proposition has been again and again canvassed, in the formation of educational establishments like the one in question, and invariably rejected for its impracticableness. The progress of society cries aloud against it, and most especially the progress made in this country and in this metropolis. In a small community, where every member is under the immediate eye of the superintending authorities, it might be possible to establish a board to grant diplomas to parties offering no other qualification than the mere passing of an examination; but in the metropolis of Great Britain, what senate, or board of examiners, would take upon themselves the responsibility—the onus—of ascertaining the claims to degrees of all adventurers who should happen to come before them? To do this with any possibility of satisfaction to themselves and the public, they must not be

left without the protection of some preceding test—some ground for admitting candidates to the final scrutiny; and every reasonable man must admit that there is no test so proper for the purpose as that of showing that the parties have been educated after an approved and recognized system.

Our worthy speculators say that this is to secure a monopoly in favour of two or three schools—such as London University College, King's College, &c. They may spare themselves the uneasiness of this notion. There will be, in this respect at least, no monopoly recognized. All the respectable schools of the metropolis will stand upon a fair footing; or rather, none will be denied the privilege of preparing candidates for the University Board, which have due means and appliances of bestowing a proper medical education. This liberal allowance, however, must not be understood to apply to those rubbishing concerns where two or three ill-authorized individuals, gathering into some obscure corner, and professing to teach three branches each, deceive the public with the semblance of a medical school. If it be a monopoly to exclude things of this sort, then we are sure there will be a monopoly —but one which we think no respectable member of society will grumble at.

Here, however, we should not omit to notice what some, who appear to be well informed, have stated rather confidently—namely, that it is in contemplation to make a degree in arts a prerequisite for a degree in medicine; and not only so, but that the said degree in arts should be taken in this metropolis, or in one of the other English Universities. We discredit the rumour, but must say, that if such plan were adopted, we should protest against it in the strongest manner. Monopoly, and that of a most obnoxious kind, would be thus stamped upon the front of the new institution. For with what reason (if a degree in arts ought to be at all insisted on) should those taken elsewhere than in London, or in England, be excluded? Surely those obtained in regular Universities in other parts of the United Kingdom, or on the Continent, provided actual residence be required, ought to be equally acceptable as a test of the requisite preliminary education.

Another important question, in the settlement of which we must all feel an interest, is—who are to be the members of the University senate; and in particular, who are to constitute the Board of Examiners? Our enlightened but restless speculators, who would have this a *National* institution (in the true revolutionary sense of the term) propose, of course, that it should be decided by *concours*. They may spare their disquietude on this head also. The appointments will be made in due time, and we trust in a satisfactory manner. One word, in conclusion, to a mortified legislator — an *honourable* man — who tells us it is a "report," but a "falsehood," that certain parties whom he names have been called into the councils of the Chancellor of the Exchequer, to lend their aid in the formation of the new establishment. We can inform him, that however current the "report" may be, and however unpalatable to him and his dupes, it is *not* "a falsehood." We do not pretend to state the particulars of what is in progress, but the *on dit* is probably not so much at fault. We merely allude to it to warn people how they listen to "falsehood" as denounced by the enemies of truth.

MEDICAL GRIEVANCES UNDER THE POOR-LAW.

Ruricola's letter, at page 619, will be found to contain a clear and comprehensive plan for remedying the present annoyances suffered by medical practi-

tioners, in consequence of the working of the amended Poor-Law. We strongly recommend this valuable document to the immediate notice of the profession at large, as containing what appears to us the most practical and efficient scheme of relief yet proposed in any quarter.

CLINICAL LECTURE

ON

A CASE OF COMPOUND FRACTURE OF THE TIBIA,

FOLLOWED BY TRAUMATIC DELIRIUM:

Delivered at St. George's Hospital, Dec. 22, 1835,

BY SIR BENJAMIN C. BRODIE, Bart.

GENTLEMEN,—I shall begin with reading the notes of a case to which I purpose to call your attention in this day's lecture.

"George Reeves, æt. 50, by occupation a coal-heaver, was admitted on the 7th December, 1835, into Oxford Ward. He had a compound fracture of the right tibia, about four inches above the ankle. The wound was small, the bone not protruded, nor the apparent injury in any way very considerable. The accident had been caused by a heavy piece of stone falling from a considerable height upon the limb. Some hours elapsed before he recovered from the shock of the accident—that is, from the state of collapse which it occasioned. The limb was placed on a pillow, with junks and a splint on either side. He was directed to take forty drops of laudanum. On inquiry, it appeared that the patient had been in the habit of taking a large quantity both of porter and gin. On that account it was directed that he should be allowed a pint of porter daily, from the time of his admission into the hospital."

The report on the 9th December, two days after his admission, is as follows:—"He slept little during the night: the pulse was frequent, but not strong. When he held up his hand, it was observed to be very unsteady — quite tremulous. His tongue was white, and, when he put it out, that was tremulous also. He was directed to take four ounces of gin daily, in addition to the porter."

The next report is on the 11th December; and it is then stated, that "for the two preceding nights he had been much excited and hurried in his manner, having had little or no sleep. The leg was examined; there was no more inflammation than there ought to be about the wound. In was placed in an Assalini's fracture box. He was directed to take a grain of acetate of morphia, with some compound extract of colocynth in the evening."

"On the evening of the 12th he became delirious, and was quite unmanageable, so that it was necessary to confine him in a straight jacket. On the morning of the 13th he got some sleep, but not until he had taken as much as two drachms of laudanum. When he awoke from this sleep he was talking incessantly. There was much trembling of the hands; the pulse very frequent, but not strong. However, he answered questions that were put to him rationally. I prescribed for him the following draught:—One ounce and a half of camphor mixture; a quarter of a grain of acetate of morphia; a drachm of oxymel, and a drachm of comp. spir. of sulphuric æther: to be taken every six hours. He was directed also to have allowed him two pints of porter and six ounces of gin daily."

On the following day (the 14th), "he was much better; the pulse was less frequent, and there was less trembling of the limbs. He had some comfortable sleep during the night, and was quite collected on awaking in the morning."

On the 16th, it appears that "he had had no return of the delirium, but that he talked a great deal at times, and in a very hurried manner. The pulse was reduced to 90 in a minute; the tongue continued white."

"On the 17th he had again become violently delirious during the night, so as to require the straight waistcoat. He took ten grains of camphor and two grains of opium, which produced a comfortable sleep, and he was now better. The pulse had risen to 110, but it was not a strong pulse, like that from inflammatory disease. The tongue was white, and brown in the centre; the bowels were open. He complained of shooting pains in the leg, but on examining it there were no marks of unusual inflammation in it; in fact the leg had been going on well during the whole of this time. He was directed to continue the same draught, taking half a grain of acetate of morphia in each draught, instead of a quarter of a grain."

On the 21st (that is, yesterday), "he was perfectly rational during the day. He had slept a good deal, but during the preceding night he had been very restless, and it had been necessary to secure him once more by the straight waistcoat. There was still some trembling of the hands. The pulse had fallen to 80, the bowels were opened regularly without medicine, and the tongue had become clean. The leg was doing perfectly well. He took the whole of his porter, gin, and medicine, regularly, and also some light nourishment. During the whole course of these symptoms he

had not been denied nourishment, but he had not had much inclination for it."

You will observe that on the 17th there had been some aggravation of the symptoms. He had been put into a ward by himself, in order that he might not disturb the other patients. His wife was with him, and there was some reason to suspect that she partook of the gin and porter allowed him; and if so, we may account for the symptoms having been then aggravated, by the circumstance of his not having had the quantity of stimulus that he ought to have had.

Here is a man, then, who meets with a compound fracture, but not a very bad one. The local injury has been going on very well, but he has been, from a short time after he came into the hospital, in a state of delirium approaching to mania in its character; and he has been getting better, not under the use of the lancet and other antiphlogistic remedies, but by stimulating him with gin and porter, and giving him opium. Here is a disturbed state of the nervous system, following a slight local injury, and not depending on inflammatory action any where.

Many cases occur in which symptoms somewhat corresponding to these follow a local injury. We say that the patient labours under *traumatic delirium*, under delirium supervening on a wound: perhaps *mania* would be the more proper appellation; for the symptoms more resemble those of mania than of the delirium of fever.

These maniacal symptoms after injuries no doubt depend partly upon the injury inflicted; but they depend more on a peculiar condition of the nervous system of the patient. The nervous system of one man is in that state that no local injury will produce these symptoms; while, in another, they are produced even by a slight accident. Various circumstances make the nervous system thus differently susceptible in different individuals. In the museum you will see a preparation of the lower extremity of the tibia, in which there has been an abscess. It was taken from the leg of a gentleman, which I formerly amputated; and it was the first case that made me understand the nature of those abscesses in the centre of the tibia, and which, since, I have been enabled in three cases to relieve by the application of the trephine; thus preserving the patient's life and limb also. In this case, neither myself nor any one else who saw the patient, understood at the time the nature of the disease. There had been an abscess in the centre of the tibia, pent up in unyielding bone; and, being thus pent up, it was the cause of an unusual quantity of pain and distress. He had been going on thus for fourteen or fifteen years, and his nervous system was, in consequence, in a state of excessive irritability. He had not any bad habits as to his mode of life, but his mind and body suffered from the effects produced by the constant torment of an abscess locked up in the centre of the tibia. He bore the operation of amputation without saying a word, but in the evening he was restless and uncomfortable, talking incessantly. There was some secondary hæmorrhage, which rendered it necessary to take the dressings off the stump; and though he had been quite tranquil at and after the operation, yet, when the dressings were removed afterwards, he roared so that he could be heard in the street when the dressings were removed. During that night he slept not at all; the next day he had a rapid pulse, was talking incessantly, and the tongue was quite clean. The pulse continued rapid, and the tongue quite clean. Night after night he procured no sleep; he talked incessantly night and day, believing that there were individuals in the room who were not there. On the fifth day, his tongue, for the first time, was covered with brown fur; the pulse was so rapid that it could not be counted; he was in a state of great prostration of strength; the retina of the eye became perfectly insensible; the pupils were widely dilated, and when a candle was put close to the eye he was not sensible of the light, and the pupil did not contract in the smallest degree. This insensibility of the organs of vision was followed by a general stupor, in which he died. No morbid appearances were found in the *post mortem* examination. In this case, the maniacal symptoms following the injury, and which terminated in the patient's death, evidently arose out of a disordered state of the nervous system, which might itself be traced to the impression made on it for a great number of years by a very painful local disease.

But to what are the peculiar nervous symptoms that have taken place in the individual now under treatment to be attributed? You see such cases not unfrequently in the hospital. They occur chiefly in persons who have been accustomed to drink a great deal of fermented liquors and ardent spirits. An individual who has these bad habits meets with an accident; he comes into the hospital, and is kept on low diet, and in the course of the following day you find the following symptoms begin to show themselves:—At first, as in this patient, you recognize the disease by there being rather a hurried manner, by the patient's hand being tremulous; then his manner becomes more hur-

ried, the hand more tremulous: he is never quiet, but is in a state of constant gesticulation, and incessantly talking. He answers questions which you put to him, and is sensible to external impressions; yet he is talking of things that do not exist, believing that people are present who are absent, or that he is employed in something in which he cannot, under the circumstances, be employed at all. He sleeps little, or more frequently not at all, and the pulse becomes rapid. The tongue sometimes remains clean, and at other times becomes furred. Although the pulse be rapid, it is not a strong, hard pulse; on the contrary, it is generally weak, so that you would say that if the patient were to be bled he would sink. If nothing be done for the patient's relief these symptoms may go till the fourth or fifth day, or longer; and then he may become exhausted, sink, and die. In some cases other symptoms supervene,—sickness, vomiting, diarrhœa, and a constant discharge of watery evacuations from the bowels.

In the *post mortem* examination, if the patient die at an early period, you may perhaps not be able to find any morbid appearances whatever; but if the disease has been protracted, you discover some effusion among the membranes of the brain; and it is said that sometimes there is ulceration of the small intestines. From whatever cause these maniacal symptoms which follow local injury arise, they do not depend on any inflammatory action of the brain or its membranes, or inflammatory affection of any other part. They are not to be relieved by depletion, and what is commonly called the antiphlogistic treatment: on the contrary, they are aggravated by bleeding, and by all other depleting measures. They indicate an absence of power, and are to be relieved not by the abstraction, but by the exhibition of stimuli. In illustration of this observation I may tell you what I have sometimes known to happen after injuries of the head. A gentleman, for example, was thrown from a carriage, and struck his head. He laboured under symptoms which, as I was led to believe, indicated a severe concussion of the brain, complicated with a slight degree of extravasation. These were followed by other well marked symptoms of acute phrenitis. He was bled; and the coagulum was much contracted, cupped on the surface, and covered with a thick buff. He was bled again, and so on, till the symptoms were relieved. But now he became maniacal, and got into the same state in which this patient was the other day. He could not sleep; he was in a state of continual jactitation, talking incessantly, believing he was driving his carriage or riding on horseback, and knowing nothing of the persons about him. Bleeding was again had recourse to, but was followed by an aggravation of the symptoms. Another practitioner was called in, and advised that he should be still further bled; and this was done in opposition to my judgment: but the patient was still worse afterwards, so that he seemed to be on the point of death. I then turned round, and gave him wine and nourishment; and the moment that the stimulus entered his circulation, the maniacal symptoms began to subside, and in the course of a very short time he was perfectly well.

There are two periods, then, after an accident, at which maniacal symptoms may occur. They may occur in certain constitutions immediately after the accident has happened; and where the nervous system is less susceptible, they may occur at a later period, when the patient has been lowered by repeated bleeding and purging, and that constant starving which severe local injury will sometimes require.

The cases in which we see these symptoms occur in the hospital are, as I have mentioned, chiefly those of drunkards, or at any rate of people very much accustomed to the stimulus of fermented and spirituous liquors. They take place so continually in persons of this class, that I generally direct that the habits of patients admitted with accidents should be inquired into; and, if they have been downright gin-drinkers (except, indeed, there be some strong reason against it), that they should be allowed a certain quantity of porter or gin from the beginning. In this case Mr. Robinson, the house surgeon, very properly made this inquiry, and the patient was allowed porter in the first instance; but you perceive that the quantity was not sufficient. Then, two days afterwards, I allowed him a certain quantity of gin; still that was not sufficient, and I found it necessary to allow him a larger quantity afterwards; and even this would not have been enough, if we had not combined other means with it. And what were those other means? Opium and Hoffman's anodyne; that is, the comp. spir. of sulphuric æther. The patient took at first a quarter of a grain of morphia, and then half a grain every six hours, with a drachm of Hoffman's anodyne; and this was attended with very beneficial results. Opium is productive of very good effects in these cases; you cannot depend solely on the exhibition of spirits and fermented liquors, and it is prudent to combine opium with them. The patient will bear a large quantity of opium here, such as would almost poison him in a state of health. Even the full doses which I ordered for this patient

every six hours were not sufficient; and he was forced to take a couple of grains of opium once or twice in addition; and although I said that the draught was given every six hours, yet he took an extra draught occasionally besides. It is curious to observe in these cases, how immediately, for the most part, the symptoms alter when the patient is put under the influence of alcohol and opium. This was very manifest throughout the whole of this man's case; and there is great reason to apprehend that the aggravation which occurred one day arose, as I mentioned, from his not getting the whole of the stimulus that has been prescribed for him.

This disease bears a near relation to some of those cases which have been described under the name of *delirium tremens*, but not to others. It has been well observed, (I believe by Dr. Stokes) that under that term writers have sometimes confounded together different maladies. In some cases, where the patient has been said to labour under delirium tremens, the maniacal symptoms have been the consequence of excessive drinking for some days. A man unaccustomed, perhaps, to drink a great deal of liquor, indulges himself in this manner for some days in succession; and at last he gets wrought up to a state of mania, from which he is to be relieved, not by giving him more stimulus, but by gradually, and prudently, and carefully, diminishing the quantity of stimulus which he has been lately taking. Then, in other cases of delirium tremens, the symptoms have been the consequence, not of the continued use of stimulants, but of their sudden abstraction; and it is to this last class that the case of this man properly belongs. There is no doubt that the symptoms depend chiefly upon the peculiar condition of the patient's nervous system: but that is not all; for if this individual had been admitted into the hospital, not suffering from a mechanical injury, but with an ulcer of the leg, it is not probable that these symptoms would have come on. They may develope themselves, and sometimes do, without mechanical injury, but still there is no doubt that the slightest impression made on the nervous system in this manner makes the patient much more susceptible of these symptoms than he would be otherwise.

The theme of medicine is ever the same, only the melody alters. And later times have been so prolific of composers, that every couple of years we sing a new song.—Hufeland.

LECTURES

ON THE

DISEASES OF THE NERVOUS SYSTEM.

By M. Andral.

As published in the *Gazette des Hôpitaux*, with the approval of the learned Professor.

INFLAMMATION OF THE SPINAL MARROW.

[We subjoin the conclusion of M. Andral's inquiry into the connexion between the cerebral lesion and the paralysis, as the subject was begun last week. We shall, however, be compelled, by press of other matter, to postpone the rest of the lecture till this day fortnight.]

The third case is to be found in the work of Morgagni, who relates it on the authority of others. An individual, 70 years of age, was suddenly attacked with apoplexy, accompanied by hemiplegia of the right side: blood was found in both ventricles, with some erosion of the right optic thalamus. This case is unsatisfactory, and does not bear the stamp of Morgagni's accuracy. It is said that the effusion existed in both ventricles; but might it not have been more considerable in the left? On reading the case, it is seen to be defective.

The fourth case belongs to Brumer; it is to be found in the third part of the Journal of the Curiosities of Nature. A woman, 47 years of age, fell down in an apoplectic fit, in whom paralysis was observed on the right side. Several apoplectic cysts, which Brumer carefully describes, were found in the right hemisphere. The first attack had taken place four years before death; some recent clots, which had been the immediate cause of the last attack, were found beside the old cysts.

The fifth case was seen by Morgagni, who relates it in his 57th letter. An old woman died, having paralysis of the right side; and on examination, softening of the brain was found, also situated on the right side. Here Morgagni relates what he saw himself; he is not content with merely stating what lesion he found, but he proceeds to describe the state of the rest of the brain, on the left side of which he found no morbid change whatever.

The sixth is recorded in the 13th letter of the same author. This instance occurred in a woman, aged 24, who had apoplexy, with paralysis of the right side; she died, and a coagulum was found in the midst of, and without, the right corpus striatum. Morgagni did not trust to his pupils, by whom the case was related; he went to see it himself; and it was

only after he had become thoroughly satisfied, that he gave it a place in his work, expressing the astonishment which it had excited.

The cases which follow are modern.

The seventh was observed by M. Bayle, and was published in the Revue Médicale in 1824. There was paralysis of the left side; and on examination after death it was ascertained that the only lesion consisted in softening of the anterior part of the left hemisphere.

The eighth case is to be found in the work of M. Rostan on Softening of the Brain. The woman who formed the subject of it was 84 years of age, and had hemiplegia of the right side. On opening the body, an old effusion, with softening, was found at the lower and back part of the right hemisphere.

The ninth case is given by M. Leuret, in the Journal de Progrès. Here the case is less clear than with regard to the others. It relates to an individual who had paralysis and contraction of the right arm, and no loss of power in the limbs of the left side. But what renders the question complicated is, that both hemispheres were diseased, although the right was the one principally affected, there having been but a very small portion of softening on the left side.

The tenth case, that by Winmeyer, relates to a man who had palsy of the left side, in whose brain, according to the author, was found an old cyst at the anterior part of the left lobe; but the hemiplegia was recent, and we cannot tell whether sufficient examination was made to ascertain the cause of the loss of power; in fact, the case is incomplete.

The eleventh and twelfth are to be found in the notes added by M. Blandin to his edition of Bichat. He relates that two men, of advanced age, died at the Bicêtre, having paralysis of one side of the body; and that in both the examination after death showed an effusion in the posterior lobe of the hemisphere on the same side as the paralysis. It is of importance to mark the seat of the effusion, because, with regard to the posterior lobe, the anatomical explanation of direct paralysis would be easy in the eyes of some, who hold that this lobe receives its fibres from a part of the spinal cord where crossing does not occur.

The thirteenth case has not yet been published; it was seen by M. Cruveilhier about two months ago. The paralysis existed on the same side as the disease of the brain.

The fourteenth and fifteenth are due to M. Duhambre, who met with them at the Salpêtrière. These two examples, which are well related, were softenings of the brain on the right side, with paralysis of corresponding limbs.

Finally, the sixteenth case of this kind has been presented by M. Fournet to the Anatomical Society, and has not yet been published.

I have never hitherto met with any thing analogous, but I admit the accuracy of some of those which have been published—which, indeed, is above suspicion. For these anomalies there must be some principle—some reason—although the explanation has hitherto escaped us. Unquestionably it is to be found in anatomy. We must, then, here lay down the principle, that paralysis may occur on the same side as the lesion of the brain.

Generally palsy affects both limbs at once, and may be either equal or of different degrees. For the most part, the paralysis of the lower limb is less complete, and less obstinate, than that of the arm. But one limb only may be affected with palsy; and then it is a question whether the anatomical lesion has a special seat in such cases. Some authors have thought it had; and this has formed the subject of various works by MM. Serres, Foville, Pinel-Grandchamp, Bouillaud, Rostan, &c.

Let us examine the question in detail. Has the hæmorrhage a different seat in cases of hemiplegia when the paralysis affects the upper, from what it has when it affects the lower limb? Some authors have laid it down as a principle, that the movements of the lower limbs are under the influence of the corpora striata and cerebral substance immediately adjoining; but that the motion of the arms derives its impulse from the optic thalami and parts surrounding them. Others, again, think that the anterior parts of the brain generally preside over the motions of the lower limbs, and the posterior parts over those of the upper limbs.

In the fifth volume of my *Clinique*, I have given an analysis of 75 cases in which the effusion was very limited: 1st, in 40 cases with hemiplegia, the lesion was found in 21 instances to be limited to the corpora striata, or extending into the cerebral substance anterior to them; in the other 19 the lesion was confined to the optic thalamus, or extended to the pulp immediately behind it. The inference, then, from these 40 cases is, that lesions of the corpora striata and optic thalami influence the movements of the upper and lower extremities indiscriminately. In 23 cases there was only paralysis of one upper extremity. Now, if the theory be true, the hæmorrhage ought to have taken place only at the posterior part of the hemispheres; but such was not the case. In

two the lesion existed in the middle lobe of the brain; in eleven the anterior part alone was affected,—a result just the reverse of what the theory would promise; finally, in the ten last cases of this second list, the effusion had taken place into or behind the optic thalamus: and experience thus gives about the same number of cases for and against the opinion of which I speak.

There were twelve cases of the 75 alluded to in which the lower limb alone was the seat of paralysis; and in all, except two, the hæmorrhage had taken place into the anterior lobe, or into the optic thalamus. Here the theory derives more support from facts, which, however, does not prevent the truth of it from being but relative; for to destroy the law, it requires only the two cases in which, with the same paralytic affection, the lesion was found in the posterior lobe.

The general consequence to be derived from these facts is, that although assuredly the same parts of the brain are not employed to regulate the movements of the upper and lower extremity, since the functions of the one may remain entire, those of the other being impaired or abolished, yet, in the present state of science, it is not possible to say what portion it is of the cerebral mass which commands the motions of the different parts of the body. The results, up to this time, are altogether negative; but they have this degree of usefulness—that they prevent us from falling into error. Does the power of motion become affected if the convolutions alone are implicated? M. Favre, in his Thesis, in 1832, gives an instance of an individual who died hemiplegic, and in whom, on examination, a small clot was found in one of the convolutions of the external lateral and rather posterior part of the left hemisphere, there being absolutely nothing elsewhere. As to the rest, this case is not unique, as M. Lallemand gives an analogous instance in his first letter, page 63, and two others in his second letter, pages 106, 151.

Cases have occurred in which there has been paralysis with the lesion limited to the convolutions of the brain. I have some cases of this kind, and all these facts constitute a powerful objection against the opinion of those who hold that the central white substance is the seat of motion.

When the lesion of the convolutions is chronic, and exactly limited to the grey substance, paralysis may be absent; from which it seems possible to maintain, that if paralysis occurs when such lesion is acute, it has depended upon the central parts of the brain having been irritated. It must be remarked, however, that in the case of M. Favre the disease was chronic.

So much with regard to paralysis in its connexion with lesion of the hemispheres; but these last may remain completely healthy, and palsy take place nevertheless.

With regard to the mesocephalon, it may be laid down as a general principle, that in hæmorrhage of this part the four limbs are paralyzed; but as in our science there are always unlucky exceptions, so in the lesion of which I speak simple hemiplegia has been sometimes observed. Anatomy can explain these anomalies: they take place when the effusion is on one side only of the mesocephalon; but such cases are extremely rare.

WESTMINSTER HOSPITAL.

REMOVAL OF THE BONES OF THE FACE.

Extract from a Clinical Lecture, delivered January 9th,

By Mr. Guthrie.

I am grieved, gentlemen, to be obliged to inform you that the case of Mrs. Brown will not turn out successfully. The removal of so many bones of the face, and of every part which appeared to be diseased, did not suffice to eradicate the mischief. I am afraid it has been reproduced from that part on the body of the sphenoid bone which occupied so much of our attention, and took up so much of our time, during the operation. I wish now I had applied to this the muriate of antimony, or the strong nitric acid. It might have done harm, but it might also have been useful, even if exfoliation of the bone had followed. It is a point to be borne in recollection for future occasions. The tumor is now advancing by the orbit, the periosteum of which seems to be engaged in it, and the external covering of the frontal bone, on its outer part, is equally implicated. The eyeball has suppurated. The tumor can also be felt behind the skin, covering the part from whence the bones were removed, but there is no projection there, as before the operation. I have only to regret that it was not done earlier, and that I had not seen her sooner; for if the disease had been confined to the superior maxillary bone, I feel very confident that success would have followed the operation. We want records of the termination of these cases after operation, undertaken at all periods of time, in order to enable us to form a correct judgment on the subject. I regret I should have to record an unfortunate one.

[Mr. Guthrie then proceeded to give an interesting lecture on Medullary Sarcoma of the Testes; which we shall take an early opportunity of laying before our readers.]

REMEDY
FOR THE
EVILS OF THE PRESENT SYSTEM OF MEDICAL ATTENDANCE
UNDER THE NEW POOR LAW.

To the Editor of the Medical Gazette.

SIR,

THE able and high-spirited letter from Mr. Barber, of Cambridge, in your last Gazette, deserves the especial notice and thanks of his medical brethren; and it is with some reluctance that I feel compelled to differ with him as to the measures which ought to be adopted on the present occasion.

However little "versed in the art of legislation" the members of our profession may be, I fear that, unless they exert themselves to devise an unobjectionable plan for medical relief to the poor—one not merely suited to their own views and interests, but calculated to obtain the support of the legislature and the landed proprietors of the country—no real and well-founded amendment of the present system can take place. It will be useless for us to apply to parliament, with a statement of our complaints, unless we are able, with some degree of unanimity and decision, to propose a rational and practicable alteration. Mr. Barber suggests "that the Commissioners should, as a general rule, desist from interfering with existing contracts." But can it be supposed that the Commissioners would desist from exercising their legal authority?—and even could this be so arranged, the power of oppression and injury would only be transferred from the *major* to the *minor* authorities; for does Mr. Barber believe that the Boards of Guardians would "continue the contracts as they were found?" Does he suppose that a "fair and open competition" can exist before a tribunal of rate-payers, where the amount of stipend must *naturally* be the criterion, and not the fitness or previous claims of the candidates? It would give me sincere pleasure to be assured that this matter might be safely left "in the hands of the local authorities;" but the contrary is evident to every close observer of the administration of the Poor-Law.

The gauntlet is thrown down by our opponents, and unless we unflinchingly take it up, and proceed to a thorough settlement of the question, the opportunity for so doing may not again occur. Were it possible to behold, without dismay, the perpetration of such inhumanity and injustice as the proceedings of the Commissioners and Boards of Guardians evince, I would rejoice that matters are now brought to a crisis, and that abuses which, under the former system, might have crept on for years without any mitigation, may now, by vigorous and honest treatment, be finally removed.

Considering it the duty of every member of our body to lend his aid, however feeble, in this cause, I propose, with your permission, to enter more fully into the details of a scheme for parochial medical attendance; the principles of which I have already submitted to you in the Gazettes of November 14th and 21st last.

At the risk of incurring the charge of tautology, or repetition, I will thus briefly sum up the evils to be remedied:—

1st. The appointment of medical officers by "tender;" which, as a *test* of the qualification of the candidates for office, is *worthless*, if not *vicious*.

2dly. The arbitrary method of "fixing on a rate of remuneration admitted on all hands to be totally inadequate, and compelling acquiescence by a threat (in many instances carried into execution*)," of invading the private practices of settled practitioners.

3dly. The supply of an insufficient number of medical attendants to the poor, causing *monopolies* of numerous parishes, and increasing the distance between the surgeons and many of their pauper patients.

4thly. The absurd distribution of parishes into certain medical districts, determined not by the comfort and safety of the poor, nor by the convenience of medical men, nor by the established usage of parishes, but by the *circuits of the relieving officers*; which are often designedly and avowedly incompatible with a ready access to relief.

5. The dangerous power vested in the relieving officers, and other non-medical persons (all of them interested in refusing orders) of deciding whether the sick pauper requires medical assistance.

6. The insulting and tyrannical treatment of the medical body by the functionaries of the Poor Law; respectable men being thereby often deterred from offering their services, and the poor being therefore left to practitioners of another description.

Any plan of amendment which provides for the removal of all these evils is worthy of the attention of the medical profession and the public. I would therefore recommend that a certain and uniform mode of parochial medical remuneration be enforced throughout the country, calculated separately for each parish, on the following principles:—the amount of sickness,

* Vide Mr. Barber's Letter.

and proportion of pauperism in the population of each parish; the superficial extent of large parishes, and the distance of small ones from the medical officer: a mode of payment, consequently, providing for all these particulars, and for a fair, though of course a low, remuneration for the cost and trouble bestowed, would answer the desired end.

Medical practitioners generally, I believe, like the idea of a salary according to the population, or according to the number of paupers, sick and well; the former is the plan of Mr. Smith, of Southam; the latter that of Mr. Yeatman, of Frome. They are both, however, in my opinion, inapplicable and deficient. Mr. Smith's does not provide for the proportion or variation of pauperism in a parish; nor does it meet properly the distance and extent of parishes. Mr. Yeatman's does not provide for the amount of sickness, which is frequently, at present (and will be more so) out of all proportion to the number of paupers; nor is it in the least suitable for small and distant parishes. This, indeed, he admits, and proposes that medical men should make distinct charges for medicines and attendance in these cases,—a mode which, however desirable to *us*, would not be listened to for a moment by the public.

I know of no plan which will so readily, simply, and effectually, meet the difficulty, as a certain payment for each case of illness. This is now so odious to medical men, from the manner in which the Poor Law Commissioners have effected it, and the unnecessary and unfair conditions with which they have accompanied it, that it is with diffidence as to its reception with my medical brethren, though with confidence in the correctness of its principle, that I venture to recommend it.

I am prepared, from authentic data, to prove, that where numerous patients are comprised within a small space, and close at hand to the medical officer, the sum of 3*s.* for each case will make a sufficient salary; at any rate this ought to be the lowest: and though we might with justice to ourselves state a higher minimum, yet the peculiar and delicate circumstances in which we are placed while engaged in the present discussion—the importance of not burdening the landed interest more than is absolutely necessary, and the desirableness of establishing a better system, even at some sacrifice on our part—induce me to name that sum.

There must, however, be an increase of this payment according to the distance of patients from the medical officer, and according to the total number of patients he attends in a given time; it being obvious that 1000 cases occurring within a short walk of the medical man's residence during one year, may be attended at less per head than 50 at five miles' distance in the same period.

After considerable trouble, and a comparison of numerous parochial salaries, I am disposed to believe that the following scale will answer the purpose of both the payers and the surgeons.

Distance of the Patients from the Medical Officer being within	The Number of Cases of Sickness and Accident attended in each Parish during one Year being 25	50	100	200	400	800	1600
	s. d.	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*	*s. d.*
1 mile	6 0	5 6	5 0	4 6	4 0	3 6	3 0
2 miles	6 4	5 10	5 4	4 10	4 4	3 10	3 4
3 —	6 8	6 2	5 8	5 2	4 8	4 2	3 8
4 —	7 0	6 6	6 0	5 6	5 0	4 6	4 0
5 —	7 4	6 10	6 4	5 10	5 4	4 10	4 4
6 —	7 8	7 2	6 8	6 2	5 8	5 2	4 8

Example—If the total number of patients attended during one year in any parish be 400, the payment per head for such patients as live within one mile of the surgeon's residence should be 4s.; within two miles, 4s. 8d.; within four miles, 5s.; and so on. If the number of patients be intermediate between any of the numbers mentioned at the head of the scale, the payment per head should be altered proportionally. Thus, if the medical officer had attended 150 patients, the payments per head would be 4s. 9d., 5s. 1d., 5s. 5d., 5s. 9d., 6s. 1d., and 6s. 6d., according to the distances respectively.

The subdivision of the scale might be carried out yet more minutely.

One column of this scale, or some intermediate rate of payment, suited for each parish, should be agreed on by both parties at the beginning of the year (perhaps according to the number of cases attended during the previous year); otherwise either party might, by fraudulent management,

increase or reduce the sum total paid, for the same amount of labour, at the end of the year.

Every distinct and different case of illness or accident in the same individual, however close in succession, should be charged, but relapses of the same disorder occurring within one month from the date of discharge should be considered as part of the former illness (to prevent any unfair advantage being taken of the rate-payers by the medical officers).

There should be no extras, except midwifery, trusses, and vaccination. I think I am expressing the opinion of a majority of medical men in proposing that the ordinary run of pauper midwifery cases should be managed by a female midwife, the medical practitioner attending only when called in by her. This will not only be a saving to parishes, but a relief to the profession. The charge should not be less than 1*l.* 1s., with an additional allowance for mileage. Vaccination might be charged at 1s. 6d. per head, and trusses at cost price.

No practitioner should be allowed to attend paupers distant more than six miles, unless he is the nearest eligible medical resident to those paupers; when a farther charge of 4d. should be made for every additional mile in each case. Nor should any one be permitted to undertake the entire care of a parish or parishes which, during the previous year, have furnished a total of above 1000 cases, nor any firm of medical men more than 1600 cases.

The objections to a "payment per case," as effected by the Commissioners, are, I believe, these:—

1st, Because the gross charge is not allowed to exceed a certain amount; thus throwing the whole burden of any cases occurring beyond the corresponding number on the gratuitous services of the profession. This proviso is so absurd and unjust, and so opposed to the principles of a payment per case, that it must be at once relinquished.

2dly, Because the system is a temptation to the relieving officer to withhold slight cases from treatment; thereby not only defrauding the medical man, by imposing on him an undue proportion of serious cases, for a payment expressly calculated on the usual average of illness, but leading to shameful neglect of the sufferings of the poor, whose diseases, if attacked in their incipient and milder stages, might speedily give way to treatment; while, if left until the relieving officer thinks them serious enough to receive an order for attendance, they may end in long confinement, permanent injury, or even loss of life, and occasion to the parochial funds a vast increase of expenditure. This objection is so powerful and reasonable, that I freely confess that, if the relieving officer is to retain the power of granting orders for medical relief, the plan of a payment per case is totally inapplicable; and I therefore must, as *an inseparable preliminary*, contend for the entire abolition of the "order" system.

Not only for the reason just stated, but as a general remedy, I recommend that *the right of decision as to the necessity of medical attendance to sick paupers be vested solely in the medical officer*. Let there be no intervening authority between the sufferer and the person to whom he looks for aid. The only necessary check is the *subsequent* evidence of the patient's pauperism; or, in other words, an investigation into his circumstances, and his need of *parochial* assistance. For this purpose I suggested, in my former communication, that the relieving officer should, within twenty-four hours *after* the patient's application to the surgeon, decide this question; but, on reconsideration, I incline to think that this would not sufficiently remedy the present evils; that it would still leave room for much abuse; and that the relieving officer is by no means a fit judge at *any* stage of the matter. I therefore recommend that this *subsequent investigation and decision* should be made by the Board of Guardians, who might inspect the weekly list of sick applicants presented to them by the medical officer, and declare that the medical relief afforded to those individuals who do not belong *strictly* to the class of permanent paupers, is merely by way of *loan*, the payments *by the scale* for such cases being recoverable from the patients according to the provisions of the Poor Law Amendment Act, or similar provisions to be made for this especial purpose.

The "payment per case" under these regulations would, I believe, answer all the requisites with which I started; it would be in exact proportion to the amount of sickness and pauperism in the parish; it would, by the increased graduated charge according to distance from the surgeon, provide for the extent of large parishes, as well as for the remoteness of small ones; and it would remunerate the medical officer for his time and trouble, by the augmentation of the rate of payment for such parishes as afford only a *few* cases of sickness in a given time and space.

As a crowning measure to these necessary reforms, I would urge the election of the medical officers by the rate-payers of each parish, instead of by the Boards of

Guardians. I have already given my principal reasons for this in your number of November 21, and need, therefore, only remark, that the welfare of the poor is more likely to be consulted by the whole body of rate-payers than by the Guardians; that *favoritism* and collusion between medical men and a few influential persons will, by a general election, be properly guarded against; that, in connexion with the preceding measures, the absurd system of "medical districts" will thereby be abolished; and that the appointment would then possess some *credit*, and induce a superior order of practitioners to undertake the duties.

I believe that I have thus provided a remedy for every one of the evils mentioned at the beginning of this communication, which I trust will not prove too lengthy for your kind insertion.

I remain, sir,
Your obedient and obliged servant,
RURICOLA.

Jan. 9, 1836.

ATTENDANCE OF MEDICAL WITNESSES AT CORONERS' INQUESTS,

UNDER THE NEW POOR-LAW SYSTEM.

To the Editor of the Medical Gazette.

SIR,

A SHORT time since I attended a coroner's inquest on a poor man, to whose assistance I had been summoned, as the nearest practitioner. His friends being unable to defray the charge, I applied for payment to the Board of Guardians; a copy of which application, together with their reply, I subjoin.

Surely, sir, this injustice to our profession cannot long exist. I earnestly trust that Mr. Warburton will succeed in obtaining an act of parliament, in the ensuing session, securing to us a fair compensation for attendance in similar cases.

Should you concur with me in the expediency of calling the attention of the profession to this subject without delay, an early insertion will oblige

Your obedient servant,
GEORGE MAY.

Reading, January 9, 1836.

To the Board of Guardians of the Reading Union.

GENTLEMEN,—I take the liberty to remind you, that some time since I made a demand of one guinea for attendance on the inquest of —— Taylor; and, at a subsequent personal interview, was favoured with your assurance that the case should be reported to the Poor-Law Commissioners, and that your decision would be guided by their opinion.

I take leave to recal your attention to the subject, and should you determine to refuse my claim, I hope I shall not be deemed presumptuous in requesting a statement of the Commissioners' opinion; as you will perceive that not merely this particular claim, but the interests of the medical profession in similar cases, are involved in their decision.

I have the honour to be, gentlemen,
Your obedient humble servant,
GEORGE MAY.

Reading, January 5, 1836.

Reading Union, Jan. 7, 1836.

Dear sir,—I have to inform you that the Board of Guardians, in conformity with the promise they gave you, made application to the Poor Law Commissioners for instructions for their guidance in respect to the charges of medical men for attendance upon coroners' inquests; and I am directed by the Guardians to transmit, for your information, the reply of the Poor Law Commissioners to that application.

"The Poor Law Commissioners for England and Wales have to acknowledge the receipt of your letter of the 19th inst. (December), and with reference to your inquiry upon whom it devolves to defray the demand of medical men for attendance upon coroners' inquests upon the bodies of paupers, the Commissioners desire to state that they are wholly unacquainted with any authority under which the payment of witnesses at coroners' inquests, or indeed of any other expenses attending inquests, can be charged upon the Poor rates.

"I remain, dear sir,
"Your obedient humble servant,
"THOS. G. CURTIES,
"Clerk to the Board.

"*To George May, Esq.*"

PRESENT STATE OF MEDICINE IN SPAIN.

As it may be some time before we receive the full report on the State of Medicine and the Medical Institutions of Spain, which is in preparation for us by a distinguished physician and scholar of that country, we present our readers, in the meanwhile, with the following short extract of a letter, just received from our

medical correspondent at Madrid, and which possesses a melancholy interest, from the illustration it affords of the miserable state of the medical profession in that fine but long-misgoverned country.

The medical profession in Spain, as regards its present condition, is in complete harmony with every thing else;—that is to say, it is in a state of revolution. Medical men belong, and with few exceptions have always belonged, to the Liberal party; and the class of *pure Physicians* consisted, almost to a man, of warm Constitutionalists, in the year 1820. By the term pure physicians we understand, in Spain, all those who belong to the universities, in contradistinction to those who belong to the colleges of surgery. When the Constitution was overthrown in 1823, the then absolute king, Ferdinand, incensed against the whole body of pure physicians, worked them all sorts of annoyance, and with much success, as you may believe. At last, and with the view of punishing them most effectually, he issued a decree, in which he commanded that no pure physician should be employed in the palace, the hospitals, or in any establishment under government; that is to say, in no public situation whatever, since in Spain all establishments are more or less under the control of the government; and, to supply their places, he converted surgeons into physicians by royal order, commanded that the colleges of surgery should be of medicine also, and created a great many young men at that time in the colleges *Physician-Surgeons* (Medico-Cirujanos, as they are called). The pure physicians, thus expelled from the court, hospitals, the establishments for mineral waters, &c., were compelled to bear their wrongs in silence till Ferdinand died. Since that event they have been in open war with the Board of Medicine, the members of which are the same physician-surgeons now to the queen as they were before to Ferdinand. Unfortunately for the pure physicians, the president of the Board, one of the physicians made by royal command, enjoys the confidence of the queen, and until lately has been able to resist the attacks, not only of the physicians, but even of the Cortes, by whom the edict of Ferdinand has been unanimously condemned. At length, however, in consequence of the late revolution, and consequent change of government, the president has resigned, and there is now a general expectation that the medical profession will be restored to order.

The present state of Spain is, as you may well believe, very adverse to the cultivation of science and literature: nobody thinks of any thing but politics. Six months ago we had only four medical journals in all Spain, and at present we have only two! The four journals were, *La Gaceta Medica* and *El Boletin de Medicina*, at Madrid; *La Biblioteca Medica*, at Zaragoza; and *Los Archivos Homiopaticos*, at Cadiz. The *Gaceta*, the journal of the physician surgeons, died two months since of inanition; and I believe the *Archivos* shared a similar fate; so that now we have only the *Boletin*, the organ of the pure physicians, and the *Biblioteca*, which has very few subscribers.—*Br. & For. Med. Rev.*, No. I.

RESPONSIBILITY OF PRACTITIONERS.—POISONING BY MISTAKE.

An army physician and an apothecary have been fined the sum of two and three hundred francs at Bruges, under the following circumstances. The physician had prescribed an emollient enema for one of his patients; but, as it was eleven at night, and the apothecary was in bed, he prepared it himself, with the assistance of one of the servants of the hospital. After having poured into the syringe part of the decoction of senna which he had made, he went to the dispensary, awoke the apothecary, and asked him for some linseed oil: the apothecary took down a bottle from a shelf, and put it on the counter. The physician took it away, and poured part of its contents into the syringe. Unfortunately he neither discovered, from the effervescence produced by the mixture, nor by the vapour, nor smell (all of which were remarked by the attendant), that the fluid was sulphuric acid. The clyster was given, and immediately afterwards the patient uttered the most horrible shrieks, and passed the night in acute pain: no remedies were availing, and he died.—*Journal de Chimie Medicale*, Aout 1835; *and Br. & For. Rev.*

MORISON'S PILLS.

An inquest was held at Birmingham on Tuesday (5th instant), on the body of a child 16 months old, the daughter of a carpenter, named Maullin, who died from great exhaustion, after having had several of Morison's pills administered to her by her father. Dr. Burt Davies, and other medical men, who examined the body of the child after death, said that the stomach and intestines were ulcerated, which they attributed to the action of drastic purgatives. The jury returned the following verdict:—"That Mary Hadley Maullin's

death was occasioned by Morison's pills having been improperly administered."—*Times*, Jan. 9, 1835.

THE SIAMESE TWINS.

This inseparable pair are now at Paris. M. Geoffroy de St. Hilaire congratulates the savans of the Academy of Sciences on the circumstance. Six years ago, he says, he applied in vain to the French government to allow this "teratological curiosity" to enter France. It may now be examined at leisure; and will be found deserving of attention, not only from the singular mode in which the individuals are united, but as presenting a specimen of a race of men little known to Europeans. M. Coste has visited these singular strangers, and raised rather a curious question about them; namely, at what epoch during uterine life their union took place? He has satisfied himself that it occurred *during the last days of the first month of pregnancy!* When the Twins were in London, about six years ago, they were minutely inspected by some of our ablest anatomists, and an account of the results will be found on record in our pages: see Med. Gazette, vol. 5.

URINARY CALCULI IN INSECTS.

M. Audouin has succeeded in analysing two minute calculi, found in what have been hitherto called—improperly, as it appears—the *hepatic* ducts of a stag-beetle (*Lucanus capreolus*). They were found to consist of uric acid: treated with nitric acid they yielded purpuric acid, characterised by its beautiful red colour on evaporation. The ducts in which these calculi were contained must, of course, in future, be considered as rather belonging to the urinary organs.—*Gazette Médicale.*

MEDICAL PRACTITIONERS IN PARIS.

It appears from the General Medical Almanac of Paris, just published, that the number of Doctors of Medicine or Surgery for the present year is 1220, being an increase of about 250 since 1833. Of the 1220 practitioners of Paris, there are 3 commanders, 34 officers, and 255 chevaliers, of the Order of the Legion of Honour.

CHAIR OF CLINIQUE EXTERNE.

Eight candidates dispute the chair left vacant by Dupuytren. Among them are MM. Sanson, Blandin, Berard, jun., and Lepelletier de Mans. Baron Richerand is president of the jury, and Lisfranc one of that body.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

January 14, 1836.

Henry Glasspoole, Watford, Herts.
Thomas Hankins, Faversham.
Robert Marsh Rendall, Merriott, Somersetshire.
John Langfield Burnard, Crewkerne.
John Brownfield, Norwich.
Charles Smith Meeke, Birmingham.
Stephen Hill, Handsworth.
John Darley Charles, Stow-on-the-Wold.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Jan.* 12, 1836.

Abscess	4	Hernia	1
Age and Debility	69	Hooping Cough	8
Apoplexy	14	Inflammation	25
Asthma	33	Bowels & Stomach	3
Cancer	1	Brain	3
Childbirth	2	Lungs and Pleura	14
Consumption	76	Liver, diseased	4
Convulsions	26	Measles	3
Croup	8	Mortification	4
Dentition or Teething	5	Paralysis	2
Dropsy	20	Rheumatism	1
Dropsy on the Brain	11	Scrofula	2
Dropsy on the Chest	1	Small-pox	10
Epilepsy	1	Stricture	2
Erysipelas	1	Thrush	1
Fever	7	Tumor	1
Fever, Scarlet	4	Worms	6
Fever, Typhus	1		
Gout	2	Stillborn	15
Heart, diseased	4		

Decrease of Burials, as compared with the preceding week . . . 67

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Jan. 1836.	Thermometer.		Barometer.	
Thursday . 7	from 29	to 40	29 92	to 29 88
Friday . . 8	29	41	29 88	29 95
Saturday . 9	25	34	29·91	29·82
Sunday . . 10	28	32	29·63	29 23
Monday . . 11	26	36	29·21	29·23
Tuesday . . 12	24	33	29·37	29·52
Wednesday 13	22	38	29·73	29·82

Prevailing winds, S.E. and S.W.

Except the 12th and 13th, generally cloudy; rain on the afternoon and evening of the 7th and 11th; and snow on the evenings of the 9th and 10th.

Rain fallen, ·85 of an inch.

Charles Henry Adams.

ERRATA.

In Dr. W. Philip's paper, in last number, page 574, last line of the note, *for* "1829," *read* "1833."

In the list of gentlemen who received diplomas in November, 1835, at the College of Surgeons, *for* "James F. Fitzgerald," *read* "James M. Fitzgerald."

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 23, 1836.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XVII.

EFFECTS AND USES OF CANTHARIDES.

Physiological effects of cantharides.—Cantharides are distinguished from all mineral and vegetable substances by a threefold operation on the body, namely, a vesicating effect when applied to the skin, a specific action on the urino-genital apparatus, and a powerful influence over the functions of the brain and spinal marrow. Many insects, some of which I shall hereafter notice, also possess these properties in common with cantharides; but I repeat, I know no vegetable or mineral substance which has all of them. Sundelin, who acknowledges that these animals hardly bear comparison with other remedies, says that perhaps Mezereon bark approaches nearest to them. But this substance has no marked action on the urinary and genital organs; while its operation on the skin and the nervous system is considerably less.

1. *Local effects.*— On all living parts these insects operate as powerful acrids or irritants, giving rise to violent inflammation, but without exciting any chemical influence, so that they do not deserve the name of *corrosives*, given them by some toxicologists.

When applied to the *skin*, the first effects noticed are, a sensation of heat accompanied by pain, redness, and slight swelling. These phenomena are soon followed by a serous effusion between the corium and epidermis, by which the latter is raised, forming what is commonly termed a *blister*, or, in the more precise language of the cutaneous pathologist, an *ampulla*, or *bulla*. The effused liquid has a pale yellow colour, with a very feeble taste and smell. Two analyses of it have been made.

Analysis by Dr. Bostock.

Albumen	6·00
Uncoagulable matter	0·14
Salts	1·00
Water	92·86
	100·00

Analysis by Brandes and Reimann.

Albumen	5·75
Animal matter, with muriate of ammonia, potash salts, carbonate, lactate, muriate, and sulphate of soda	0·26
Water	93·99
	100.00

If the cuticle be removed, the subjacent corium is seen intensely reddened, and, by exposure to the air, oftentimes becomes exceedingly painful. If irritants be applied, a secretion of pus takes place, and sometimes a whitish-looking false membrane is formed. Long-continued irritation occasionally causes tubercular granulations, arising, as Rayer says, from hypertrophy of the papillæ. Not unfrequently I have noticed ecthymatous pustules around the blistered surface; and in one remarkable case the whole body, but more especially the pectoral region (to which the blister had been applied), was covered with them. Sometimes the vesicles of eczema occur. Ulceration and gangrene are not uncommon: the latter effect is occasionally observed after exanthematous diseases, especially measles. I have seen death result therefrom in two instances.

The constitutional symptoms frequently produced are, excitement of the vascular system (as denoted by the increased frequency of pulse, heat of skin, and furred tongue), and irritation of the urinary and genital organs (marked by heat and pain in passing the urine, which is usually high-coloured, or there may be complete suppression.) It not unfrequently happens that the part to which a blister has been applied remains considerably darker coloured than the surrounding skin. Rayer states that the disappearance of these discolorations is hastened by the use of sulphurous baths.

When swallowed, cantharides act topically on the *gastro-intestinal membrane*; in poisonous quantities exciting inflammation of the mucous lining of the alimentary canal, accompanied with constriction and difficulty of swallowing, which is sometimes so great, that not a particle of fluid can be got into the stomach without the most inexpressible anguish: violent and burning pain, nausea, vomiting, frequently of bloody matters, sometimes with flakes like the inner lining of the alimentary tube, and great tenderness to touch. These phenomena sufficiently indicate the gastric inflammation. Ptyalism is not an uncommon occurrence. The enteritic symptoms are, abundant and frequent evacuations sometimes of blood, with horrible griping and burning pain, and exquisite sensibility of the abdomen.

The *volatile odorous matter* evolved by these insects is a local irritant; for it causes itching and even inflammation of the eyelids and conjunctiva, irritation of the air passages, marked by epistaxis, convulsive sneezing, &c.

Applied to *wounds*, cantharides act also as local irritants, causing active inflammation. In a dog, Orfila found that death took place therefrom in 32 hours.

2. *Remote effects.*—In addition to the local effects just described, cantharides produce disorder of parts remote from those to which the insects are applied. Thus, by inhaling the volatile odorous matter, as is done when persons sit under trees on which the animals are found, or by breathing the vapour of the decoction of cantharides, an affection of the urinary organs may be brought on. The same remote effects may also be excited by blisters, by handling the insects, by applying them to wounds, by swallowing them, or by injecting solutions of their active principle into the veins. We may classify the remote effects of cantharides into those observed in the urino-genital, the nervous, and the vascular systems.

(*a*) *Action on the urino-genital system.*—The pain in the loins, and the alteration in the quantity and quality of the urine, are the symptoms indicative of the inflamed condition of the kidneys. The burning pain and tenderness in the hypogastric region, and the constant desire to pass the urine, with the inability of doing so except drop by drop, are evidences of the vesical inflammation. The action on the genital organs in the male, is proved by priapism, which is sometimes accompanied by satyriasis, sometimes not; and by the occasional inflammation and mortification of the external organs. In the female, the action on the sexual system is shewn by the local heat and irritation, and by abortion.

(*b*) *Action on the nervous system.*—The affection of this system is proved by the pain in the head, disordered intellect, manifested in the form of furious or phrenitic delirium, convulsions of the tetanic kind, and subsequently coma. It is deserving of especial notice, that sometimes several days elapse before the nervous symptoms show themselves: thus, in a case related by Giulio, they appeared on the third day; in another instance mentioned by Graaf, on the eighth; and in a case noticed by Dr. Ives, they were not observed until the fourteenth day.

(*c*) *Action on the vascular system.*—The pulse becomes hard and frequent, the skin hot, and the respiration quickened; diaphoresis is occasionally observed.

In the account now given of the action of cantharides, I have disregarded the order in which the symptoms occurred, as also the modifications resulting from varying quantities, that I might the better fix your attention on the nature and locality of the effects. My next object is to examine the operation of different doses, and the symptoms, in the order of their occurrence. I may premise, however, that the susceptibility to the influence of cantharides is very unequal in different persons. Werlhoff mentions the case of a lad who used to be attacked with priapism and involuntary emission by merely smelling the powder. Amoreux says, in one case a pinch of the powder caused death; while in another a spoonful occasioned only slight heat in the throat and ardor urinæ. Dr. Hosack has mentioned an instance in which a man took nearly six ounces of the tincture, with the view of self-destruction, yet no dangerous symptoms followed. In contrast with this, I may instance a case that came within my own knowledge, where one ounce of the tincture produced serious symptoms. Orfila has seen twenty-four grains of the powder prove fatal.

1. *Action in small or medicinal doses.*—In very small quantities there are no obvious effects. If we increase the dose, a sensation of warmth is felt in the throat,

stomach, and respiratory passages, with increased secretion from the alimentary tube. By continued use, a tickling or burning sensation is experienced in the urethra, with frequent desire to pass the urine, which may or may not be altered in quality or quantity. In some cases diuresis is observed, in others not: in the latter the urine is generally higher coloured than usual. Occasionally the sexual feelings are excited.

2. *Action in larger doses: Subacute poisoning.*—The symptoms are here more marked. We observe heat in the throat, stomach, intestines, and respiratory passages; pain in the loins, burning sensation in the bladder, with frequent desire to evacuate the urine, which is sometimes bloody, and passed with difficulty. Painful priapism, with or without satyriasis. Pulse more frequent, skin hot, and the respiration quickened: the nervous system frequently excited.

3. *Action in still larger doses: Acute poisoning.* — The symptoms observed are, in part, common to other irritant poisons, in part peculiar to the vesicating insects. Violent burning pain in the stomach, with exquisite sensibility and constant vomiting; extreme thirst, dryness, and fœtid odour of the mouth, and not unfrequently ptyalism. Burning pain and spasmodic contraction of the bladder, giving rise to the most excruciating agony. Notwithstanding the incessant desire to void urine, nothing but drops of blood are passed, and with great pain. The constriction of the throat and difficulty of deglutition are most distressing and alarming: the unfortunate sufferer is constantly tormented with violent gripings, purging, generally of blood, extreme tenderness of the whole abdominal surface, faintings, giddiness, convulsions, and an almost hydrophobic aversion to liquids, with delirium terminating in coma.

The mode, and the immediate cause of death, are various: sometimes the nervous symptoms kill before gangrene makes its appearance; but more usually the patient dies from the inflammation and subsequent mortification of the alimentary tube or of the genital organs.

Cases.—You will find several illustrative cases detailed in Orfila's Toxicology, and references made to others, in the valuable work of Dr. Christison on Poisons. The following is taken from Hufeland's Journal:—

In 1820, four men swallowed some tincture of cantharides, mistaking it for a liqueur. In half an hour, nausea, vertigo, and a sensation of burning in the mouth and throat, had come on. Within an hour these symptoms had increased, with vomiting and violent pain in the bowels. Graaf being called in, found them in a most miserable condition. Their symptoms were, sense of suffocation, vomiting of blood, burning sensation in the œsophagus and alimentary canal, incessant thirst, with incapability of swallowing. Large pieces, apparently of the inner lining of the throat and gullet, were brought up. The abdomen was tense and painful. The remedies employed were warm baths, demulcent and oleaginous substances, and leeches. During the night they were restless and in great agony, but towards morning procured some sleep.

On the second day fever appeared: the thirst was more violent, and the deglutition difficult; the pain in the bowels was very severe, and the lips were sore. The tongue was deprived of its outer covering; the palate dark brown. As yet there was no retention of urine, though the desire to pass it became more frequent. Soon, however, strangury came on, and gradually increased. The remedies employed were camphor and emulsions internally, emollient clysters with opium, and frictions with camphorated oil. By these means, the difficulty and pain in evacuating the urine were alleviated

On the fourth day the patients were much better, though two of them yet suffered severely. In one, the secretion of urine seemed wholly stopped, clots of blood only being evacuated. The catheter was introduced, but without the smallest quantity of urine passing. A tepid injection into the bladder was used with advantage. Camphor and gum were given internally, while the oil of turpentine was employed externally. In 24 hours the urinary secretion was established, and in three weeks this patient had recovered.

One of the four (a man forty years old) soon lost the strangury, but was seized on the eighth day with violent nephritis, and such terrible phrenzy that four men were required to hold him. By bloodletting and the employment of leeches, giving camphor and calomel internally, and applying cold to the head, the symptoms disappeared in three days.

Although there are several reasons for believing that Toffania and the Marchioness de Brinvillier employed arsenical mixtures for poisoning, yet it has been supposed that they did not confine themselves to one poison, but used several; one of which has been stated to be a mixture of opium and cantharides. I must refer you, however, to Beckmann's article on secret poisoning, for further information on this point.

Lesions of texture.—On opening the bodies of persons poisoned by cantharides,

the usual appearances are inflammation of the alimentary tube, of the urinary and genital organs, and congestion of the cerebral vessels. In a man who died ten weeks after having swallowed some in a frolic, on the internal surface of the stomach were found "fungous tubercles, varices, erosions, and little ulcerations." In a female who died on the fourth day after taking two doses of twenty-four grains, the mouth and tongue were stripped of their lining membrane; the œsophagus, stomach, intestines, omentum, peritoneum, and internal parts of generation, were found inflamed. In a case related by Dr. Ives, "the blood-vessels of the brain and cerebellum were gorged, the cerebellum spread over with lymph, the villous coat of the stomach softened and brittle, and the kidneys inflamed and presenting blood in their pelvis." Bonnet, in one case, found ulceration of the bladder. It is deserving of remark, that inflammation of the urino-genital organs is more likely to be met with in patients dying within a few days after poisoning. Foster, in his experiments on dogs, frequently observed the particles of cantharides in the evacuated fæces. In my last lecture, I alluded to the fact that cantharides do not readily undergo decomposition, but have been recognized in a body after nine months interment. I have also pointed out to you the advantages to be derived therefrom in medico-legal investigations.

Effects of cantharidin. — Robiquet thus describes the effects of cantharidin:— The 1-100th part of a grain, placed on a slip of paper and applied to the edge of the lower lip, caused, in about a quarter of an hour, small blisters. A little cerate being applied, served only to extend the action over a larger surface, and both lips were in consequence covered with blisters. Some atoms of cantharidin, dissolved in two or three drops of almond oil, were rubbed over a small piece of paper and applied to the arm; in six hours a blister was formed, the size of the paper. The volatility of cantharidin at a comparatively low temperature, and the action of the vapour on the conjunctival membrane, are shown by the accident which happened to one of Robiquet's pupils, and which I mentioned in the last lecture. I have suffered once in preparing this substance. I applied one drop of an ætherial solution of impure cantharidin to the inside of the lower lip; but immediately afterwards, repenting of my temerity, I wiped it carefully off. In about an hour a blister had formed on the inside of the lip, and it was five or six days before the part had completely healed. Bretonneau, in his experiments on animals, has not found any marked aphrodisiac effect produced by cantharidin. He observed that this substance rendered the circulation slower and caused lethargy, which terminated fatally.

Modus operandi.—There are certain theoretical points connected with the operation of cantharides, worthy of notice.

1. *Theory of the local action.*—The topical effects of Spanish flies depend on cantharidin, and possibly on the volatile odorous oil also. These appear to act primarily on the nerves, and secondarily on the vessels of the part. Hence, in certain affections of the nervous system, blisters will not excite vesication. I have several times noticed this in apoplectic cases.

2. *Theory of the remote action.*—It has not yet been positively determined whether the remote effects of cantharides arise from absorption, or sympathy. The usual explanation is, that the active principle being carried into the blood, is thrown out of the system by the kidneys; that in passing through the renal vessels it irritates them, and thus excites nephritis; and that the urine, loaded with this acrid matter, coming in contact with the bladder, causes the vesical symptoms. But as the cantharidin has not yet been detected in the urine (for want of sufficient chemical characters to recognize it), the theory cannot be considered as proved. The principal argument in favour of it, is the powerful effect which blisters have on the bladder, and the influence of a piece of gauze, or tissue paper, placed between the blister and the skin, in preventing this. If the urine contain the cantharidin, I should think there would be no difficulty in detecting it by evaporating to dryness, and digesting the residue with æther. Let the ætherial solution be concentrated by spontaneous evaporation, and then try its vesicating properties.

Uses.—Hippocrates used vesicating insects (under the name of cantharides) internally; but the practice was subsequently regarded as dangerous, for, so lately as the year 1693, the President of the College of Physicians committed Dr. Groenfelt to Newgate for daring to employ them!!

1. *Local uses.*—Cantharides are frequently used as topical agents; sometimes as stimulants, sometimes as rubefacients, at other times as vesicants. Archigenes, and subsequently Aretæus, are the first persons recorded to have employed cantharides externally.

(*a*) *To stimulate topically.*—Tincture of cantharides with water (in the proportion of three or four drachms of the tincture to a pint of water) has been employed to stimulate ulcers; more especially sinuses and fistulous sores. It is used on the same

principle that stimulant and irritant applications are made to the eye in ophthalmia—that is, to excite a new action, which shall supersede the old one. We are told by Dr. Paris, that Matthew's once celebrated injection for fistula in ano, is a wash of this kind. In alopecia, or baldness, when this is not the result of old age, unguents of cantharides have been employed to promote the growth of hair. Powdered cantharides have been advised as an application to the parts bitten by rabid animals.

(*b*) *To produce rubefaction.*—For this purpose the tincture may be mixed with soap or camphor liniment; or when it is desirable to limit the effect to a particular spot, and especially if friction be objectionable, the common blistering plaster may be applied, allowing it to remain in contact with the part for an hour or two only. Rubefacient liniments are employed to excite the sensibility of the skin in numbness and paralysis; as also to produce local irritation in neuralgic and rheumatic pains. In the inflammatory affections of children, you will occasionally find it useful to employ the plaster merely as a rubefacient.

(*c*) *To excite vesication.*—A considerable number of substances (mineral, vegetable, and animal) cause vesication when applied to the skin. Horse-radish, mezereon, liquor ammoniæ, and acetic acid, may be mentioned as examples. To these may be added heat, applied in the form of hot water or a hot metallic plate. For facility of application, certainty of effect, and slightness of pain, no agents are equal to the vesicating insects; and these are now almost solely used.

It was formerly supposed that the efficacy of blisters was in proportion to the quantity of fluid discharged. It is hardly necessary I should say that this notion is incorrect; the truth being that the therapeutic influence is in proportion to the local irritation, and has no more relation to the quantity of fluid discharged, than that the latter is frequently (not invariably) in the ratio of the former. Stoll's axiom is therefore correct:—"Non suppuratio sed stimulus prodest." As to the precise manner in which blisters, or indeed any remedies, influence diseases, we are quite in the dark. We are accustomed to refer their operation to the principle of counter-irritation, already discussed in a previous part of the course. I must refer those who feel interested in the question, whether blisters ought to be applied in the neighbourhood of, or at a distance from, the affected part, to a paper by Barthez, in the *Recueil de la Société Médicale de Paris*. In this country we generally apply them near to the morbid part; to which practice Barthez assents, with some exceptions.

We employ blisters in inflammatory diseases, both acute and chronic; in the former, however, preceding their use by blood-letting. In chronic inflammatory diseases we often employ what is termed a perpetual blister—that is, remove the cuticle from the ampulla, and dress with savin or cantharides ointment. This practice is advisable in chronic diseases of the chest, of the joints, of the eyes, &c. Blisters are sometimes useful in erysipelas; thus to localize the disease, when disposed to spread, and as a revulsive applied to the feet, in erysipelas of the head. A blister to the perineum has been sometimes found beneficial in gleet.

It is hardly safe to apply blisters to children immediately after exanthematous diseases, sloughing being not an unfrequent result. If you want to cause counter-irritation, the best plan is to dilute the common blistering plaster, by mixing it with three times its weight of soap cerate. I have seen this frequently employed, but never observed any unpleasant results. Another plan sometimes adopted is to apply a common blister, for an hour or two only, so that it shall merely produce rubefaction.

2. *Remote uses.*—These will require examination under distinct heads, according to the particular object we have in view in employing cantharides.

(*a*) *To act specifically on the urinary organs.*—Sometimes we employ cantharides to act specifically on the kidneys and bladder. Thus in dropsy they have been used to excite diuresis, though they frequently fail in producing this effect. In fact, both Dr. S. Carmichael and Dr. Cullen state they never succeeded with the remedy. In that curious and intractable disease, diabetes, cantharides have been employed, but without apparent benefit. In paralysis of the bladder they are frequently useful, when there are no marks of local irritation. You are no doubt aware that either of two opposite conditions may be the result of paralysis of this organ, namely, retention and incontinence of urine. The latter condition is not unfrequently met with in children, and is very likely to be relieved by cantharides. We are told that these insects are particularly serviceable in that species of incontinence which occurs during sleep only; but I have seen them cure the disease during day, and fail in giving relief at night. The case alluded to was that of a boy, 14 years old, who had been subject to incontinence of urine since his infancy. He was a robust lad, and apparently in the most perfect health. I put him under the influence of gradually-increased doses of

tincture of cantharides, and within two months he was enabled to retain his urine by day, but it still passed involuntarily at night; and though he continued the remedy for a considerable time, no further benefit was obtained. You will also find cantharides useful in that form of incontinence of urine which occurs after lingering labours from the long-continued pressure of the child's head. You must not, however, commence the use of this remedy until all symptoms of local irritation have subsided.

(*b*) *To act on the organs of generation.*—In consequence of the specific stimulus exerted by cantharides on the bladder, it has been supposed that the effect might be extended to the uterus; and thus they have been employed as stimulating emmenagogues, in some cases with apparent benefit, but frequently without any obvious effect. Abortion has occasionally happened from their employment, as I have myself witnessed in one case.

Cantharides are also employed as an aphrodisiac, both in man and other animals (as horses, heifers, and asses). In man, if given in sufficient quantity to affect the sexual feelings, it endangers the patient's safety. Most of the cases in which we are requested to administer aphrodisiacs, will be found, on examination, to require moral rather than pharmacological treatment. In discharges from the genital organs, beneficial effects are frequently obtained by the internal use of cantharides. In gleet it has been often found serviceable. Mr. Roberton, who has published a work on its use in this complaint, explains its efficacy by saying, it excites a mild inflammatory action in the urethra, which supersedes the previous morbid one: this is shewn by the discharge becoming thick, opaque, and puriform. Mr. Roberton says cantharides are equally serviceable in leucorrhœa: in this I think he is wrong.

(*c*) *In chronic skin diseases.*—Pliny states that cantharides (*Mylabris cichorei*) were employed in a disease which he terms lichen. At the present time, tincture of cantharides is not unfrequently employed in lepra, psoriasis, and eczema. Having found other remedies very successful in lepra and psoriasis, I have rarely had occasion to try cantharides; but Rayer, in his elaborate treatise on the Diseases of the Skin, says, "Of all the energetic and dangerous remedies that have been used in lepra, the tincture of cantharides is, perhaps, that which has the most remarkable influence over the disease. The great objection to its employment is its liability to excite inflammation in the digestive organs and urinary passages, especially among females, which necessitates the immediate suspension, and occasionally the entire abandonment of the medicine." Biett has found it successful in chronic eczema, as well as in the scaly diseases.

(*d*) *In diseases of the nervous system*, cantharides were at one time in great repute. The cases in which they were employed were hydrophobia, epilepsy, chorea, tetanus, and mania. I need hardly say experience has shewn they deserve little attention in any of these complaints. If, however, you should be inclined to try them, recollect that they excite the nervous system, and are, therefore, better adapted to those cases accompanied with a deficiency rather than an excess of vascular action.

(*e*) *In obstinate sores*, Mr. Roberton has employed cantharides on the same principle as he uses them in gleet.

Preparations.—It is, I conceive, quite unnecessary for me to give a description of all the preparations that have been or may be made of cantharides. I must refer you to Jourdan's *Pharmacopée Universelle* for a most complete account of all those at present used in Europe, and shall confine myself to the preparations employed in this country.

1. *Tincture of cantharides.*—If you wish to exhibit cantharides internally, by far the safest plan is to extract the active principle by some solvent. Now æther, alcohol, water, proof spirit, or oil (fixed or volatile), may be employed for this purpose. In this country, a tincture made with proof spirit is used. The strength of this preparation varies in the three British Pharmacopœias,—a circumstance much to be regretted. In the London Pharmacopœia, the proportions are three drachms of cantharides to two pints of proof spirit. The dose of this preparation for an adult is ten or twelve minims, gradually increased to a drachm. You had better exhibit it in some demulcent liquid, as linseed-tea or barley-water, always carefully watching the effects on the system, more especially on the urinary organs.

2. *Plaster of cantharides.*—This is the well-known *blistering plaster.* Here again the formulæ of the three Colleges are not uniform in their strength. In the London Pharmacopœia, 1 part of cantharides is employed with $2\frac{1}{2}$ parts of a base (composed of wax, suet, lard, and resin). I have already alluded to the practice of some druggists in diminishing the weight of the cantharides, and substituting powdered euphorbium. In making blistering plaster, it is necessary to be aware, that if the cantharides be added to the base while the latter is very hot, their vesicating power is diminished, and hence they are never to be mixed in until the other in-

gredients are nearly cold. This plaster is usually spread on leather by the thumb (a heated spatula being objectionable, for the reason already mentioned). To prevent the blister moving, its margin is to be covered with adhesive plaster. And in order to guard against any affection of the urinary organs, place a piece of gauze or silver paper between the plaster and the skin. The efficacy of the blister depends on the fatty matter dissolving the cantharidin and transuding through the muslin or paper. Some recommend the paper to be soaked in oil, which is supposed to dissolve the cantharidin. Now oil, not being miscible with the blood, is not readily absorbed; and hence, it is supposed, arises its protective influence.

The usual time requisite for a blistering plaster to remain in contact with the skin is twelve hours; the vesicle is then to be cut at its most depending part, and dressed with spermaceti ointment. When the irritation caused by these plasters is excessive, it is sometimes necessary to substitute a poultice for the ointment. When we wish to make a perpetual blister, the cerate of cantharides is employed as a dressing; or if we wish to excite less irritation, and prevent the possibility of the urinary organs being affected, the cerate of savine.

3. *Cerate or ointment of cantharides.*—In the London Pharmacopœia, this is prepared by adding one part of powdered cantharides to six parts of spermaceti cerate. But the Dublin College order an ointment composed of a decoction of cantharides and ointment of white resin, evaporated to a proper consistence. This is a less irritant application than that in the London Pharmacopœia. Both of them are employed to excite a purulent discharge from blistered surfaces.

4. *Ointment of cantharidin.*—According to Thierry a very active ointment is made by incorporating one grain of cantharidin with one ounce of lard. In order that the active principle may be properly mixed, it should be dissolved in alcohol. A little essential oil may be added, to render the ointment odorous.

Method of treating cases of poisoning by cantharides.—In all cases of poisoning, the first object is to expel the poisonous substance from the stomach. It is not, however, always necessary, or even proper, that, to attain this object, we should use the stomach-pump, or exhibit emetics. When the poison is a powerful irritant, it causes inflammation of the stomach and vomiting; and when these effects have taken place, emetics can only add to the irritation, without being serviceable. Sometimes it is impossible to introduce the tube of the stomach-pump; and even if it were practicable to effect this, we should only increase the patient's danger. These remarks apply particularly to the treatment of poisoning by cantharides, in which both the stomach-pump and emetics are to be prohibited, unless, indeed, the accident shall have been discovered before the vomiting has commenced, in which case emetics may be exhibited, and the throat tickled to excite sickness. No antidotes are known. Oil was at one time thought to be an excellent remedy, but subsequent experience is against this opinion. Cantharidin is, as I have before mentioned, very soluble in oil; and hence, instead of alleviating, it has been supposed to add to the patient's danger, by extracting the activity of the flies, and thereby assisting their operation on the stomach. This theoretical view of the subject, first broached, I believe, by Pallas, seems supported by experience. Orfila found that cantharides macerated in cold oil, and afterwards given to dogs, killed them in a few minutes; and Dr. Christison says, "The case mentioned in the Genoa memoirs was evidently exasperated by the use of oil." I confess, however, I think farther experience is required to determine the hurtful consequences of employing oil; for as the editors of the "*Dictionnaire de Matière Médicale*" very properly observe, on the same principles that oil is prohibited, mucilaginous drinks ought also to be proscribed, since cantharidin, aided by the yellow matter, dissolves in water; and on the other hand oil, in some cases, has appeared to be beneficial. White of egg, with water, milk, linseed tea, and other demulcents, have been recommended. Oleaginous and mucilaginous injections into the bladder are advised, in order to relieve the vesical symptoms. In addition to these means, blood-letting, general and local, opium, and the warm bath, must be resorted to.

Other Coleopterous Vesicants.

I have already several times alluded to other vesicating insects besides the common Spanish fly employed in this country. They belong to four genera, namely, *cantharis* (or *lytta*), *meloe*, *mylabris*, and *lydus*.

I. The genus Cantharis, or *lytta*, contains the following species, which have been employed as vesicants:—

1. *Cantharis vesicatoria*, already described. Though very uncommon in this country, it is regarded as indigenous; and is accordingly described and figured in Donovan's "*British Insects*," and enumerated in Stephens's "*Catalogue of British Insects.*"
2. *C. vittata*, or *potatoe fly* of North America. It contains *cantharidin*, and has been frequently employed as a sub

stitute for the first-mentioned species. It is officinal in the "*Pharmacopœia of the United States.*"

3. *C. atrata* is also a native of North America, and is mentioned as a blistering insect.
4. *C. marginata*, an American insect, possessing vesicating properties.
5. *C. cinerea*, another North American blistering insect.
6. *C. ruficeps*, a native of Sumatra and Java, possessing, according to Waitz, extraordinary blistering properties.
7. *C. gigas*, a native of Guinea and the East Indies. It is described by Pfaff as *Lytta cærulea*.
8. *C. violacea*, a native of the East Indies. This is the *Lytta gigas mas* of Buchner.
9. *C. atomaria*, a native of the Brazils, where it is said to be employed.
10. *C. syriaca*, or *Lytta segetum*, is probably the *green cantharides*, said by Forskal, in his *Materia Medica Rahisina*, to be employed in Arabia.

II. The genus Meloe (*May worm*, or *Oil beetle*) contains also a large number of species which have been employed medicinally; some of them are indigenous. In the 11th volume of the Transactions of the Linnæan Society, you will find a description of the British species, by Mr. W. E. Leach.

1. *M. majalis*, or *true May worm.*
2. *M. proscarabeus.* This is the *Proscarabeus vulgaris* of Stephens, and is a native of this country. Both this and the preceding species are found in Portugal, Spain, France, &c., and are officinal in several of the European Pharmacopœias. Their operation is similar to cantharides, and they are believed to contain the same active principle. *M. proscarabeus* has produced death in two instances.

III. The genus Mylabris contains two species deserving of notice:—

1. *M. cichorei.* This is a native of the East Indies and China. I have already stated the reasons for believing that it was the insect mentioned by Dioscorides and Pliny, and perhaps also by Hippocrates, under the name of κανθαρις. It contains the same active principle, and is quite as powerful, as our common blistering fly (*Cantharis vesicatoria.*)
2. *M. sidæ*, or *M. pustulata*, is a native of the Cape of Good Hope. It is said to be employed in China.

IV. The genus Lydus yields one species (*L. trimaculatus*), which is said by Referstein to be employed in the northern parts of Europe as a blistering insect.

ON THE

NERVOUS SYSTEM;

Being a Lecture, delivered at the Aldersgate School of Medicine, on Jan. 6, 1836,

By Marshall Hall, M.D. F.R.S., &c. &c.

Gentlemen,—I am about to bring before you an important class of diseases,—that of the Diseases of the Nervous System.

I have, for some years, been prosecuting this subject; but I find it so extensive, that, even after so considerable a period, I am only enabled to present you with a slight *sketch* of the plan and objects of my investigations, the results of which will, in due time, be submitted to the Royal, and Royal Medical and Chirurgical, Societies.

I have endeavoured to pursue this inquiry through the medium of anatomy, physiology, and pathology; but my aim has also continually been to improve the diagnosis and practice. I think that the science and art of medicine are by no means incompatible acquirements, and that the boast of being a mere practitioner should cease to be a cloak for ignorance and indolence. To pretend to understand the diseases of the nervous system without an intimate knowledge of its anatomy, physiology, and pathology, is the height of folly and presumption. The *symptoms* of these diseases can be justly interpreted by the anatomist, the pathologist, alone; and an accurate knowledge of the symptoms is absolutely necessary to practice: it is the principal source of the diagnosis, and our constant guide in the administration of remedies.

Nor is a knowledge of the structure and functions, the anatomy and physiology, of the nervous system, sufficient alone, for the understanding of the diseases of this system. The nervous system does not exercise its functions uninfluenced by the other systems of the animal economy, or by the general system as a whole. The influence of these upon the nervous system must be clearly understood, before the view of the subject can be said to be complete.

Indeed, the entire economy of the human frame, although constituted by various systems, is one and indivisible, and it is impossible that one of those systems should be morbidly affected without the participation of the rest. In order to accommodate this difficult and complicated subject to our limited understanding, it has been found necessary, however, in the pursuit of medical science, to proceed ana-

lytically, and to imagine the different systems of which the whole animal economy is composed to be separated so as to admit of distinct examination; and we speak of the nervous, the circulatory, the respiratory, the digestive, the urinary systems, &c. as distinct objects of inquiry. We are compelled, however, afterwards, to review the subject synthetically, and to consider the influences of one or more of these systems, or of the general system, upon the other systems, respectively.

On the present occasion I proceed to treat, in this manner, of the nervous system; or of that system by means of which we are connected, through sensation and perception, volition and voluntary motion, with the external world;—that system by means of which the *ingesta* and *egesta* of the animal economy are regulated;—that system by the agency of which, out of the ingesta, the various organs, limbs, &c. are formed and nourished.

The first objects which will occupy us will be the anatomy and physiology of the nervous system: we shall then consider its pathology. This will naturally lead us to consider the distinct diseases to which this system is liable. We shall then trace the influence of morbid states of the other systems taken individually, and of the general system, upon the nervous system. This plan will be pursued both in regard to the adult and the infant.

I.—*Of the Anatomy of the Nervous System.*

I believe all anatomists have divided the nervous system into the cerebro-spinal and the sympathetic. The first of these is represented in this plate by M. Manec: it consists, first, of the cerebrum and cerebellum; secondly, of sentient nerves, which pursue their course to them, and of motor nerves, which proceed from them, either along the base of the brain or along the spinal marrow, in every external part of the animal frame. The second is partly represented in this other plate, by the same anatomist: it comprehends the internal ganglionic or sympathetic.

To these two subdivisions of the nervous system, I believe a third must be added, before our views of that system can be considered as at all complete;—it is one which I claim the merit of first pointing out in all its fulness. Suppose the cerebrum and cerebellum, the *centre* of the first subdivision of the nervous system, and the ganglionic, or the second subdivision of this system, removed, *this remains.* It consists of the *true* spinal marrow, distinguished from the sentient and motor nerves, which run along its course, as an *axis* of *excitor* and *motor* nerves. It is the seat of a peculiar series of physiological phenomena, and of a peculiar class of pathological affections. In the former are included *all* the funtions which relate to the immediate acts of *ingestion* and *egestion;* to the latter, *all* spasmodic diseases.

According to this view of the subject, instead of dividing the nervous system into—

I. *The cerebro-spinal*, and
II. *The ganglionic* or *sympathetic*,

I would propose to divide it thus:

I. *The cerebral*, or *the sentient* and *voluntary;*
II. *The true spinal*, or *the excito-motory;* and
III. *The ganglionic*, or *the nutrient*, *the secretory*, &c.

I think, too, that there is good reason for viewing the fifth and posterior spinal nerves as constituting an external ganglionic nerve, for the nutrition, &c. of the external organs; so that I would further propose to subdivide the ganglionic subdivision of the nervous system into—

1. The *Internal*, comprising
   1. *The sympathetic;*
   2. *The vagus?*
2. The *External*, comprising
   1. *The fifth;*
   2. *The posterior spinal.*

I must first observe, that the designation cerebro-spinal is incorrect. It comprises *two* subdivisions of the nervous system, which must be distinguished from each other, and of which the cerebrum and the true spinal marrow are the respective centre and axis. I shall proceed to treat of each of these.

The first comprises every part of the nervous system which relates to *sensation* and *volition*, the nerves of *sense*—the olfactory, the optic, the auditory, the gustatory, the nerves of touch, and the whole of the nerves of voluntary motion. Its centre is the cerebrum, including the cerebellum; its sentient nerves run variously from the organs of sense, and from the *external* surfaces, first *without* the cranium or spine, and then *within* the cranium or spine to that centre; its voluntary nerves pursue a similar but retrograde course *from* that centre to the muscles of voluntary motion.

A peculiar set of nerves constitute, with the true spinal marrow as their *axis*, the second subdivision of the nervous system. As those of the former were distinguished into sentient and voluntary, these may be distinguished into the excitor and motory. The first pursue their course principally from *internal* surfaces, characterized by peculiar excitabilities, *to* the true medulla

oblongata and spinalis; the second pursue a course from that medulla to muscles having peculiar actions concerned principally in ingestion and egestion. The motions connected with the former, or cerebral subdivision, are sometimes, nay, frequently, *spontaneous*; those connected with the true spinal are, I believe, *always excited*.

The anatomy of this subdivision is still little known. I propose the subject to myself for investigation. I will merely venture, on the present occasion, to present you with a brief outline of it, by way of example.

I. The *excitors* belong chiefly, perhaps entirely, to—

1. *The fifth*;
2. *The pneumo gastric*; and
3. *The posterior spinal nerves.*

1. The excitor branches of the first of these are distributed—

1. *To the eye-lid*;
2. *To the nostril*;
3. *To the fauces**;
4. *To the face.*

2. The excitor branches of the second—

1. *To the larynx* †;
2. *To the pharynx*;
3. *To the lungs*;
4. *To the stomach.*

3. The excitor branches of the third—

1. *To the anus*;
2. *To the cervix vesicæ*;
3. *To the cervix uteri*;
4. *To the general surface of the body.*

II. The motor branches are distributed to—

1. *The orbicularis*;
2. *The larynx*;
3. *The pharynx*;
4. *The muscles of respiration*;
5. *The sphincters*;
6. *The ejaculators*;
7. *The general muscular system.*

These arrangements will be perfectly intelligible to you when I come to speak of the physiology; but before I proceed to that part of my lecture, I must draw your attention once more to the third subdivision of the nervous system, or the ganglionic.

The *internal* ganglionic includes that usually denominated the sympathetic, and probably the vagus. The external ganglionic embraces the fifth and the posterior spinal. The arguments for these divisions are principally physiological; they will be given, therefore, in that division of my subject to which I now proceed.

* The naso-palatine; the pharyngeus?—See Bellingeri; Arnold.

† The *internal* laryngeal.

II.—*The Physiology of the Nervous System.*

In order to convey a distinct idea of my views of the nervous system at once, I shall begin by the detail of one experiment and the performance of another.

A horse was struck with the poll axe over the anterior lobes of the brain. It fell instantly, as if struck with a thunderbolt, was convulsed, and then remained motionless. It shortly began to breathe, and continued to breathe freely by the diaphragm.

When lacerated or pricked by a sharp or pointed instrument, as a strong *pin*, on any part of the face or surface of the body, it was totally motionless, manifesting no evidence of sensation or volition.

When, on the other hand, the *eye-lash* was touched with a *straw*, the eye-lid was forcibly closed by the action of the *orbicularis*. When the cornea was touched, the eye-ball was drawn outwards by the *abducens*. When the verge of the anus was touched, the sphincter contracted forcibly, the tail was raised, the vulva drawn towards the anus.

The upper part of the medulla oblongata was now destroyed by an instrument passed through the orifice made by the poll-axe: there were violent convulsions; the respiration ceased, and the eye-lid and eye-ball remained motionless on the application of stimuli.

Now I imagine that it will not be disputed that the blow of the poll-axe, in this case, annihilated the cerebral or sentient and voluntary functions; and that a peculiar set of excito-motory phenomena remained. Deep lacerations produced no evidence of the former; the touch of a straw induced a full manifestation of the latter. The destruction of the medulla oblongata removed all trace of excito-motory phenomena in the eye-lid and eye-ball.

Now you observe here this living frog. Its sentient and voluntary functions are obvious. I divide the spinal marrow, below the occiput, with these scissors: all is still. Not a trace of *spontaneous* motion. The animal would remain in this very form and position, without change, until *all* signs of vitality were extinct. But now I pinch a toe with the forceps. You see how both posterior extremities moved. All is now still again; there is no spontaneous motion, no sign of pain from the wound made in the neck. It is without sensibility—without volition; the *power* to move remains—the *will* is extinct. I now pinch the integument. You observe the

result—the immediate occurrence of excito-motory phenomena.

I now destroy the whole spinal marrow with this probe. It is in vain that I pinch the toes; the integuments, the limbs, are motionless. Could the former *excited* motions be those of irritability? I will try the truth of this suggestion by seeing whether, now that the axis of the excito-motory system is destroyed, with its phenomena, the application of a slight galvanic shock will prove the subsistence of irritability. You see how instantaneously and forcibly the muscles are stimulated to contraction.

Is not the proof, from this experiment, of the distinction between the motions of volition, of the excito-motory system, and of irritability, perfectly and unequivocally complete?

I must now proceed more systematically with my lecture.

I shall not detain you long with the physiology of the cerebral subdivision of the nervous system: it embraces sensation, perception, volition, and voluntary motion. The senses are the smell, the sight, the hearing, the taste, and the touch; they convey to the mind all we know of the external world. Perception is derived from them. Volition is a subsequent mental act, and voluntary motion a frequent result. And thus the motions which result from sensation always *imply volition;* but as volition may exist without any previous sensation, the voluntary motions are frequently *spontaneous.* It is by this character that the motions which belong to the sentient and voluntary system are distinguished from those which belong to the excito-motory. These are never spontaneous: they are *always excited.* Even the motions of respiration, as far as they belong to this system, are excited motions, as I shall shew immediately. Legallois, M. Flourens, Sir Charles Bell, are equally in error when they consider the medulla oblongata as the *source* of the respiratory motions; it is the *channel* through which the excitors act, and the organ which *combines* the different movements which constitute the acts of respiration. Equally remote from the truth, I think, is the opinion of Dr. Philip and Mr. Mayo, that the acts of the respiration are entirely voluntary. This is, in fact, a mixed function, as all the acts of the excito-motory system may be; and although generally belonging to the excito-motory system, yet capable of being affected through the medium of volition. This subject will be pursued hereafter.

A point which belongs more immediately to our present subject (the cerebral system) is that of the influence of the senses over the acts of volition. There is a most interesting case of anæsthesia, published by Dr. Yelloly, in the Transactions of the Medico-Chirurgical Society. The patient could hold a cup in her hand securely, if she kept her eyes fixed upon it; but if she ceased to look at it, it fell to the ground. I have this day seen a patient with a slight degree of paralysis of feeling and of voluntary motion in his lower limbs. He walks safely whilst his eyes are fixed upon the ground, but stumbles immediately if he attempts to walk in the dark. His own words are, "My feet are numb; I cannot tell in the dark where they are, and I cannot poise myself." The voluntary motions are regulated by the sense of touch, when this is unimpaired, or by that of sight, when the touch is paralyzed.

Many attempts have been made to *localize* the functions of the cerebrum; that is, to prove certain functions to be attached to certain parts of that organ, without, however, much success. Affections of one hemisphere paralyze the *opposite* side of the body only; so do affections confined to one side of the cerebrum, and of the tuber annulare: in the last case the paralysis is said to be confined to the *motions**. Affections of the medulla oblongata and spinalis convulse or paralyze parts situated on the *same* side of the body.

An injury inflicted upon the cerebrum or cerebellum, in experiments, induces paralysis, but not convulsions; but if the medulla be touched, either convulsion or paralysis may be the immediate effect, according to the degree of the injury.

If the medulla oblongata be seriously injured, the most instantaneous death is the consequence. Injuries to the lower parts of the medulla spinalis affect the sphincters, and the organs of generation.

I now proceed to treat of the acts of the excito motory system. As the designation of this implies, there is always the application of an appropriate stimulus, or cause of excitement or irritation; this is followed by the contraction of peculiar sets of muscles. It is clearly proved that the influence of the stimulus is carried along an excitor and incident nerve, to the medulla oblongata or medulla spinalis, and that it is reflected thence along other reflex or motor nerves. The incident excitor nerves, the medulla, and the reflex motor nerves, constitute the system. They remain, as I have already stated, after the centre of the cerebral system has been removed by experiment, or destroyed by disease.

I have already detailed the experiment of a horse, felled and reduced to a state of

* Ollivier, Traité de la Moelle épinière, Ed. ii. p. 527.

insensibility by the poll-axe. No infliction upon the skin, by a pin or other pointed instrument, was felt—no voluntary motion induced; but the touch of a straw, applied to an eye-lash, or the cornea, immediately induced a firm contraction of the orbicularis palpebræ. There can be no doubt that a filament of the first branch of the fifth pair of nerves, or trifacial, conveyed the impression to the medulla oblongata, and that a filament of the seventh pair, or facial, reconveyed it from the medulla oblongata to the orbicularis. All this is wonderful, and, I believe, quite unknown to physiologists. But the light touch of the same portion of straw applied to the verge of the anus, induced a firm contraction of its sphincter. In this case the physiology is similar; but the anatomy is unexplored. Nerves which arise from the verge of the anus take their course to the spinal marrow; from whence some mysterious influence is returned to the sphincter muscle.

Apoplexy and hydrocephalus are subjects of our daily observation: even *their* symptoms are unintelligible to any but an anatomist. In the former there is perfect coma—blindness, deafness, perhaps insensibility to impressions which would, in other circumstances, be productive of pain; yet the patient breathes, and the sphincters still do their office: one thing only is observed — the due ratio which usually subsists between the number of respirations and of the pulsations of the heart, is lost. In hydrocephalus, or the hydrocephaloid disease, there may be a dilated pupil, with total blindness; yet, although the eye remains unclosed whilst the finger or any other object approaches the cornea, a touch of the tip of an eye-lash immediately induces the closure of the eye-lids! At length, with augmented disease, this phenomenon ceases in its turn. The excitor nerve of the eye-lash loses its excitability, and the motor of the orbicularis its motor power; the eye lids are unmoved on touching the lash, and are permanently but half closed. These are interesting facts, for they not only denote the seat and the nature, but the degree of the disease.

Filaments of the fifth pair of nerves are the excitors distributed upon the border of the eye-lid and surface of the eye-ball,—upon the nostrils, probably upon the fauces, —certainly the face,—and are the *first* agents in inducing closure of the eye lid, sneezing, vomiting, and sobbing, when the eye-lash is touched, the nostrils stimulated, the fauces irritated, and cold water is dashed upon the face. Other nerves convey the reflex influence from the medulla oblongata to the orbicularis, and the various respiratory muscles whose actions are combined in the acts of sneezing, vomiting, and sobbing.

Filaments of the pneumo gastric are the excitors, when carbonic acid gas, or a drop of water, comes in contact with the larynx, —when the dust of ipecacuanha is inhaled in the bronchia with the effect of inducing asthma,—in deglutition,—in ordinary respiration,—and in the act of vomiting produced by antimony in the stomach and calculi in the gall-duct or ureter.

There are several interesting and peculiar facts connected with the excito-motory system, which I must briefly mention. If the fifth nerve in the fauces be irritated, vomiting is induced; if, on the contrary, the eighth in the pharynx be excited, the act of swallowing follows. It has happened, that when a patient has wished to excite vomiting by tickling the fauces with a feather, he has, by passing the feather down, induced such an action of the muscles of deglutition, as has carried the feather into the œsophagus. There are two cases of this kind in the Medical Observations and Inquiries, vol. iii. p. 7, and vol. ii. p. 231. A similar event has occurred with regard to the female catheter: certain nerves being excited on introducing this instrument, an action of the muscles has been induced, which has drawn the catheter out of the grasp of the surgeon into the bladder. I show you a catheter which was extracted from the bladder after such an accident. The very case is detailed in the "Medical Facts and Observations," vol. i. p. 96. Such cases are not rare even. My friend Mr Toogood, of Bridgewater, has been called to two such accidents, and extracted the catheter by the urethra. The *cause* of the occurrence had not been pointed out before. I believe a similar event has taken place in regard to the rectum.

Spinal nerves are excitors and motors in regulating the action of the sphincters, and of the ejaculatores seminis.

This important part of the nervous system entirely presides over ingestion and egestion—over the ingestion of air, of food, &c., and over the egestion of the fæces, the urine, the semen, the fœtus. In all, the presence of an excitant is essential: the act of deglutition cannot be effected without the contact of some substance with the pharynx, although the touch of the finger be sufficient. A similar remark applies to the ejaculatory muscles.——

But this subject is best illustrated by adverting to the important function and acts of the muscles of respiration. Legallois, M. Flourens, and Sir Charles Bell, have erroneously considered the acts of the respiration as spontaneous, and the medulla oblongata as their source and *primum*

mobile. I think I have, on the contrary, proved by experiments that this important function consists of excited acts, *excited* through a peculiar system of *excitor* nerves, and principally filaments of the fifth, the pneumo-gastric, and the spinal nerves. You dash cold water on the face of your patient, you pinch the pneumo-gastric in an experiment, or you sink gradually into the sea in bathing; in each case an act of inspiration is excited. Compare these nerves, too, at their origin, and you will find that they all agree in one remarkable character—that of being formed of numerous distinct filaments. A thousand other facts demonstrate the same thing: a drop of water fallen into the larynx; tickling the soles of the feet affect the muscles of the respiratory system so as to threaten death by asphyxia. The medulla oblongata*, and not the pneumo-gastric nerve†, *combines* the action of the different muscles together into acts of respiration, &c.

I perfectly agree with Sir Charles Bell in the opinion that the respiratory is perfectly distinct from the other subdivisions of the nervous system; but I venture to differ from him in viewing the respiratory as but *a part* of a more extensive system—as an *excited* and not a *spontaneous* function —as *originating*, when the cerebrum is removed, in the par vagum *as its excitor*, and not in the medulla oblongata.

But I must hasten on. I will close my remarks on this subject by stating, that however distinct the cerebral and true spinal subdivisions may be, these exert an influence upon each other which is essential to the well-being of the individual. The anencephalous fœtus, though it may be born alive, and even live for some hours, is not *viable*; it must *soon* die. Apoplexy and hydrocephalus destroy the patient, by destroying the cerebral functions merely. During sleep even, although this be *chiefly* an affection of the brain, the functions of the true spinal marrow are somewhat impaired; the respiration is noisy, frequently slightly stertorous, and irregular. Yet the respiration does proceed, acts of deglutition take place, and the sphincters do their office. Still a marked distinction between the cerebral and the true spinal functions is, that the former are partly suspended in *sleep*, and entirely in *coma*, whilst the latter are unimpaired: in sleep and in coma the eyelash is susceptible to the slightest stimulus, and the orbicularis, the sphincter of the eye-lid, and the other sphincters, with the muscles of the larynx and of the respiration, do their office. This state of things cannot last long, however, because the integrity of the cerebral functions is essential to the continuance of the true spinal and the other functions of the animal economy. Hence the fatal omen attached to stertor, choking, relaxation of the sphincters, and other morbid affections of the true spinal functions, in cases of cerebral disease.

On the other hand, if the excitor-motory system be impaired in its functions, the acts of the cerebrum are interrupted. The volition is perfect in chorea, in stammering; but the voluntary movements, from the morbid condition of the excito-motory system, are irregular and imperfect. I have this day witnessed a remarkable fact: a patient who had suffered a degree of loss of power of the left arm and leg, from a protracted epileptic seizure, and who could not close the hand firmly otherwise, could grasp any object placed in it with considerable force.

I must briefly mention one fact more. The whole *tone* of the muscular system is the result of an excito-motory function. The limbs of an animal, or of a part of an animal separated from the influence of the cerebrum, become relaxed on destroying the spinal marrow. If we remove the tail and the rectum from a recently decapitated turtle, the sphincter retains its circular form, the tail its firmness, phenomena which cease entirely on withdrawing the portion of spinal marrow remaining within the caudal spinal canal*.

The third subdivision of the nervous system embraces *all* the ganglionic nerves. I have divided it into the *internal*, which comprises the sympathetic and perhaps a part of the pneumo-gastric; and the *external*, which embraces all the other ganglionic nerves, the *fifth* especially, and the posterior spinal.

Lastly, different *remedies* act upon distinct parts of the nervous system: opium acts on the cerebral; strychnine and hydrocyanic acid on the true spinal subdivisions of that system.

The only novelty which attaches itself to this part of my division of the nervous system, is that which relates to the external ganglionic, or external nutrient, nerves.

I am not aware that any preceding inquirer has suggested the real office of the ganglia on the fifth and posterior spinal nerves. These ganglia were first observed to be attached to the portio major of the fifth and the posterior spinal nerves, not, as Sir Charles Bell states*, by Monro, but

* Recherches Expérimentales sur le Système Nerveux. p. 180, &c.
† The Nervous System, 1830, p. 46.

* See a paper by the author in the Transactions of the Royal Society for 1833.
† The Nervous System, 1830, Pref. p. vii. &c.

by Prochaska. This latter author observes, in the preface to a republication of his work in 1800:—

"Hic tractatus, qui anno 1779 prodivit, plures novas observationes circa structuram systematis nervosi a me factas continet. Harum nonnullæ, quibus in textu non fuit locus, in explicatione figurarum uberius exponuntur, ad quas spectat nova arbor vitæ corporum olivarium, vid. Tab. I. fig. 3, 4, 5; fasciculus funiculorum nerveorum quinti paris cerebri, qui insalutato ganglio semilunari sub eodem tertium ramum ejus nervi maxillarem inferiorem dictum petit, vid. Tab. II. fig. 4, 5; sic quoque radices anteriores omnium nervorum spinalium, quæ insalutata ganglia radicum posteriorum transeunt, vid. Tab. III. fig. 1, 2. Super eandem materiem plura præclara scripta isto tractatu serius in lucem prodivere, inter quæ præcipue *Monroi Observations on the Structure and Functions of the Nervous System* (1783); *Soemeringi Ueber das Organ der Seele, Königsberg*, 1796; et *Reilii Exercitationes Anatomicæ de Structura Nervorum*, 1797, adnotari merentur."

Prochaska adds, p. 353, "Quis rationem dabit:"..."Quare radices anteriores nervorum spinalium ganglia spinalia insalutata transeant, et quare nam solæ posteriores radices ganglia spinalia trannare cogantur."..."Quare omnium cerebri nervorum solum quintum par post ortum suum more nervorum spinalium ganglion semilunare dictum facere debet, sub quo peculiaris funiculorum fasciculus ad tertium quinti paris ramum maxillarem inferiorem dictum, properat insalutato ganglio semilunari ad similitudinem radicum anteriorum nervorum spinalium?"

Sœmmering has asked similar questions. Sir Charles Bell has quoted Prochaska, and Sœmmering, and Scarpa, in a tone of exultation; but I confess that to me it appears that that justly celebrated physiologist has not approached any nearer to the resolution of these questions than his predecessors. Sir Charles Bell has, I believe, distinctly proved the difference of function between the anterior and posterior spinal nerves—a brilliant discovery, which will, as long as anatomical and physiological science last, perpetuate the memory of his genius; but there is no connexion between the function of sensation and the existence of a ganglion; and the unequivocal sentient nerves, as the olfactory, the optic, the auditory, are without any thing very distinct of this kind.

The questions, then, still remain, why is the portio major of the fifth, especially, and the posterior spinal, nerves provided with ganglia? The reply to these questions, and the argument, may be stated thus:—

1. There is an internal nerve for formation, nutrition, secretion, &c. 2. This nerve is ganglionic. 3. There are external organs and structures requiring nutrition, &c. 4. There are also external ganglionic nerves. The inference is plain, that these constitute the external ganglionic sub system. The fifth especially abounds with ganglia.

It is true that the semilunar and external spinal ganglia differ in appearance from the ganglia of the sympathetic, as Sir Charles Bell has well displayed. What is the nature of this difference? To this question I find no reply in authors. It is plain, however, that the difference consists in their being, alone, *plexic*. The internal ganglionic nerve is purely nutrient; its ganglia are simple. The external involve sentient, and I believe excitory, nerves, with the nutrient; they combine, therefore, the appearances of the plexus and of the ganglion.

I must add another argument upon this point. If the sensation of the face be lost by paralysis, arising from disease of the *brain*, the eye is safe; but if the same event occur from compression or destruction of the *nerve within* the cranium, by disease, or in an experiment, the eye ceases to be nourished, and becomes destroyed! In the former case the nerve of sensation merely has suffered; in the latter the nerve of nutrition, as well as of sensation, has been involved in the disease or injury.

III.—*The Pathology of the Nervous System.*

I now proceed to that part of the subject which it is more peculiarly my province to treat in this place—the pathology of the nervous system.

In order to conceive a clear idea of the pathology, we have only to imagine the physiological phenomena already noticed assuming a pathological character. Now the force of these phenomena may be augmented, or diminished, or annihilated.

In regard to the cerebral functions, we have, in the sentient nerves, pain or insensibility; in the cerebrum itself erroneous perceptions and volitions, or delirium, or a total deficiency of these faculties, or coma; in the motor nerves continual voluntary actions, or paralysis.

We may take the face with its sentient and motor nerves to illustrate a part of this subject. We may have morbid sensibility in the face; and this may assume the form of *tic douloureux*. We may, on the other hand, have loss of sensibility: this may arise from disease of the opposite hemisphere, or of the fifth nerve, within, or without, the cranium. The

former case constitutes hemiplegia of the face; the latter cases have been particularly described by Sig. Bellingeri* and Sir Charles Bell†. We have in these affections interesting calls upon our resources for the diagnosis.

In hemiplegia the loss of sensation is rarely complete, and there is usually paralysis of the muscles of the face, and the susceptibility of the nostrils to irritants is unimpaired; this was the case in a patient whom I recently examined, by the kindness of Dr. Watson, in the Middlesex Hospital.

In the case of disease of the fifth within the cranium, the loss of sensibility is frequently complete, the nostril has also lost its susceptibity to the impression of stimuli, and eventually the eye, not being nourished, shrinks and collapses; the power of the masticatory muscles is impaired, but the face is not distorted by any *apparent* paralysis.

If the seventh or facial nerve be affected, we may have *spasmodic tic*, as represented in this portrait, or paralysis, as represented in this. The patients are drawn attempting to close the eyes. In the case of spasm, it is the eyelid of the *contracted* side of the face which does not close. In the case of paralysis, it is the eye of the *relaxed* side which is reposed: in *both* it is the *affected* side.

Both these affections may have their origin *within* or *without* the cranium. I am at this moment attending a case of spasmodic tic, arising from a cause within the cranium, with Mr. Tyrrell. With the tic there is defective vision of the eye of the side affected. Spasmodic tic arising from causes external to the cranial cavity are not uncommon: this is a slight sketch of such a case, which I had taken from a patient at Bridgewater in 1817.

I will close these hurried remarks by an observation of some interest. In paralysis of the face, from disease of the opposite hemisphere, the eye-lids can be closed, as in this representation;—in paralysis of the facial nerve, the orbicularis is paralysed, as you have already seen in this. What is the rationale of this difference? The seventh, like the fifth, is a compound nerve. As the latter embraces excitor and ganglionic filaments, which are not involved in the attack of hemiplegia, so the former comprises a branch belonging to the excito motory system, which is not affected in disease of the cerebrum. At least such is my view of this subject.

I must not, however, extend my observations on the cerebral system, but hasten to that in which particularly to engage your interest. That I shall readily do this, on account of the part which I have taken in its elucidation, I have no doubt; but I wish particularly to do, on account of the intrinsic and particular value of the subject.

Both the fifth and seventh pairs of nerves are more complex than they are represented to be by Sir Charles Bell. The former includes excitor and nutrient nerves, with the nerve of sensation; and it has appropriate origins, distributions, and offices: of its offices, sensation alone is impaired by cerebral disease; but all are annihilated by the pressure of a tumor within the cranium. The seventh comprises pure cerebral and true spinal nerves: the cerebral only is affected in hemiplegia, and the orbicularis retains its power; all are paralyzed by the pressure of a tumor below the ear, and we have paralysis of the sphincter of the eye-lid. This remark leads me to observe that *ptosis* is a cerebral paralysis, whilst *logophthalmia* is one of the true spinal system: to the latter system *strabismus* also frequently belongs.

The first remark I would make is a very broad one. I believe that the *whole* order of spasmodic and convulsive diseases belongs to this, the excito-motory division of the nervous system,—and that they cannot be understood without a previous accurate knowledge of this system!

A second remark is equally important. All these diseases have their source in *one* of *two* parts of the excito-motory system: the *first* series have their origin in the spinal marrow itself, the axis or centre of the system; I shall designate these cases by the epithet *centric:*—the *second* series have their source in the excitor nerves, consequently at a distance from that centre; I shall denominate them the *eccentric*. I will soon convince you that this distinction is not an unimportant one: the prognosis depends upon it almost entirely; the centric diseases are, for the most part, incurable; the eccentric diseases, on the contrary, as generally, with some particular exceptions, admit of cure. I will briefly illustrate these positions:—You have two little patients with croup-like or other convulsion; one of these cases may arise from disease within the cranium or spinal canal; it will most probably prove incurable: the other may arise from dentition, a cause acting upon an excited branch of the fifth; I need scarcely add that it will generally yield to the prompt and energetic use of the appropriate remedies.

A third remark is, that in all, or almost all, the order of spasmodic diseases, the

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* Dissertatio Inauguralis, 1818.
† The Nervous System.

parts most immediately concerned in ingestion and egestion,—the orifices and exits of the frame, are those principally affected. The physiology has become pathology. The *larynx* is *closed* in the convulsions of children, in epilepsy, in puerperal convulsion; it is spasmodically affected in tetanus and hydrophobia; it is partially affected in the croup-like convulsion, in hysteria, in which there is frequently loss of voice, &c. The *pharynx* is affected in some of these diseases. The respiratory muscles are so in all. In epilepsy we observe affections of the sphincters, and even of the ejaculators.

No disease can illustrate the pathology of the excito-motory system better than epilepsy. It is sometimes centric, and incurable; frequently eccentric, arising from gastric or intestinal irritations, and curable. It involves every part, and every function, of which I have spoken under the head of the physiology. The fourth and sixth nerves are affected, and the eyes move convulsively; the tongue is protruded, the teeth are forcibly closed upon it, the mouth is variously moved with the extrusion of bloody foam; the larynx is closed, and there are forcible convulsive efforts of the expiratory muscles; and as I have just stated, the sphincters are sometimes relaxed, and the ejaculators occasionally expel the semen.

I must here draw a brief parallel between epilepsy and the act of coitus. Galen designated this act as a slight epilepsy, observing—"Hippocratis verba hæc traduntur;" την συνουσιαν ειναι μικραν επιληψιαν; it has also terminated in actual epilepsy, or, like epilepsy, in cerebral hæmorrhage.

The condition of the larynx and of the respiratory motions affords an important diagnosis between epilepsy and hysteria. In the former the larynx is usually closed with forcible expiratory efforts; in the latter it is open, with heaving, sighing, breathing.

In one case of epilepsy, my patient, who was musical, lost the power of singing the higher notes after each attack. It is well known how frequent loss of voice is in hysteria. In hysteria we have rarely, if ever, a bitten tongue.

A terrible disease of this order is tetanus. All the symptoms of tetanus sometimes arise from disease within the spine. This ought to be termed *centric* tetanus. Far more frequently the cause is seated externally,—in the course of some of the excitor nerves of the system. A nerve included in a ligature, or lacerated in a wound, is most frequently the *eccentric* seat of tetanus. In both cases it is as plain that it is the excito-motory division of the nervous system which is involved in the disease, as is the sun at meridian day.

To show you how little this subject has been understood, I will adduce one fact especially. Even Mr. Swan, than whom no one has dissected the nervous system with more success, imagines that tetanus may have its seat in the ganglionic or sympathetic system of nerves*.

Mr. Swan observes—"I have been induced to inquire how the body is usually affected after accidents. From that inquiry I have been led to state, that when a severe injury has been received, the ganglia of the sympathetic nerves become irritated, and consequently the parts to which they distribute nerves." The irritation "may be communicated to many of the cerebral and all the spinal nerves, and from these to the spinal chord; thus producing tetanic spasms, spasms varying according to the part of the sympathetic nerve most affected, as well as the extent and complexity of the irritation."

It is difficult to conceive how the sympathetic could either be affected by the cause, or produce the symptoms, of tetanus; its functions are interstitial, not obvious to our senses; seen only in their *effects*. It is plain, on the contrary, that the real seat of this disease is that portion of the nervous system which I have distinguished from the rest, and designated the excito-motory. Tetanus may be produced at will in the frog or salamander, by applying strychnine to the skin. If the head be moved, the frame is still tetanic. If any portion of the spine, even the tail, of the salamander be separated, it exhibits all the phenomena of perfect tetanus! These cease on destroying the caudal portion of the spinal marrow, by means of a fine needle. If in the decapitated turtle you lay bare certain nerves, and pinch them continuously with the forceps, you immediately induce a state of tonic contraction of the muscles of all the four extremities, and of the tail. This experiment is the very *type* of tetanus, and leaves *no* doubt what particular part of the nervous system is affected in this disease.

I have not time to say a word about hydrophobia. But consider how this disease is induced; what symptoms present themselves; what parts, what functions, are involved; and you cannot fail to fix upon the particular division of the nervous system affected in this most terrible of maladies.

But I must hasten to conclude. Allow me to say one word respecting vomiting. This act may be excited by disease within

* A Treatise on Diseases and Inquiries of the Nerves, 1834; p. 325, &c.

the cranium, by irritation of the fifth (?) in the fauces,—of the pneumo-gastric in the stomach, the gall-duct, the ureter,—of spinal nerves at the cervix uteri. This familiar phenomenon combines these excitor nerves, and the motor nerves of respiration, into one system.

On the other hand, dentition produced strangury and tenesmus,—symptoms of calculus, in the little boy of a friend of mine, symptoms which ceased on freely lancing the gums.

In one case, extreme spasmodic stricture of the sphincter ani was produced by the unsuspected presence of a calculus in the urethra. There is no more common event than retention of urine from passing a ligature round a hæmorrhoidal tumor. In *all* these various cases an excitor nerve is irritated; the irritation is carried to the medulla oblongata or spinalis, and reflected upon the muscle, or system of muscles, excited to spasmodic action.

The time does not now admit of my adducing more facts of this kind. Before I conclude, however, I must lay before you some facts of another description.

First, disease of the meninges and of the brain induce spasmodic actions. How is this to be explained? I think, upon the principle of *counter-pressure*. In an interesting case most anxiously watched, and accurately detailed to me, by my friend Mr. Toogood, of Bridgewater, of a little girl, aged thirteen months, the croup-like convulsion occurred repeatedly, until one day, when the bones of the cranium separated, the convulsion then ceased. In a case of spina-bifida related to me by Mr. Herbert Evans, of Hampstead, there was a croup-like convulsion whenever the little patient turned, and so pressed upon the tumor. In the case of anencephalous fœtus described by Mr. Lawrence, convulsion was produced on pressing on the medulla oblongata. These and other facts lead me to think that convulsions, arising from cerebral disease, is thus to be explained. Convulsion, in its turn, produces cerebral congestion, and probably effusion.

What is the rationale of convulsion from excessive hæmorrhagy? It struck me that this question might be resolved by experiment. I went to the house of a butcher, and begged to see a sheep killed. The usual mode of doing this is, first to divide the spinal marrow, and then to open the large vessels. At my request, not only the spinal marrow, but the entire neck, was divided, the head being separated from the body with the exception of the skin: the blood-vessels were then divided. I watched the effect of the flow of blood. After a certain hæmorrhagy had taken place, the animal was violently convulsed. This convulsion could only be spinal.

One final word upon the pathology of the external portion of the ganglionic system. I think it probable that many of those cases in which the limb of an infant ceases to grow, are cases in which the disease is seated in the posterior spinal nerves leading to the part—probably at or near their origin. This conjecture must be confirmed by the careful post-mortem examination of such cases. It may possibly admit of being illustrated by experiment.

You will be assisted in your study of the diseases of the nervous system by the Syllabus of these diseases which I have had printed for you. The anatomy, the physiology, the pathology, the individual diseases, the infantile diseases of this system, are pretty fully arranged in it. I shall in future lectures treat of these subjects in their turn, and, as usual, in a perfectly *practical style*, from which the present lecture must be viewed, in some degree, as a deviation.

I *must* now conclude. If I have awakened your attention and interest in reference to this important pathological and practical subject, I have done all that I could expect from a single lecture. I have one request to make of you: receive what I have said in a love of the truth; seek not to dispute, but to prove—to confirm or correct my statements. It is some years since I began my inquiries; much, very much, remains to be done. I trust I shall be aided by *you* in the further prosecution of the subject. Be careful how you observe; remember my words on a former occasion—*fulness*, *accuracy*, *probity*, should be the legible characters written upon every *case*, and the mental and moral characters of every *observer*.

DEATH OF AN INFANT FROM INHALATION OF HOT TURF ASHES.

To the Editor of the Medical Gazette.

SIR,

THE nature of the injury, in the subjoined case, I believe to be very rare. I have no recollection of having met with one like it in the course of my reading, and it is new to those of my professional brethren to whom I have made known the particulars. As such, I transmit it to you for publication, thinking it may prove interesting to your numerous readers. The drawings, sent herewith, which most accurately

illustrate the condition of the parts affected, were done by our assistant, Mr. F. G. Delamotte, of whose skill as an artist honourable mention has been made, both by you and myself, in a former volume of the Gazette.

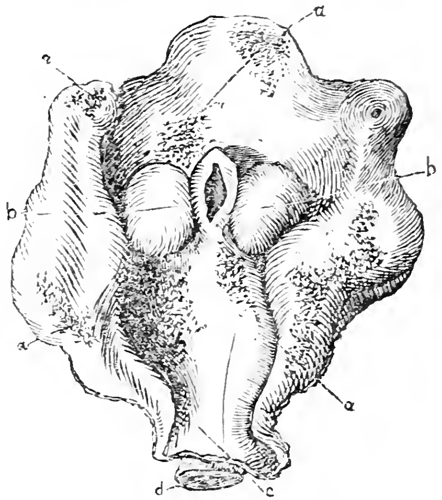

FIG. 1.—*Interior of Pharynx.*

a, a, a, a, Particles of ashes.
b, b, Vesications on each side of rima glottidis.
c, Œsophagus.
d, Trachea.

On the 12th of this month, as I was riding to the house of a gentleman whom I was attending at Seale, I was informed that a child, belonging to the parish, had been severely burnt at Puttenham, about five miles from here; and that a messenger had been sent to request me to come immediately. I reached the house where the child was, at a few minutes after one o'clock, found it had been dead nearly an hour, and that the distressing circumstances attending its death were as follows:—

About eleven of the same morning, the mother had left the child alone, in the only room down stairs which was inhabited, during not more than three minutes, for the purpose of washing some potatoes for her pig, which she did in a back apartment adjoining the room. The child was a very fine boy, 16 months old, unable to walk; but with the assistance of chairs, or any of the furniture in the chamber, to hold by, he could move himself from one place to another. Unfortunately, he was near the fire-place when the mother quitted him; and on her re-entering the room, she saw her poor child lying motionless, with his face on the hearth, which contained a vast quantity of ashes.

The cottagers in this neighbourhood, where heath so much abounds, chiefly use, for firing, the turf-sods collected from the commons or heaths, the ashes of which are very fine, and retain the fire in them longer than those of either coal or wood. Their fire-places are large, occupying the whole space of the bottoms of their huge chimnies, and raised a few inches only above the level of the floor. Thus, owing to the large consumption of this turf, which burns rapidly, an immense amount of ashes often accumulates on the hearth, and in the present instance it is estimated there were not less than two bushels. The child did not, however, fall into the midst of this, but his face was discovered on a part so thin, that the impression thereof was barely distinguishable.

The infant was instantly taken up by the mother, who called one of her neighbours to her aid. He could not cry—indeed, was scarcely able to breathe; and appeared to them to have something in his throat to prevent his breathing more audibly, or his crying. A mixture of oil and milk was poured into his mouth after they had removed the particles of ashes adhering to it, and to the nostrils, but it plainly could not be

swallowed. He lay perfectly still for probably an hour, and respiration was so feeble and slight, that it was perceptible only upon holding a looking-glass before the face. A little after twelve life ceased, without a struggle or the least movement of the body.

The only *external* marks of injury were, a minute vesication on the tip of the nose, and two others on the upper lip.

On the following day I proceeded to examine the body. In the back part of the mouth some particles of ashes were apparent upon separating the jaws, but no signs of lesion of the mucous membrane. My first object was to ascertain the state of the larynx and lower portion of the pharynx, which I accordingly removed from their natural situation by dividing the muscular attachments of the latter from below upwards. In these parts the whole mischief done was most obvious. The lining membrane of the inferior portion of the pharynx was highly inflamed, and covered here and there with ashes; it was also somewhat infiltrated. (*Vide* drawing.) That of the epiglottis might be truly said to be in a state of vesication and infiltration, both on the tracheal and palatine surfaces, and that of the rima glottidis likewise. This latter passage was rendered quite impervious even to air; and from the distended and swollen state of the mucous membrane, its sides approximated in the centre, and resembled two cherries. Both surfaces of the epiglottis were of a deep red colour, and contained ashy matter, which could be traced a little distance down the œsophagus, and on a portion of the cricoid cartilage internally.

No doubt could exist but that the foregoing state of parts was produced by the direct application of the *hot* ashes, which were inhaled by the poor infant in the effort of crying; and it is very singular that these should be so instantly conveyed to the vital parts thus mortally injured, and that none other adjacent structures should exhibit equally severe traces of damage. (*Vide* drawings.)

The trachea and bronchia were filled with a reddish-coloured frothy mucus; and both lungs afforded the appearances of great congestion.

From the above account of the state of the larynx will be explained the symptoms manifested during the very short period the child survived the accident. The chink of the glottis could not have been *perfectly* closed immediately; but still the aperture was insufficient to allow of more than the most slender respiratory process.

Had I been at the first moment upon the spot, the performance of tracheotomy might have prolonged the little sufferer's existence for some hours. I am not sanguine enough to have expected more from it; at least the results of the operation in the cases that may be compared to the foregoing one—those, I mean, caused by receiving boiling water from the spouts of tea-kettles—would lead me to this conclusion.

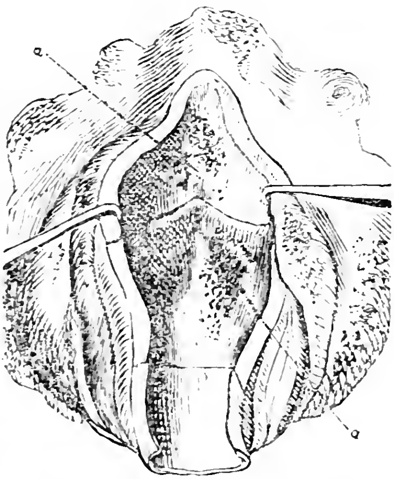

FIG. 2.—*Internal Surface of Larynx exposed.*

a, *a*, Ashes.

Perhaps a certain *honourable* contemporary editor "will not be satisfied" with my conduct relating to the above case, should he now ever read any thing so respectable as your journal. I beg to inform him, therefore, that a coroner's inquest has been held upon the child; and as he has taken such a lively interest in one lately carried on here, wherein I was the medical witness, and in relation to which I have been honoured with his especial insult, and exemplification of literary *candour* (for I do consider it an honour to be insulted and abused by such a character), that he means to move, "in his place" in the house, "on the re-assembling of Parliament," for a *verbatim* transcript of my deposition of the verdict of the jury. I would take the liberty of re-

commending to him, in order to save time, trouble, and expense to the nation, to make a motion at the same time for the like copies connected with the present inquest. No doubt some "innocent" person would here also be found implicated by me, if he would but "manfully" come forward on the occasion with just *twenty-two* misstatements at his back, *Anglicè* falsehoods, half of which must of course be fathered upon the inventive faculties of the *unfortunate* printers (I mean no allusion to the recent *bankruptcy*).

I remain, sir,
Your obedient servant,
G. Bury.

Farnham, Dec. 17, 1835.

ON THE

AVERAGE DURATION OF LIFE,

AND THE

VIE PROBABLE, IN THE SEVERAL COUNTIES OF ENGLAND.

To the Editor of the Medical Gazette.

Sir,

Perhaps you may have remarked that in my last letter I did not attribute an annual rate of mortality to any of the counties of England, thereby avoiding the customary but unsatisfactory manner of so doing without reference to the increase of population,—a fallacy which could not have escaped the notice of the most incurious observer, had he adverted to the well-known fact, that human life is most precarious at its two extremes—in infancy and in old age; so that a rapidly-increasing population (which infers the existence of many infants and their deaths in like proportion), produces a preponderance of mortality in infancy; a slowly-increasing or stationary population (which infers a large proportion of aged persons), produces a similar preponderance in old age; and I still fear that until the effect of these counteracting influences shall have been generally recognized and understood, the facility of dividing an existing population by the annual number of deaths, will continue to produce formal statements and inferences of much less value than has hitherto been ascribed to them.

Another method of estimating the comparative health of nations, was afterwards introduced by those who bestowed on it the title of *vie probable*,—a commodious phrase for expressing the number of years at which one half of the population are dead, the other half survives. But the incompetency of this *vie probable* for its purpose was not concealed by so powerful a counteracting influence, the large proportion of births, and consequently of infant deaths, in an increasing population, producing an overwhelming result, which puts to flight the *vie probable* as an absolute measure of health and longevity.

A third and better method for approximating to the same kind of knowledge, has been devised and explained by those who have given it the title of *vie moyenne*, which is found by collecting the number of years during which all the deceased have lived, and dividing the total by the number of the deceased. Thus is ascertained the average duration of life, which would be equivalent to what is called in England "the expectation of life at birth," were it not that the *vie moyenne* is influenced by the increase of population, while "the expectation of life," being ascertained by pursuing the same individuals through life, is not liable to variation otherwise than from an actual variation in the duration of human life, which has been much prolonged since the ages of the deceased were first recorded.

According to the unquestionable theorem of Laplace, "Le nombre d'années qui exprime la durée moyenne de la vie est le vrai rapport de la population aux naissances. La détermination de ce facteur est le point le plus délicat et le plus intéressant des statistiques vitales." In other words, the *vie moyenne* indicates the annual proportion of births (and therefore of deaths), supposing the population to be stationary, or rather to have been retrospectively stationary, through the life of man: a condition which probably has never been fulfilled—certainly has never been known to have occurred; which amounts to the same obstacle in practice. And it is to be understood that an increasing population will always exhibit a *vie moyenne* shorter than the true expectation of life, a decreasing population having an opposite tendency. Thus in England it has been sufficiently ascertained that the expectation of life at birth is at present (combining the sexes) about forty years; whereas the highest amount of the *vie moyenne*, calculated upon the

deaths of 18 years (1813-1830), little exceeds 40 years in any county, and in the other extreme is no more than 25 years.

Knowing by experience how difficult it is to explain in words the effect of increasing population on the *vie probable* and the *vie moyenne*, I shall take the liberty of exhibiting a *scale of coincidence*, founded on the fact, that an increase of 57 per cent. in 30 years (1801—1831), has produced in England a *vie probable* of 25·4 years, while a less increase of about 20 per cent. in some of the counties has left to them a *vie probable* of 40 years; which agrees well enough with a difference of ·4 (four-tenths of a year) for 1 per cent. increase of population.

The *vie moyenne* of England I have calculated at 33 years, which in like manner becomes 40 years in some of the counties, agreeing with ·2 (two-tenths of a year) for 1 per cent. increase of population; which in both cases is carried on to 99 per cent., up to which, from 19 per cent. increase, the annexed scale is necessary for including the several counties. It is compressed or abbreviated by omitting the numbers which do not apply to the increase of population in any of them.

SCALE OF COINCIDENCE

Of the Vie probable, *and of the* Vie moyenne, *with the Increase of Population.*

Per Centage Increase.	Vie Probable.	Vie Moyenne.	Per Centage Increase.	Vie Probable.	Vie Moyenne.	Per Centage Increase.	Vie Probable.	Vie Moyenne.
0	49·2	44·4	37	33·4	37	59	24·6	32·6
1	47·8	44·2	38	33	36·8	60	24·2	32·4
2	47·4	44	40	32·2	36·4	61	23·8	32·2
7	45·4	43	41	31·8	36·2	62	23·4	32
12	43·4	42	42	31·1	36	64	22·6	31·6
17	41·4	41	43	31	35·8	67	21·4	31
19	40·6	40·6	44	30·6	35·6	72	19·4	30
21	39·8	40·2	45	30·2	35·4	73	19	29·8
22	39·4	40	47	29·4	35	74	18·6	29·6
25	38·2	39·4	48	29	34·8	77	17·4	29
27	37·4	39	50	28·2	34·4	81	15·8	28·2
29	36·6	38·6	52	27·4	34	82	15·4	28
32	35·4	38	55	26·2	33·4	87	13·4	27
33	35	37·8	56	25·8	33·2	92	11·4	26
36	33·8	37·2	57	25·4	33	97	9·4	25
						99	8·6	24·4

It is proper to guard your readers against expecting perfect accuracy in the application of this scale; evidently the *plus* and *minus* of longevity in different situations is a disturbing element; and more powerful than this (as being generally applicable) is another consideration—that the collected deaths extending from 1813 to 1830, apply in strictness to a middle point, to the enumeration of May or June 1821; and it is only from the equable march of population since that time and previously to it (about 15 or 16 per cent. in each of the three decennial periods), that we are justified in applying the foregoing scale to the increase of the entire 30 years *. One-half of the inhabitants of England are not above 20 years of age—two-thirds are not above 30 years of age; so that one-third of the existing population (all above 30 years old) were born previously to the enumeration of 1801, when the increase was proceeding at a lower ratio by one-half. Of this the scale can take no cognizance; and in the counties which have increased equally in the entire thirty years, the increase has not always occurred in regular gradation. Thus, in the counties of Berks and Salop, which have equally increased 33 per cent. in population between 1801 to 1831, the three decennial periods shew an increase as 8, 12, 10, in the former—as 16, 6, 8, in the latter; which cannot but have produced discrepancy in the ages of those who die. Expecting fair allowance for all these recited obstacles to accuracy of result, I have ventured to construct the following table:—

* The increase of females (far the best criterion) has been as 14·15—15·71—15·46—in the three decennial periods.

TABLE *exhibiting* (1.) *the Increase of Population* (1801—1831) *in England and Wales, and in the several English Counties;* (2.) *the* Vie probable, *or Age at which One Half of the Population were dead* (1813—1830); (3.) *the* Vie moyenne, *or Average Duration of Life* (1813—1830), *with the Difference in both Cases from the foregoing* Scale of Coincidence; (4.) *lastly, the Annual Mortality* (1816—1820), *one-tenth being here added, in compensation for the usual Rate of omitted Entry in Parish Registers.*

COUNTIES OF ENGLAND.	(1.) Per Centage Increase of Population 1801–1831.	(2.) *Vie Probable.* One Half dead at (Years).	Difference from Scale.	(3.) *Vie Moyenne.* Average Life. (Years)	Difference from Scale.	(4.) Annual Mortality 1816–1820. One in
Rutland	19	38·4	— 2·2	39·1	— 1·5	56
North York	21	38·2	— 1·6	40·3	+ ·1	55
Hereford	25	41·7	+ 3·5	41·3	+ 1·9	54
Wilts	29	35·7	— ·9	38·1	— ·5	57
Westmorland	32	35·9	+ ·5	39·2	+ 1·2	47
Berks	33	32·5	— 2·5	36·8	— 1 0	49
Salop	33	31·3	— 3·7	36 5	— 1·3	49
Northampton	36	30·5	— 3·3	35·9	— 1·3	50
Bucks	36	30·0	— 3·8	35·8	— 1·4	48
Dorset	38	35·3	+ 2·3	38·3	+ 1·5	57
Oxford	38	30·7	— 2·3	36·0	— ·8	51
Essex	40	27·4	— 4·8	33·9	— 2·5	52
Suffolk	41	31·9	+ ·1	36 9	+ ·7	59
Huntingdon	41	26·7	— 5·1	33·7	— 2·5	55
Northumberland	42	32·4	+ 1·0	37·3	+ 1·3	51
Norfolk	43	25·8	— 5·2	34·3	— 1·5	53
Devon	44	31·4	+ ·8	36·2	+ ·6	53
Hants	44	29·6	— 1·0	35·0	— ·6	55
Cumberland	45	28·4	— 1·8	35·3	— ·1	49
Hertford	47	28·1	— 1·3	34·6	— ·4	48
Derby	47	25·8	— 3·6	33·8	— 1·2	53
Somerset	48	30·5	+ 1·5	36·0	+ 1·2	55
Bedford	50	26·6	— 1·6	34·3	— ·1	51
Leicester	52	25·6	— 1·8	33·3	— ·7	50
Lincoln	52	25·6	— 1·8	33·3	— ·7	53
Worcester	52	23·7	— 3·7	32·5	— 1·5	48
East York	52	24·2	— 3·2	32·7	— 1·3	49
Gloucester	55	30·4	+ 4·2	35·3	+ 1·9	54
Kent	56	24·2	— 1·6	31·5	— 1·7	45
England (and Wales)	57	25·4	Nil	33·0	Nil.	52
Durham	59	26·4	+ 1·8	34·6	+ 2·0	48
Cornwall	60	33·7	+ 9·5	37·5	+ 5·1	62
Cambridge	61	20·8	— 3·0	30·4	— 1·8	50
Nottingham	61	19·7	— 4·1	30·1	— 2·1	49
Warwick	62	21·0	— 2·4	30·1	— 1·9	43
Middlesex	67	20·6	+ 5·2	30·7	— ·3	41
Sussex	71	26·7	+ 6·9	34·1	+ 3·9	61
Stafford	72	18·5	— ·9	29·2	— ·8	46
West York	73	18·3	— ·7	29·3	— ·5	51
Chester	74	20·6	+ 2·0	30·4	+ ·8	47
Surrey	81	24·0	+ 8·2	30·7	+ 2·9	44
Lancaster	99	11·6	+ 3·0	25·6	+ 1·0	46
Isle of Ely	64	13·8	— 8·8	27·9	— 3·7	43

It will be perceived that I have added the Isle of Ely at the bottom of the foregoing table; this district is distinguished from the rest of the county of Cambridge by a peculiar jurisdiction, and very unfavourably as regards health and longevity. It is highly fertile, but flat, alluvial, and marshy; and containing a population of 46,000 inhabitants, furnishes a fair specimen of the effect of marsh land, and will partly explain the foregoing table. To your medical readers it may be superfluous to remark, that every surface retentive of water differs only in degree from fenny, or marsh land, in its deleterious effect on human health; so that to the under-ground drainage of arable land may be attributed much of the increased longevity which has occurred in England, beginning with the year 1785. This wholesome process was vastly increased from 1795 to 1815, in consequence of the high price of corn, till the end of the war. The Western half of the County of Suffolk (commonly called *High Suffolk*) is a prominent instance of the benefit of Under-ground drainage, as conducive to health, as well as insuring fertility.

It will be understood that the entire of England cannot but represent the average longevity of all its counties, and this is denoted in the table by the word *nil*, signifying *no variation* from such average; which is, in fact, the foundation of the whole calculation of *plus* and *minus*. Next above ENGLAND (in column 3) stands Kent; which county having increased 56 per cent., ought, according to the scale of coincidence, to give *vie moyenne* 33·2 years, while, in fact, it gives no more than 31·5, less by 1·7 than the average longevity of England, if the scale be correct; and Kent being a county bordered on its northern limit by much marsh land (especially at the mouth of the river Medway), and containing Romney marsh on its south coast, the longevity of the entire county is thus depressed below average. The same defect, from the same cause and in a greater degree, affects the county of Essex; and all the counties bordering the Isle of Ely manifest some depression. Next below ENGLAND (in column 3) stands Durham; which county having increased 59 per cent., ought to give *vie moyenne* 32·6 years, but, in fact, gives 34·6; so that it may be deemed healthy above average, to an extent measured by the difference of these numbers.

The excessive cases are Ely, already described as remarkably unhealthy, and in the other extreme Cornwall, a county well defined on the north and south by the sea, and in character opposed to Ely, as consisting of a middle ridge of high land, sloping on both sides with uneven surface, to the north and south coasts; which are no where flat, much less marshy. A great quantity of rain falls in this county, nearly twice as much as in London *, but the soil is not retentive, and water never stagnates on its surface. With these favourable characteristics, Cornwall shows a longevity of five years above the average deducible from its per centage increase, in like manner as Ely declines three and a half years below such average.

The seeming longevity of Sussex is liable to subtraction, because its population has been much increased by the immigration of adults, who have settled at Brighton, Hastings, and other places of fashionable resort on the coast. Surrey has experienced a similar immigration, in a greater degree, from its containing the south suburb of the metropolis, where the parishes of Lambeth, Newington, and Camberwell, have enormously increased in population. I need not repeat that infant mortality reduces the *vie moyenne*, nor say that *therefore* immigration has an opposite tendency.

The whole table contains forty-two counties, or parts of counties. In nine of these the difference of the actual *vie moyenne* from the scale of coincidence, does not exceed six months; in eight others, from that to twelve months; in twelve, from that to eighteen months; and in six, from that to two years: the rest have been already specially noticed. These are, indeed, considerable variation as is the difference of longevity in various situations; and for due appreciation of the scale of coincidence, they are to be contrasted with the negation of any such scale; in which case the *vie moyenne* of West York, at twenty-nine years, must be compared with that of North York at forty years; these being parts of the same county, and in contact along a border of seventy miles in extent. So the results of Rutland, North York, and Hereford, which average at 40 years, are, on that supposition, to be deemed compatible with the

* At Penzance, 44·7 inches; in London, 25 inches. See "The Medical Topography of Cornwall," for this and other particulars, by Dr. Forbes.

vie moyenne of Lancashire at $25\frac{1}{2}$ years; which would infer a different race of beings, rather than different counties in the same England; and, in favour of the scale, it must not be forgotten that it is supported by no larger basis than the known increase of thirty years. In the year 1841, it may be constructed with the advantage of ten years' additional basis; when three-fourths, instead of two-thirds, of the population will have been born after the enumeration of 1801.

I shall now advert to the *vie probable* (which occupies column 2 of the table, and enhances the difficulty of withholding assent to it.) Herein the average *vie probable* of England is calculated at 25·4 years, and, being marked by the word *nil*, is the origin of a range of variation from it which extends twice as far as that of the *vie moyenne*—to thirty-two years instead of sixteen years. Consequently the difference from scale is usually double in the several counties: in some it is widely different from this, especially in Middlesex (which is much the same as the metropolis); a fact which I cannot pretend to explain, the relative amount of difference between the *vie moyenne* and the *vie probable* never having been (to my knowledge) investigated; and I can furnish nothing more than the facts which appear in the table. Not only is the effect of infant mortality very manifest in calculating the *vie probable*, but the greater number and great mortality of male infants create a very perceptible sexual variation of result. "This cause is so powerful, that in Lancashire, where one-half of all individuals born do not attain the age of twelve years, one-half of the males are dead at seven years, one half of the females at sixteen or seventeen years of age."—(Population Preface, p. xlvi.)

The last column (4) of the table, exhibits the annual rate of mortality in the several counties; and perhaps it may be remembered that in my last letter this was omitted: whereby I avoided this usual comparison of the annual number of deaths with the existing population, until I could show at the same time the fallacy already mentioned, of so doing without reference to the increase of population. In fact, the annual rate cannot be computed at less expense of labour than a calculation of the law of mortality throughout human life in each county; nor would the result of such labour be satisfactory, or indeed valuable, considering the familiar, the daily, migration of individuals from one county to another.

I have heretofore had occasion to notice the difficulty of obtaining an accurate knowledge of the first year of life*, in which, according to the most probable accounts, more than one-sixth (18 per cent.) of male infants—more than one-seventh (15 per cent.) of female infants—usually perish. This granted, the annual rate of mortality of Males in England and Wales is less than one in forty (40·5), their maximum expectation of life occurring at four years of age, and being then nearly fifty years; one-half of them attain to the age (*vie probable*) of 43·5 years. The annual rate of mortality of females is one in 44 (43·7); their maximum expectation of life, which is not less than fifty-three years, occurs at three years of age. One-half of them attain to the age (*vie probable*) of forty-eight and a half years. The expectation of life of the two sexes conjointly is very nearly forty-two years at birth; which also represents the annual mortality of England to be as one in forty-two, were the population stationary.

I should commit a crime, or, at least, be guilty of a grievous defect in my endeavour to convey knowledge to your readers, if I left those who may refer to the tables of English mortality, which you inserted in your 16th volume (p. 585), at a loss to understand why the expectation of life there given differs half a year (as 39·96 to 40·5 in male life; as 43·2 to 43·7 in female life) from what is here stated. The distinction is, in truth, somewhat technical between the expectation of life *at birth and under one year*: the last phrase always meaning the middle of that year; because some begin to live in the first day of every year, some in the last day; wherefore the age of an individual is always assumed by deducting one half year from his next birth-day. This, indeed, is not strictly true, especially in the first year of life, because many more die in the first months of their life than in the rest of the first year. This is an anomaly difficult to be overcome; and the second year partakes of the same obstacle. In fact, the best calculators are so little in unison as to the first year of life, that in the tables of M. Quetelet, which you have inserted (vol. xv. page

* Med. Gaz. vol. xvi. p. 272.

599), the expectation of life at birth is stated at 26, 24, and 20 years, in England, Belgium, and France, respectively*. Yet all these are raised, and with sufficient coincidence, to about forty years' expectation in the next year; and from five to sixty years of age differ no more than English counties differ from each other; and considering that few insurances or other contingencies rely on a life younger than five years, or above sixty years, the expectation of life is sufficiently established for practical purposes; the same result as from stationary population being attained by the official necessity of watching the termination of every life enrolled in the Government Annuity-Office, and the Equitable Assurance, to an amount (already stated) of 30,000 deaths. Previously to the life-annuity labours of Mr. Finlaison, and the publication of the experience of the Equitable Assurance-Office, nothing really authentic existed, unless we may except the earliest and best work on the subject, by Mr. Kerseboom (published 1742), whose materials commence in the year 1615. A reprint of this very scarce book, or rather a translation of it into French or English, is a great desideratum, and, with such notes as might now be appended from more modern experience, would form a most valuable addition to what Laplace has well named *Statistiques vitales.* The essay of M. Deparcieux (also constructed on government annuities), *Sur les probabilités de la durée de la vie humaine*, is as good as could be produced from his materials; but these were not sufficiently distinct to confer much permanent value on the results†.

I must now be permitted to recur to the *Scale of coincidence*, which appears in a former part of this letter, because the *vie moyenne* therein becomes 44·4 years, supposing no increase of population to have occurred; whereas the expectation of life at birth, which ought to be the same, has since been stated at forty-two years, and of course (according to Laplace), the rate of annual mortality would in that case also be as one in forty-two. I do not undertake to reconcile this discrepancy, which, indeed, would be to affirm, and even to warrant, the accuracy of a scale merely tentative and suppositious, in so far as it transgresses the limit of experience. This limit occurs at about 20 per cent. increase of population, according to the table of English counties, and short of this increase the table is purely inferential; nor does it follow from these two or three approximations, that the *vie moyenne* must proceed to *zero* in a regular arithmetical ratio, as assumed in the scale. If the *zero* denoting stationary population stood opposite to forty-two years *vie probable*, as well as forty-two years *vie moyenne*, the congruity would be satisfactory; but I have already said that nothing certain has been theoretically established, as to the limit or progressive ratio of the *vie probable.* Examples of its result in two instances appear in a small pamphlet of much value, by Sir F. D'Ivernois, *Sur le mouvement de la population de deux paroisses de Suisse*: in which the following particulars are worthy of attention.

The parish of *Montreux* appears to have increased in population 25 per cent. in about sixty or sixty-five years, say 10 per cent. from 1801 to 1830. The number of inhabitants was 2833 at the last of these dates. The *vie probable* was thirty-nine in the year 1765, as computed from the ages of the deceased during the ten or eleven preceding years. The *vie moyenne* was then found to be thirty-six years. Similar computations from the same parish register give fifty-five years *vie probable*, forty-five years *vie moyenne*, for the first ten years of the present century.

The other Swiss parish, *Leysin* (also in the Pay de Vaud), has increased in population from 405 to 447 inhabitants (about 10 per cent.) in 80 years. In the middle of the last century, its *vie probable* was 61 years, now calculated at 64 years; its *vie moyenne* was 50 years, and is now 56 years.

M. D'Ivernois blames, as an inexcusable negligence in the last enumeration of the inhabitants of the Pay de Vaud, that their ages were not ascertained; but as he also says the sexes were not distinguished, which is indeed an unaccountable omission, how could he expect the inquiry to be extended to the age of every individual, which is in

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* In the tables appended to Mr. Milne's Treatise on the Valuation of Annuities, the expectation of life at Carlisle is stated to be 38·72 at birth; 44·68 in the next year. In Sweden to be 33·12 at birth, 44·11 in the next year. In France, according to M. Duvillard, 28·76, and 36·35; but the basis of this last computation is unknown.—See Milne's Treatise, § 855, § 859.

† See Mr. Milne's Treatise, § 865, § 868.

truth a labour of some detail? True, indeed, as regards the value of these *Lilliputian* observations on two parishes (as he modestly designates them), the omission is somewhat vexatious, as the seeming miracle of health and longevity might entirely disappear, if we had opportunity of comparing the mortality of each grade of life with the total number of individuals existing in the same grade *. For explanation of the phenomenon, we are thus driven to conjecture that from some peculiar or local cause, the youth of these alpine parishes have recourse to temporary migration, and return to spend their old age at home (as a hunted hare to her remembered seat), after the fatigues of an active life. But setting aside this supposition, the paucity of children born at these places, which seems to be rated little higher than one in fifty of the population annually, cannot but raise the average duration of life to a high figure; and if this paucity arises from the cause which limits the fecundity of 100 marriages at Geneva to 275 births, the parents of children thus prudentially restricted in number, will always be able and willing to exert the greatest care in the preservation of every child to adolescence; and as the mortality of such children may thus be diminished one-half, the *vie probable* and *vie moyenne* may consistently and actually be exalted to the high figures which are recorded in this remarkable pamphlet. I shall not advert further to it, than to infer from these examples, in which the *vie probable* exceeds the *vie moyenne*, that the same result will always be found if authentic instances shall hereafter be obtained of the particulars of any decreasing population on a large scale.

I entirely agree with M. D'Ivernois in the importance of ascertaining the ages of the living as well as of the deceased; and if this inquiry was not repeated in England in the year 1831, the omission was comparatively immaterial, because the known ages of persons existing in the year 1821 were best applicable to the ages of the deceased from 1813 to 1830; and further, because the only machinery which can be employed in enumerating English population would have been overburthened in 1831, had the extensive inquiry then made into *occupations* been accompanied with a demand for the ages of individuals. I venture to think and to hope that no such obstacle will occur in the year 1841; in which case the number of children under one year, and between one and two years of age, may be asked, and the difficult question of infant mortality in some degree illustrated.

The more I have been led to reflect on the influence of increasing population, and of the results which may be anticipated from ascertaining its progress and amount, the more importance I attach to the inquiry, as one which may enable us hereafter to decide from the ages (the grades of age) of the existing population of any nation, the ratio and amount of its increase retrospectively during seventy or eighty years; that is, as far backwards as leaves a competent number of individuals alive at those advanced ages; and thus as years roll on, statistical knowledge will acquire a continuity which time only can bestow, and which will greatly increase its value.

In the meantime examples worthy of notice may perhaps be drawn from the several English counties—that is, by a more accurate investigation of the parish registers during the last century; for it should be understood, that from imperfect knowledge of parish registers at the time of collecting the returns in 1801, many omissions unavoidably occurred in the several counties; but as the deficient returns were afterwards obtained and placed in a supplement, the entire of England became respectably complete; not so the several counties, from a comparison of which with each other, much may be expected; and I am persuaded that a calm revision of the original Parish Register Returns of 1801 (which are safely preserved), will throw new light upon the subject, because many of the returns for the last century were defective, especially in the early part of it, which defects could not be accurately weighed in the formation of the abstract of 1801; but the inquiry of 1831, by which was obtained a complete list of all parish register-books now extant, furnishes the requisite knowledge, especially from 1754 (the date of the Marriage Act), which goes far back into the life of the oldest persons now in existence. I shall therefore apply myself to this task, if permitted (as I doubt not) by competent autho-

* For a decisive illustration of this kind relative to the mortality of North York and West York, see vol. xvi. pp. 270-271.

rity; and shall deem myself well rewarded in applying to a modern purpose of useful knowledge materials unattainable elsewhere in competent extent, and of such unsuspected authenticity as are the parish registers of England.

I remain, sir,
Your most obedient servant,
JOHN RICKMAN.

January, 1836.

SECOND

REPORT OF CASES TREATED IN ST. GEORGE'S HOSPITAL,

(*Being from July* 1, 1835, *to Jan.* 1, 1836);

WITH

"ROUGH LEAVES" FROM HIS CASE BOOK.

By R. MACLEOD, M.D.

ON the 1st of July, 1835, there were in the hospital, under my care, 26 patients. There have since been admitted 106, giving a total of 132.

Of the above cases there have been disposed of	100
Results unknown*	9
Remained under treatment January 1, 1836	23
Total	132

The 100 "disposed of" are as follow:—

Discharged cured	69
——— relieved	8
——— without relief	7
——— for misconduct	1
Transferred to the care of others	3
Removed by relations	1
Dead	11
Total	100

The deaths were produced by the following diseases:—

Organic diseases of the heart, with dropsy	3
Pericarditis	1
Phthisis	2
Bronchitis	1
Tuberculated liver, with ascites	1
Encephaloid disease of liver	1
Abscess in cerebellum	1
Fever	1
Total	11

* Owing to the cases not having been regularly kept, during a short period when I was absent from London.

Deducting from the total 132 the seven of whose cases I have but incomplete notes, and the three which were "transferred," there remain 122, which are accounted for as follows:—

CEREBRAL DISEASES—8.

Apoplexy, with hemiplegia, 1.—Under treatment, convalescent, 1.
Hemiplegia, 1.—Relieved, 1.
Active determination to brain, 1.—Cured, 1.
Chronic disease (organic?) of brain, 1.—Stationary, 1.
Epilepsy, 1.—Stationary, 1.
Mania, 2.—Cured, 1; relieved, 1.
Abscess of cerebellum, 1.—Dead, 1.

SPINAL DISEASES—2.

Paraplegia, 2.—Discharged without relief, 1; under treatment, convalescent, 1.

VARIOUS NERVOUS DISEASES—7.

Chorea, 2.—Cured, 1; under treatment, convalescent, 1.
Hysteria, 3:—

Prominent symptom, palpitation	1
——— pain of left side	1
——— globus	1

Cured, 3.

Paralysis of arms from lead, 2.—Relieved, 1; discharged for misconduct, 1.

Very little has occurred in connexion with the diseases of the nervous system which is worthy of being recorded. In the case of paraplegia, as well as that of partial paralysis of the forearm resulting from the poison of lead, strychnia, pushed to such extent as to produce decided constitutional effects, had no favourable influence whatever on the disease; and this corresponds with almost all the trials I have made of the remedy; so that I cannot venture to say I have ever seen it unequivocally serviceable.

The cases of chorea suggest the remark that although carbonate of iron generally proves successful, yet I have seen several instances wherein it has failed to cure the disease within a reasonable time, even when given in large doses. When this happens I have recourse to the *liquor arsenicalis*, and with invariable, or almost invariable, success. An illustration of this occurred in one of the cases included in the above return; but as it was given in a former number of your journal, I shall not make farther reference to it here. (See case of Ann Gelling, Med. Gaz. Nov. 21, 1835, page 285.) The shower-bath is well known

to be a powerful auxiliary remedy, but its good effects may be partially or entirely prevented where the child is very much afraid of it. This occurred in a little boy at present in the hospital, who became decidedly worse immediately after the use of the bath; so much so, indeed, as to render it necessary to discontinue it after a few trials.

Several interesting points of inquiry are suggested by the case of abscess in the cerebellum discharging through the ear, but as these also have already been briefly touched upon, (Gazette, Oct. 31, p. 157), I refrain from entering upon the subject here.

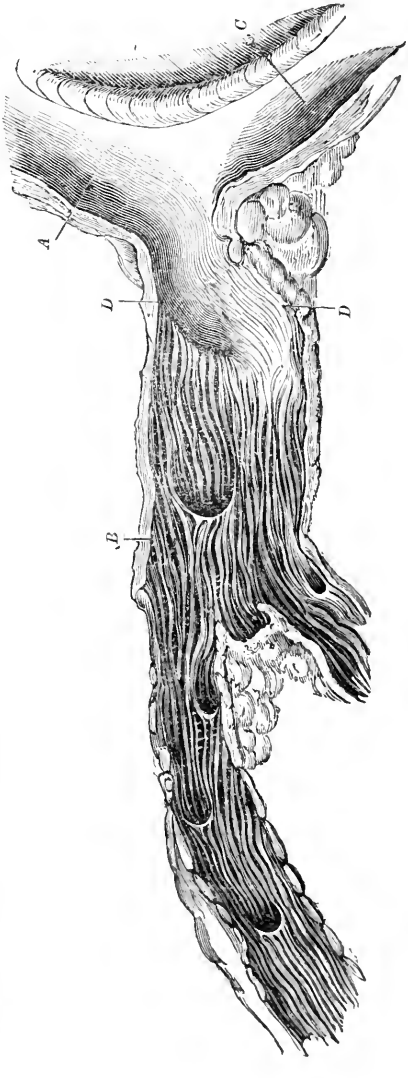
DISEASES OF THE RESPIRATORY ORGANS—18.

Chronic disease (tubercular?) of the larynx, 2.—Discharged without relief, 1; removed by relations, 1.
Phthisis, 8.—Dead, 2; relieved, 3; under treatment, 3.
Hæmoptysis, 2.—Cured, 2.
Bronchitis, 3.—Dead, 1; convalescent, 1.; Cured, 1.
Pleuritis, 1.—Cured, 1.
Pulmonitis, 2.—Cured, 2.

DISEASES OF THE HEART AND ITS INVESTMENTS—11.

Acute pericarditis (rheumatic), 4.—Dead, 1; cured, 2; convalescent (since cured), 1.
Organic disease, without dropsy, 2.—Relieved, 1; under treatment, 1.
Ditto, with dropsy, 5.—Dead, 3; cured (as regards the dropsy), 2.

The fatal case of bronchitis occurred in a man 55 years of age, who had been subject to repeated attacks of cough, with expectoration, which had latterly become complicated with anasarca. He was brought into the hospital for an aggravation of his habitual symptoms, or rather for a more acute attack supervening upon the chronic disease. The air could be heard entering the lungs pretty freely, but with a peculiar sibilus universally, except over the larger bronchi, in which situations there was a hoarser rhonchus. The expectoration was extremely scanty, and consisted of very tenacious mucus. On opening the body the mucous membrane of the trachea presented nothing remarkable, but just below the bifurcation, and into the minutest ramifications which could be traced, it was thickened, and formed prominent plaits or folds running longitudinally, with narrow interstices. The air tubes contained very little mucus. Some idea of the appearance may be gathered from the accompanying engraving, made from a drawing, which my colleague Dr. Hope had the goodness to execute for me.



A, the trachea.
B, the right bronchus.
C, the left ditto.
D, points at which the striæ of the mucous membrane commenced.

The case entered as rheumatic pericarditis, which proved fatal, was one in which the attack supervened upon old disease of the heart, the chief point of interest consisting in the state of the semilunar valves. I subjoin the case.

Case in which the Semilunar Valves were perforated.

Margaret Collard, ætat. 24, admitted December 23, 1835. Had been frequently subject to rheumatism, an acute attack of which had commenced about a week previously. The prominent symptoms were—great anxiety; pain in the region of the heart, with rapid tumultuous action of the organ, and a loud bellows sound. She was bled twice to ℥xii. (viz. on the 23d and 24th), and took large doses of calomel and opium. She died on the 26th, and the body was examined next day.

The pericardium was every where glued to the heart by firm adhesions, which had every appearance of being ancient; but the membrane covering the left ventricle, particularly towards its base, was minutely injected, and seemed soft and pulpy, as from recent inflammation. The heart was of the natural size; the semilunar valves, both pulmonary and aortic, but particularly the latter, were remarkably attenuated. The half of each valve next the free edge was as thin and transparent as the finest membrane; and two of the aortic valves had numerous (five or six) perforations close to the margin, giving it the appearance of a kind of net-work: the thin portion terminated abruptly by a line running across the middle of the valve, dividing it into two parts, that next the vessel having the usual appearance; that next the margin having the aspect above mentioned. Only one corpus aurantii was present; it existed in one of the aortic valves, and was placed in the centre, not at the edge. The mitral valves were much thickened, and one of them particularly shortened, as if a portion of it had been glued to the parts adjacent.

DISEASES OF THE FAUCES—3.

Ulceration (syphilitic), 2.—Cured, 2.
Cynanche tonsillaris, 1.—Cured, 1.

DISEASES OF THE ABDOMINAL VISCERA—24.

Chronic muco-gastritis, 3.—Cured, 3.
Indigestion (functional), 3.—Cured, 3.
Ditto (from organic disease), 2.—Stationary, 1; under treatment, 1.
Hæmatemesis (from disease of stomach?) 1.—Under treatment, 1.
Enteritis, 1.—Cured, 1.
Diarrhœa, 2.—Cured, 2.
Chronic disease of bowels following fever (ulceration?), 1.—Cured, 1.
Colica pictonum, 2.—Cured, 2.
Hepatitis (chronic), 2.—Cured, 2.
Encephaloid liver, 1.—Dead, 1.
Organic disease of liver, with dropsy, 3.—Cured, 1; dead, 1; under treatment, 1.
Abdominal tumors, nature not ascertained, 2.—Stationary, 2.
Kidney, disease of, with dropsy, 1.—Convalescent (as regards the dropsy), 1.

DISEASES OF UTERINE SYSTEM—4.

Leucorrhœa, 1.—Cured, 1.
Amenorrhœa, with various hysterical symptoms, 2.—Cured, 2.
Ditto, with hæmatemesis, 1.—Cured, 1.

In connexion with the above list, which includes diseases of the stomach, I may introduce an—

Additional Note regarding Kreosote.

In my last report, published in July, I stated that I had tried Kreosote in several cases of vomiting, and that the result of my experience led me "to regard it as of doubtful efficacy." More extended observation has not induced me to change this opinion, and I am now quite satisfied that its powers have been greatly overrated, and that it is inferior to hydrocyanic acid in allaying morbid irritability of the stomach. Almost immediately after the appearance of my former paper an account was published of cases treated at the North London Hospital by Dr. Elliotson, from which it would appear that the success of the Kreosote there, was very different from what it had been at St. George's. I can only say that I have fairly, and without prejudice, given the result of what I have seen, and have to regret that in my hands the remedy has not proved equally successful. It answered best in hysterical patients, in whom nausea and vomiting supervened; but as in such cases the symptoms are apt to come on and to subside suddenly, it is sometimes difficult to decide whether their cessation can fairly be attributed to our medicines. Where relief was afforded, this usually resulted from small doses; at least, if four, or at most six drops, did not answer the purpose, I have very rarely seen any benefit from three times that quantity.

In my former notice I pointed out an effect of kreosote not previously described, namely, that of communicating a blackish colour to the urine.

In a postscript to his paper, dated *August*, Dr. Elliotson adverts to the same circumstance as a phenomenon which had been mentioned to him, but which it is evident, from his mode of expressing himself, he had not seen. As no reference is made in either of the above papers to my former "Note regarding Kreosote," the appearance of the cases treated at the North London Hospital, and of the postscript to Dr. Elliotson's paper, the month after my paper was published, must of course be regarded merely as singular coincidences.

FEVERS—6.

Intermittent (tertian), 1.—Cured, 1.
Continued, 5.—Cured, 4; dead, 1.

In the case of fever which proved fatal, the disease was already far advanced before the patient was admitted; and the ileum was found, after death, to be ulcerated.

EXANTHEMATA—4.

Variola (severe, after vaccination), 1.—Sent to Small-pox Hospital, 1.
Scarlatina, 2.—Cured, 2.
Erysipelas (of face), 1.—Cured, 1.

CHRONIC DISEASES OF SKIN—3.

Impetigo, 1.—Cured, 1.
Lepra, 1.—Cured, 1.
Syphilitic eruption, 1.—Under treatment, 1.

RHEUMATISM—30.

Acute (without symptoms referrible to the heart), 10.—Cured, 6; convalescent (since cured), 4.
Chronic, 9.—Cured, 5; relieved, 2; convalescent, 2.
Chronic pain of various joints, with thickening of parts, 5.—Cured, 3; convalescent, 2.
Gonorrhœal rheumatism, 2.—Cured, 2.

DROPSIES, NOT APPARENTLY CONNECTED WITH ORGANIC DISEASE—4.

Œdema (more or less general) from exposure to cold, 4.—Cured, 4.

Bleeding in Acute Rheumatism.

I presume that most practitioners are in the habit of abstracting blood in cases of acute rheumatism, but so far as my observation has extended, the depletion is practised on principles very different from those which are acknowledged in regard to other inflammatory diseases. I mean, either that the bleeding is looked upon only as favouring the operation of certain remedies which are to follow, or at all events as a means which may moderate, but cannot be expected to *extinguish* the rheumatism, and which therefore requires to be aided by certain routine prescriptions. The more I see of rheumatism *in its acute form*, the more am I disposed to think that it is amenable to the same laws as other inflammations; and the object of the present remarks is very briefly to direct attention to this point.

When an attempt is made to argue that rheumatism ought to be treated like other inflammations, the most common answer is, that it is a "specific" disease; and by this all its peculiarities as to pathology and treatment are supposed to be accounted for. But I suspect that this expression is often used rather loosely, and without any very *specific* meaning. If it be intended to intimate that rheumatism is not identical in its phenomena with various other inflammations, it is very true; but the same thing holds good equally with regard to the inflammations of many other textures, as well as of those implicated in rheumatism. Pleuritis is manifested by different general symptoms and different organic changes from bronchitis; and pulmonitis is in those respects different from either. Inflammation of skin differs from that of cellular tissue; that of veins, from both. Why, then, should we think it necessary, in the case of rheumatism, to seek for any other causes to explain its peculiarities different from those which we admit as explanations of the analogous peculiarities observable in the inflammations above enumerated? I say, that the difference between the inflammation of the fibrous and synovial textures in rheumatism, and that of any other texture which may be assumed as a standard of comparison, is not greater than between such standard and various other inflammations which might be named. Again, if by "specific" be meant that there is any thing in the exciting cause of the disease different from that of other inflammations, there is obviously no ground for the assumption: atmospheric vicissitudes give rise to rheumatism or pleurisy, or ophthalmia, and are not more unequivocally the origin of one of these than of another. With syphilis, and other really *specific* inflammations, the case is very different.

Then, as to the terminations of rheu-

matism, in what do they differ from those of other inflammations that may not be reasonably attributed to the peculiarities of structure? We have pain, heat, swelling, and redness, ending in effusion; and this effusion may either be absorbed or become organized, just as in other inflammations. And further, rheumatism may, and (I conceive more frequently than many suspect) does, go on to other and more formidable terminations, particularly that in suppuration. I have recently seen two instances of this kind, in which several joints were simultaneously attacked with pain, swelling, redness, and all the ordinary symptoms of acute rheumatism. The disease was treated as such, and the affection of several of the joints subdued, but in one case it remained persistent in the hip, and in another, both in the hip and shoulder. The patients died, and matter was found in the several joints above named.

Perhaps the most remarkable phenomenon connected with rheumatism is the rapidity with which it shifts its seat from joint to joint. This has been attributed to metastasis, and held by some to be an argument against bleeding; but I doubt whether either the inference or the ground assigned for it be free from objection. The extension of rheumatism appears to me more striking than its metastasis. When a joint is considerably swollen, it is not usual to see that swelling disappear very suddenly; but rather this: after a time the pain in the situation first attacked becomes moderated, and the disease extends to other parts successively; the swelling speedily diminishing, but usually taking two or three days after the patient has ceased to complain of pain to subside entirely. As to the metastasis to the heart, of which we hear so much, it seems to me to be quite imaginary: that is to say, imaginary as to there being any metastasis. It is infinitely rare to see a case in which rheumatism suddenly leaves the joints while the pericardium becomes simultaneously affected, but it is of every-day occurrence to see the heart passing through the disease *pari passu* with the limbs. Nay, in some instances the heart is the part first attacked. I have more than once known patients seized with symptoms of pericarditis at a time when they had no *other* rheumatic affection, and in whom, next day, one or more joints became affected with pain, swelling, and redness, the disease of the external and internal fibrous membranes thenceforward running their course together.

But it may be said that the rheumatic affection of the heart differs from ordinary inflammation; and I have met with some who have affected to distinguish between rheumatic and common pericarditis. On this point I shall only remark, that I regard the assumed power of diagnosis (independently of the presence or absence of rheumatism externally) as a piece of refinement, if not affectation.

My object in the preceding remarks has been to shew that rheumatism does not differ so much from other inflammations as, in my humble opinion, to warrant the great difference in the treatment adopted; but I would be understood as speaking at present of the acute stage alone; because if this has been suffered to pass, various changes in the pathological condition supervene, requiring other, and often very dissimilar, modes of treatment. As I have said, many assume, as an established principle, that acute rheumatism cannot be cut short by bleeding; but I know that of those who give a tolerably confident opinion on this subject, very few have ever tried the method they condemn. Last winter two severe cases of rheumatism were brought into St. George's Hospital: one presented great tumefaction and redness of the left hand and wrist of only thirty-six hours' duration: the other had more general pain of the limbs, but no swelling nor redness, and had several times suffered from rheumatism. Dr. Alexander, a very intelligent young physician, then attending the hospital, told me that he had seen acute rheumatism very rapidly cured by bleeding, without any thing else, and at his suggestion both patients were bled largely. In the former case the disease was literally *extinguished*, for the patient was well next day—quite free from pain, and had no recurrence of the symptoms: in the latter the pain was moderated, but only yielded ultimately to other remedies. Now it is in that stage of rheumatism only of which the former may be regarded as a type, that I have since, and more particularly very recently, adopted the plan of bleeding and purging in the same manner, and to the same extent, as in other acute inflammations. Hitherto I have

had no reason to distrust the expediency of this plan; and I subjoin, in illustration, some cases thus treated, not picked for the purpose, but being all the patients admitted with acute rheumatism at my last "taking in," except one case of pericarditis with old disease of the heart;—(case of Collard, described above.)

To one other point connected with this subject I beg to direct attention. I have read and heard that the risk of pericarditis in rheumatism is increased by bleeding. On what does this opinion rest? When pericarditis does occur, we bleed, and experience justifies the practice. Are we, then, to believe that the very means which, after pericarditis has occurred, tends to subdue it, if adopted when it is absent, paves the way to its approach? I apprehend that a simpler and truer explanation of the fact (if it be a fact), that pericarditis occurs more frequently when patients have been bled than when they have not, is to be found in this—that few practitioners have recourse to bleeding except in the more acute and severe cases. Now it is just in these that pericarditis is of most frequent occurrence, whatever treatment may have been employed. In one of the subjoined cases (that of Sarah Gurney) it will be seen that the patient, having as yet no pain in the chest, was bled, soon after which pain in the region of the heart came on, which still continued at the time of her admission next day, when, being bled a second time, the cardiac symptoms entirely disappeared. Now surely the fair inference from this, is not that the first bleeding caused the heart affection, but that it was not sufficient to prevent the extension of the disease to the pericardium, then already supervening.

But I by no means wish in any case to limit the treatment of the most acute rheumatism to bleeding (except to ascertain its actual degree of efficiency.) I only wish that if it be entitled to the same rank here as in other acute inflammations, it should enjoy that rank. The more rapidly the inflammation of any important part produces effusion, the more speedily must measures be adopted to counteract this tendency. Thus in croup we bleed to reduce the inflammation, and probably if we were confined to one remedy this would be the safest to choose; but as we are not so limited, and as the effusion of lymph into the trachea might suffocate the patient, even although the inflammation which produced it had ceased, so, while we bleed, we also give calomel with a liberal hand. Now in rheumatic pericarditis, even if we were certain that blood-letting alone would arrest the inflammation, we should still in common prudence give mercury, as the best antidote to the effusion; and thus in the case of Collard above mentioned, it was begun in doses of five grains every four hours, combined with opium, and afterwards continued in doses of five grains every six hours.

Having thus endeavoured to guard myself, I trust I shall not be misunderstood as to the manner or extent to which I would suggest that a trial be given to blood-letting and purging on the onset of acute rheumatism; and I beg to refer to the recent work of M. Bouillaud for a more elaborate and skilful exposition of the same doctrines—doctrines which in France have much more novelty than here, where, after all, the question of bleeding is only one of degree.

CASES OF ACUTE RHEUMATISM.

Case I.—*General Bleeding twice, and Leeching once, in 24 hours; Purging with Senna and Salts; Cure.*

James Crow, ætat. 19, footman.

Dec. 17.—Was seized a week ago, after exposure to cold, with acute pain in the feet and ankles, which parts soon became considerably swollen and red. He had fever, and was confined to bed. Two days ago the pain in the lower limbs began to abate, the wrists becoming simultaneously affected in a similar manner. This continued to increase; and on his admission, which took place yesterday afternoon, the left hand and wrist were considerably swollen, and very painful. The pulse 88, bounding.

He was bled to ℥xii. and ordered a senna draught, which was taken this morning.

Says the pain is better to-day, but not gone. The swelling is also diminished. Pulse 74, very large, and with a peculiar harshness. No pain or uneasiness referred to the region of the heart; but the ear detects a loud bellows sound alternating with the pulsations at the wrist.

It appears that about four years ago he suffered from violent palpitations, and has ever since been subject to a recurrence of

the symptoms on any slight exertion. Has never had rheumatism before.

The blood drawn yesterday exhibits a large and very firm coagulum, but without buff or cupping. Tongue thickly furred; bowels freely purged.

Mittatur Sanguis ad ℥xiv. Applicentur Hirudines xii. sterno. Rep. Haust. Sennæ cras mane.

18th.—Entirely free from pain; swelling of wrist, &c. considerably diminished. Blood exhibits the same characters as before. Bowels again freely purged; pulse 72, soft and natural; bellows sound of heart much less remarkable.

On the 19th he had no pain, and the swelling was nearly gone. On the 22d, having continued entirely free from complaint, he was discharged, with directions to apply to me (as he lived in my neighbourhood) if he had any recurrence of his complaint. I have not since heard of him.

Case II.—*General Bleeding thrice, and Leeching once, in five days: Purging with Senna and Salts; Cure.*

James Sharp, ætat. 21, labourer.

Dec. 23, 1835.—Has suffered for ten days from pain in the knees, attended with swelling. Two days ago the hands and wrists were also attacked: they are now swollen, with slight redness, and total inability to move the parts. He has also acute pain in the right shoulder, which, however, does not appear to be swelled. Pulse 84, unequal, and with occasional intermission; no uneasiness referred to the heart, and no anormal sound to be heard on applying the stethoscope over it; skin pungently hot; tongue furred.

Mittatur Sanguis ad ℥xx. Hab. Haust. Sennæ cras mane.

24th.—Pain much diminished in knees and shoulders, but not in the wrists; pulse and skin as yesterday; bowels purged; tongue still loaded; blood drawn yesterday exhibits a large firm slightly buffed coagulum.

Rep. Venesectio ad ℥xx. quamprimum. Haust. Sennæ cras mane.

25th.—Says that he "has very little pain any where;" moves his hands freely, having no pain in them, and much less swelling; pulse 72, soft; blood much cupped and buffed; bowels have acted only once, and that but slightly.

Rep. Haust. Sennæ quamprimum et cras mane.

26th.—The only remaining pain is in the right knee, which is still rather "sore." Bowels have been freely purged; tongue cleaning.

Applicentur Hirudines xiv. genu sinistro. Haust. Sennæ alterno quoque mane.

28th.—Knee was greatly relieved by the leeches; but was attacked with pain in the right hand and wrist yesterday afternoon, the part being now swollen, and with perceptible redness. [This relapse followed within a few hours after the weather became damp, on the breaking up of the frost.]

Mittatur Sanguis ad ℥xii. Rep. Haust. Sennæ cras mane.

29th.—Is again entirely free from pain, and complains only of weakness.

[This patient was kept in the house in consequence of one of the leech-bites festering. He continued free from complaint till the 18th January, when, being told that he must leave the house, he next day said that he had a recurrence of pain in the right shoulder: this pain (which I presume never existed) left him as soon as the immediate risk of being discharged had passed.]

Case III.—*Bleeding twice, Leeching once, in five days: Purging with Senna and Salts; Cure.*

John Pater, ætat. 25, coachman.

Dec. 23d.—Admitted yesterday, when he complained of acute pain in the feet, ankles, and knees, with swelling, and a slight blush of redness. Pulse 96; skin hot; tongue furred. Was ordered yesterday—

V. S. ad ℥xiv. Haust Sennæ cras mane.

Blood cupped and buffy; bowels purged; symptoms much relieved.

Pulv. Ipecac. Comp. gr. x. h. s. Haust. Sennæ cras mane.

25th.—So much better as to have got up and walked about the ward this morning (without leave).

Rep. Medicamenta.

26th.—Rather more pain last night. Pulse 94, with some sharpness.

V. S. ad ℥xii. Rep. Alia.

28th.—No pain since he was last bled, except a little in the right knee at night; pulse 80, soft; bowels moderately opened each day.

Hirudines xii. genu dextro. Rep. alia.

From this time he rapidly recovered, without any farther relapse.

Case IV.—*General Bleeding and Leeching: Purging with Calomel and Black Draught; immediate Relief; Relapse on fourth day: Repetition of Bleeding and Purging; Cure.*

Sarah Gurney, ætat. 18, house-maid.

Dec. 21th.—Attacked four days ago,

after exposure to cold, with acute pain in the feet, ankles, hands, and wrists, all of which are swollen, and the latter of which are red. Has pain also, though not to so great a degree, in the back and larger joints; countenance is expressive of much suffering; and she either lies perfectly motionless, or cries with pain when moved. Has pain in the region of the heart, which, she says, came on the day before yesterday, she having been bled in the morning. On applying the stethoscope, a slight bellows sound is perceptible;—somedegree of rubbing sound (?) Pulse 100, bounding; skin hot and dry; tongue dry and furred; bowels purged by medicine taken before her admission.

Mittatur Sanguis ad ℥xiv. Applicentur Hirudines xx. regioni cordis.
Calomel gr. iij. hora somni. Haust. Sennæ cras mane.

25th.—Appearance greatly improved; no pain whatever in the chest, and that of limbs nearly gone; swelling much diminished; chief remaining pain is across the back, between the shoulders; bowels have only acted once; pulse 90, softer; skin cooler; tongue cleaner.

Hab Haust. Sennæ quamprimum et cras mane.

26th.—No pain, except in a slight degree when she moves; swelling of ankles and wrists has almost entirely disappeared; pulse 90, soft; no anormal sound perceptible on examining the chest; sleeps badly.

Pulv. Ipecac. Comp. gr. x. hora somni.
Haust. Sennæ al. mane.

28th.—Suffered a relapse yesterday afternoon (in common with several other rheumatic patients), in consequence of a sudden change in the atmosphere. The left hand and wrist are again swollen, and very painful, with perceptible redness; pulse 104, sharp; tongue pretty clean, but dry; bowels freely purged.

Mittatur Sanguis ad ℥xii. Calomel gr. iij. h. s. Haust. Sennæ cras mane.

She was next day entirely free from pain, and suffered no farther return of the symptoms, having been detained in the hospital till January 13, to guard against the risk of relapse.

Hydriodate of Potass in certain forms of Chronic Rheumatism.

It will be seen by the preceding table that several of the cases admitted as rheumatism were of a chronic character, and attended with thickening of the periosteum, or induration about the joints, particularly the ankles, wrists, and fingers. As a general rule such cases are benefited by sarsaparilla, and some of the above were unquestionably of syphilitic origin. In more than one, however, the effect of the sarsaparilla seemed to carry the patient on to a certain point in his cure, after which its effects ceased. Under such circumstances the hydriodate of potass, in doses of gr. iii., gradually increased to gr. xv., three times a day, speedily renewed the curative process, and completed the recovery. These were cases in which mercury had been freely used at an earlier period. In several other cases of thickening about the periosteum and ligaments, not apparently connected with syphilis, the hydriodate of potass was attended with speedy and decided benefit. One of those was a very obstinate case of inflammation of the synovial membranes of the ankle-joint and middle finger, connected with gonorrhœa: the disease was rapidly cured by this remedy, after a great variety of others had been tried in vain. A case of chronic enlargement of both wrists and both ankles, of nine months' standing, and of rheumatic origin, is now under treatment, and is rapidly yielding to the sarsaparilla with hydriodate of potass.

Note on the rate of Mortality in St. George's Hospital.

In the British Medical Almanack for 1836, a table is given, which professes to illustrate the comparative rates of mortality in the different hospitals of the metropolis. By the table in question it would appear, that at St. Bartholomew's the rate is only 7·6 per cent., while, at St. George's, it is 10·6. But, in estimating the value of such statements, it is essentially necessary to be made aware of the data on which they are founded, and of how far the results given are deduced from corresponding premises. In all hospitals at which wards are set apart for particular diseases, not generally of a fatal nature (such as venereal or ophthalmic cases, or certain chronic maladies—as uterine affections, &c.), the rate of mortality must necessarily be lower than at those institutions where this is not done. Again: the rate of mortality will almost unavoidably be higher at those hospitals where the number of applicants much exceeds that which can be admitted. Now both these causes are in operation at St. George's; it has not wards for

cases of the nature first alluded to, while, as to the latter circumstance, the number of applications greatly exceeding the means of accommodation, a selection is systematically made of the worst cases. Another very important point to be inquired into is, whether persons brought into hospitals with fatal accidents are or are not returned among the deaths. At St. George's such cases are included in the list of those who have been treated in the hospital, although, in many instances, the patient is dying when admitted.

During the year 1835, the admissions amounted to 2175; the deaths to 215, or very nearly 10 per cent., but of these, not fewer than 51 were "deaths from accidents;" and deducting this number both from the admissions and the total mortality, we have the deaths reduced to about 1 in 13, or something less than 8 per cent.; which brings it within a fraction of the lowest rate. According to the British Medical Almanack, the average mortality of the Paris hospitals amounts to 12 per cent., and that of La Charite, of Berlin, to 15·1 per cent.; but this is so much greater than the mortality of the London hospitals, that I cannot help suspecting that we have not yet sufficient data on which to form a correct estimate of the comparative rates of mortality in different hospitals, either at home or abroad.

CASE OF FATAL HÆMORRHAGE FROM LANCING THE GUMS.

To the Editor of the Medical Gazette.

SIR,

IN Mr. Pereira's lecture on the history and use of Leeches, in the Gazette of the 9th of this month, he mentions the death of a child from the bite of a leech, and adds, "In some persons there appears to be an hereditary predisposition to hæmorrhage, so that very slight wounds are attended with serious and even fatal effects."

These observations so forcibly recal to my recollection a case which occurred in my practice at Bromley, a few years ago, that I trust you will not consider it unworthy of a place in your valuable journal. It was that of a child, who died from having the gum lanced.

On a Sunday morning I was called to visit the child of Mr. R. Stockwin, about six months old, who had been suffering for some days from diarrhœa. Observing the upper gum much swollen, I advised the mother to allow me to lance it, telling her that teething was probably the chief cause of the child's illness. To this she very reluctantly consented. About eight o'clock in the evening a messenger was sent to inform me that the child's gum had continued to bleed the whole day. As I supposed it could not be of serious consequence, and as the parents of the child lived two miles off, I sent a styptic lotion, and promised to call in the morning.

I was sent for early on the following morning, and found the child pale and exhausted from the loss of blood. It was constantly crying and sucking; and the mother's neck, as well as her clothes and those of the child, were covered with blood. I never, in the course of a very long practice, witnessed so deplorable and distressing a sight; and it was very difficult to know what to do. I tried styptics in various forms, such as a very strong solution of sulphate of copper, one of alum, spirit of turpentine, and nitrate of silver, but all to no purpose, as no application could be retained on the wound for an instant, owing to the child's constant attempts to suck. I then had recourse to the actual cautery, by means of a heated skewer; but it did not in any degree stop the bleeding, and the child died on Tuesday.

Now suppose such a case had occurred in a family of high rank, and the child had died, what a sensation would it not have caused, and how highly injurious might it not have proved to the surgeon's reputation! A similar case may happen again. Surely, then, it is important to know what mode of treatment would be likely to arrest the hæmorrhage; and I hope that some of your able correspondents will favour us with their opinion on the subject.

I ought, in conclusion, to remark, that there certainly existed in this child a predisposition to hæmorrhage, as the father told me, after its death, that if only accidentally scratched by a pin, the part would bleed for several hours.

I remain, sir,
Your obedient servant,
R. T. TWYNTON,
Surgeon.

39, Queen-Square, Bloomsbury,
Jan. 22, 1836.

FATAL CONSTIPATION CAUSED BY A KNOT OF TENIÆ.

To the Editor of the Medical Gazette.

SIR,

SHOULD you deem the inclosed brief case of constipation (on account of its cause) at all interesting to your readers, you will oblige a constant reader by inserting it.—I am, sir,

Your obedient servant,
J. K. PARKINSON, M.R.C.S.

Milverton, Jan. 20, 1836.

The parents of a poor boy, æt. 4 years, requested my attendance, on Wednesday last, in consequence of his (to use a provincialism) not having had "the use his body" for three days. From that day no means were left unemployed which were considered at all available, but with no effect; and on the Monday following, after the most acute suffering, he died. His symptoms at last presented all the features of strangulated hernia, except the local one. A few minutes before his death, his mother found he had passed something in the bed, which proved on inspection to be five teniæ, inseparably knotted together into a round hard ball. I must mention that an enema was not employed, owing to the parents persisting against it, as some ignorant neighbours (women) gave it as their opinion that it would be too strong.

MEDICAL GAZETTE.

Saturday, January 23, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

CROWN AND ANCHOR MEETING.

THE event of the week has been the meeting of medical students at the Crown and Anchor. Nearly a thousand, it is said, were congregated on the occasion—some, we believe, from a well-intentioned, however misguided, motive—others for frolic sake—and by far the greatest number out of pure curiosity. Mr. Wakley, M.P., promised to lionize: "he pledged himself," said the advertisement, "to be present." No doubt; pledging is a part of the system of the *pledged* member for Finsbury. The chair, too, it was announced, was to be taken by a Mr. Meade, a private teacher — or "grinder," as it is commonly termed—and a protegé of the *honourable* Finsburian.

Of a meeting called together under such auspices, much good could scarcely be anticipated. No professional man of any rank—no teacher who might have a moral weight with the students—could venture to be present. But Mr. Carpue and Mr. Liston were there.

The accounts which we have read in the newspapers of the general conduct of the meeting, have not been very consistent—some describing it as very decorous, and some just the reverse. We have taken pains to gather correct information on the subject, as well as concerning the previous and subsequent proceedings, and we believe that the *facts* which we are enabled to state cannot readily be assailed or denied.

It appears, then, that on Thursday evening fortnight a candidate for the license of the Apothecaries' Society was rejected, his examiners having found him insufficiently prepared. Whether he was really so or not, we cannot pretend to say, having at present no data on which to rest an opinion. The candidate himself was highly indignant, and in the heat of passion wrote shortly after a letter to the Court of Examiners, demanding a *public* examination, giving, at the same time, a copy of the said letter to the Editor of the Lancet for publication. On the subsequent Thursday evening Mr. Meade was in attendance; and the alarm being sounded, calling together, at a short notice, a large assemblage of students, this gentleman took up his

position at a public-house in the neighbourhood of the Hall, where he harangued his excited auditors from one of the windows. But this nuisance presently attained such a height, that the City Marshal had to interfere and cut short the gentleman's eloquence. On the breaking up of this tumultuous assembly, some of the windows of Apothecaries' Hall were smashed.

All was now organized for a grand display at the Crown and Anchor. Meade and Wakley were in their glory. The first denounced the *system* pursued at the Hall, of rejecting pupils of his: he amused his auditors with *his* account of the questions and answers on which gentlemen were rejected; and ended by declaiming on the necessity of reform in these and many other things. Wakley poured forth his usual virulence against all the medical corporations, particularly, of course, against "Rhubarb Hall." He prophesied, as usual, the downfal of "corruption," and the speedy uprise of "One Faculty" in its stead. He promised *his* aid (!) in Parliament to forward the cause of the students, and offered most magnanimously to present their petition.

Mr. Wakley, *M.P.*, is obliged, we all know, to labour in his vocation. If he does not keep the cauldron in a "bubble, bubble, toil and trouble" condition, his occupation is gone: hot water is his element; and if he does not attend meetings (no matter of what kind), and get into the newspapers, the worthy electors of Finsbury will discharge him. Besides, it is his interest to hoax the students,—through a pure and disinterested regard for them. And, moreover, what could he do in the present case? He met the parties, without any contrivance of his own, was pressed, and in consequence pledged himself, to attend.

Will this sort of excuse serve the turn of Mr. Liston? This gentleman, we understand, actually gave his pupils a holiday on the occasion, and hurried off hot-foot with them to the place of meeting. How amiably kind, and how considerate of the interests of his class! No doubt the opportunity was irresistible. It is a fine thing to be a popular man; and it is only what we are in duty bound to do—to support our friend Wakley. There, then, met face to face the worthy pair, in congenial coalition. Of the speech of the "celebrated surgeon," as one of the newspapers called him, we have been able to learn nothing farther than that he spoke, or perhaps attempted to speak.

As to poor Mr. Carpue, we cannot conceive what brought *him* there; unless to while away some of his idle moments with recollections of days gone by. Friend Wakley, besides, wanted *respectable* support.

Now what *are* we to think of a meeting, however quietly it may have been conducted, which was assembled under such patronage? We tell the students that they have been grossly imposed upon. They might have met together in large numbers, and have carried much weight in their proceedings (always supposing that they had a legitimate object for their assembling), had they carefully eschewed the agitators who took them under their wily protection. Such "friends," they will see, when a little older, or a little calmer, are worse than avowed enemies: at present they can scarcely appreciate the motives of those worthies.

When we say, *if* the object of their assembling were legitimate, we wish to guard ourselves from being understood to pronounce any opinion on the merits of the present case. We believe that (apart from the agitating purposes of the volunteer chairman and "friends") the avowed object of the students was the redress of grievances, real or supposed, connected with their examinations at the Hall. If those grievances truly ex-

ist, we must, of course, admit that the conduct of the students in calling a meeting, in order to secure better treatment in future, is both proper and laudable.

Now, from all we can hear, the charges made against the Court of Examiners, more particularly with reference to the recent case of rejection, are principally two; both of them of a sufficiently serious nature. The first is, that the *matter* of the examination was, as it is said frequently to be, captious, quibbling, and capricious, and that good answering was refused. Secondly, that the *manner* of the examination was uncivil in the extreme—rude, and such as to irritate and disconcert the candidate.

With regard to the first of these accusations—in support of which we have at present no more than the vague assertions of certain rejected men and their grinders—we would propose that it be put to the proof. We say, *let the late examination be published; let the Court of Examiners be called upon to lay before the public the questions and answers in the case under discussion;* and if they refuse, let judgment go by default—the world will know what to think of their refusal. But this cannot well be done without the consent of the rejected candidate: let him, therefore, instead of calling for a *public* examination (contrary to all precedent, and which would lead to obvious inconveniences and difficulties hereafter), *let him demand the publication of the examination of which he complains;* and if the Court of Examiners refuse to concede that very fair request, they will stand condemned by their own act, and we shall be among the first to pronounce sentence upon them.

The other charge is one, for which, perhaps, so simple a mode of defence cannot easily be suggested. There are so many ways of shewing incivility, so many modes of taking it, and it is altogether so relative a thing, that it cannot readily be adjusted by any invariable standard. But there is a degree of rudeness too strongly marked to be misunderstood; and such, we apprehend, is the case in the present instance. We have reason to believe, upon what we consider to be unquestionably good authority, that the conduct and bearing of some of the members of the Court, to those who come before them, are not only wholly deficient in the amenities which conciliate, but repulsive and unwarrantably coarse. There are certain members, in short, who are particularly noted for this sort of behaviour to candidates; and we have no hesitation in saying, that if they cannot conduct themselves otherwise, their presence ought as speedily as possible to be dispensed with. It *may* be that the examinations are sufficiently good; but who can expect fair play where such petty tyranny, and paltry displays of boorish bad manners, are exercised? Besides, the example is catching: when the leading members of the Court pour out the venom of their spleen on those with whom they come in contact, it can scarcely be expected that the Officers of the Court should be more orderly or better-behaved than their superiors. We regret to learn that gentlemen are in numerous instances subjected to incivilities from the very moment, or even before, they cross the threshold of the establishment at Blackfriars.

The redress of these sources of annoyance, is, we repeat, a very proper object for the intervention of parties really aggrieved by them. But such persons should take care to ascertain that the first grievance—that relating to the alleged captiousness of the examinations—has some other foundation than the interested statements of grinders and restless agitators. Any hook will serve a selfish, mortified, and unemployed legis-

lator, to introduce himself into the councils of those who must eventually be injured by his interference. Nor are the students to fancy that their cause can in any way be bettered by the co-operation of a couple of disappointed surgeons, long noted for their turbulence and the pertinacity with which they thrust in their "noses" where they are not wanted. Let such gentry be eschewed altogether: and with moderation and gentlemanly demeanour in the conducting of such measures as they may think it prudent to adopt, the medical students of London may exhibit an example to those of the provinces and other parts of the empire, at once creditable to themselves and the nation to which they belong.

Since the preceding was written, we have seen a public protest, on the part of the King's College students, "disclaiming all participation in the intemperate proceedings of the Crown and Anchor Meeting." This at least shews that the business of Monday night was not the act of the *general* body of the London students. We have heard, moreover, that other schools also intend to enter a similar disclaimer.

RESUSCITATION OF THE APPARENTLY DROWNED, OR DEAD.

We perceive, from the last report of the Royal Humane Society (1835), that it is in contemplation still further to simplify those admirable precepts, which have been so extensively circulated, for the recovery of persons in a state of suspended animation. Experience seems to show that the practice of attempting to inflate the lungs had better be dispensed with; at least by non-professional people. What Mr. Dalrymple says on the subject, in a note addressed to his brethren of the Committee, is particularly worthy of being quoted:—

"It has been the peculiar object of the Committee that the instructions should not only be simple and of easy application, but, above all, that they should be safe and harmless in the hands of non-medical persons; because artificial respiration is exceedingly difficult of performance by persons ignorant of the structures concerned, or unaccustomed to the operation; and even if they succeed in introducing air into the lungs, there is still much danger attendant upon the mode by which it is impelled, and the force and degree of violence used. This the annual reports of the Society within the last few years, and since the publication of M. Leroy's excellent memoir, have, to a certain extent, insisted on. It is also a singular fact, that one of our most active and useful medical assistants, Mr. Woolley, residing near the Serpentine, *in all the cases in which he has been successful in restoring suspended animation, has never performed the operation of inflating the lungs;* and in all those cases wherein he did use the bellows, his exertions were of no avail. I do not mention this to throw discredit upon the process of artificial respiration, since I firmly believe that there are cases in which it has been, and will still be, of the highest service; but rather to show that those cases are comparatively rare, and that with the difficulty of its application by non-medical men, the dangers attendant on its use are not only augmented, but much valuable time is lost which might be better employed."

The same gentleman also notices the danger of foreign matters passing into the air-passages, in the act of introducing brandy and water into the stomach, ere the contents of that organ have been satisfactorily removed. He suggests, in consequence, that with regard to these several points, some modification might be adopted in the instructions, suited to the improved state of medical knowledge.

We are glad to observe another excellent remark of Mr. Dalrymple's, relative to the detailed methods of treatment given in the Society's yearly reports. This detail, he suspects, is frequently the sole guide of medical men, especially junior practitioners, in their treatment of asphyxia. "It is therefore," he says, "of the highest importance to the Royal Humane Society, that its published methods of treatment should be at once the best and the most scientific text-book upon the nature and treatment of asphyxia, from

whatever cause originating, whether with reference to the additions which are constantly making to our present stock of knowledge on that difficult and interesting branch of the medical art, or as it regards the best and most approved apparatus to be used for the purpose of restoring life.

"The question is, therefore, whether it might not be advisable for the Committee to solicit the co-operation of several of the medical friends of the institution, in reviewing what has been already done by others, verifying or not their conclusions; and instituting a series of experiments, with a view to the full development of the theory and treatment of asphyxia."

It is the intention of the Committee, it appears, to follow up Mr. Dalrymple's observations; and they hope to have it in their power to give, in their next annual report, the results arrived at by the medical referees. We need scarcely add that we shall be anxious to hear more on this important subject.

LECTURES ON SUBJECTS CONNECTED WITH CLINICAL MEDICINE;

Delivered at St. Bartholomew's Hospital,

BY DR. LATHAM.

ON SYMPTOMS.

Possible Fallacies of Auscultation—How it may lead to an erroneous Diagnosis; in Pneumonia; in Dilatations of the Bronchi; in Emphysema—Pathology of Dilatations of the Bronchi—Pathology of Emphysema—How the intimations of Auscultation and Percussion may apparently contradict, yet really confirm, each other.

I HAVE endeavoured to speak of the Auscultatory Signs that belong to the Lungs as plainly as I could. First I took them singly, and tried to fix their separate value by a reference to the simpler forms of disease; and then I took them together, seeking their relative and combined value in forms of disease that are more complex. And thus I found that a single Pathological Condition might be denoted by a single Auscultatory Sign; that in Bronchitis, when it had not passed the stage of mere vascularity, there was nothing but a *dry* Sibilus, and when it had reached the stage of effusion, nothing but a *moist* Crepitation. Moreover, I found that, as diseases were cumulative, so might their Auscultatory Signs be cumulative also; and that a mixed case of Pulmonary Consumption often contained every one of the Auscultatory Signs that have been mentioned.

These Signs, both singly and cumulatively, I have followed as far, perhaps, as they can be altogether trusted in the diagnosis of pulmonary diseases. Not that, beyond this point, they have no further aid to contribute, only they need more confirmation from concurrent circumstances, and require more care on our part, to avoid certain errors, to which, if too implicitly relied upon, they are apt to conduct.

Auscultation is not infallible. I have known it betray the most wary and experienced into downright error; as when a certain sound, which in forty-nine cases indicates one thing, has in the fiftieth case indicated another. You ought to be aware how this may happen. As also, when the several Auscultatory Signs of one and the same case seem to set themselves in opposition to each other, one indicating this thing, and another that; one confuting what another affirms. This apparent contradiction is at first very perplexing; but, being understood, turns out to be no contradiction at all; and the real consistency and the apparent contradiction furnish together a sure diagnosis of a particular form of pulmonary disease. You ought to be aware how this, too, may happen.

In what manner, then, an Auscultatory Sign may give false intimations, and how several signs may seem to contradict, and yet be perfectly consistent with each other, I will now endeavour to explain. For this purpose I must touch a little upon some points of Pathology.

It has been by the light of certain facts in Pathology, considered as general truths, that Auscultation has reached some of its most important conclusions.

Of such facts these two deserve to be especially noticed; viz. that Tubercles and Tubercular Cavities have their origin in the upper lobes, and Inflammation its local origin in the lower lobes, of the lungs.

These two facts are so generally true, that they have been set up as signals (if I may so say) to steer our diagnosis by; and Auscultation has reached some of its safest conclusions entirely from faith in them.

But ordinary diseases will sometimes occur under extraordinary circumstances, or in unusual situations; and then we are as apt to be thrown out in our diagnosis as the pilot is in his course upon any unexpected alteration of lights or signals

on the coast. He makes false points, and so do we.

Thus in every instance of exception to the two general truths which have been specified, there is a perilous chance that Auscultation will lead us wrong.

In forty-nine cases out of fifty Pectoriloquy is a direct symptom of a cavity formed in the lungs, the result of Tubercular Disease, or the result of Inflammation. Then comes the fiftieth case, in which there is Pectoriloquy, arising not from a cavity either tubercular or inflammatory, but from some other condition; and in this fiftieth case the Pectoriloquy, I suspect, almost always deceives us.

A young woman (Mary Taylor) was admitted into the hospital in September, 1833. For two or three years she had been liable to slight coughs, and in the last spring had suffered Influenza. The Influenza passed away, but a slight cough was still left behind.

Three days before her admission she had a rigor, inability to lie on the right side, and pain shooting from beneath the right clavicle through the chest to the scapula.

At her admission she was flushed and hot, her respiration hurried, her pulse 112 in a minute, and full and soft; and she complained of pain in the situation just mentioned.

Auscultation found a healthy Respiratory Murmur, unmixed with any unnatural sound, throughout the entire left lung, but in the right lung Cavernous Breathing, and a loud Pectoriloquy above the spine of the scapula, and small Crepitation all around it; also a space beneath the clavicle dull to the ear, and dull to Percussion, while the rest of the lung was healthy. Six leeches were the most active remedy which her strength would admit, and they were applied beneath the clavicle.

She was delirious through the night, and perspired greatly. The next day she was more flushed: her dyspnœa was aggravated; her pulse had gained in frequency, and had lost in power; she was altogether very much sunk. The Pectoriloquy was still clear and evident, and the *small* Crepitation still every where about the scapula, and moreover in front, about the mamma.

Our diagnosis in this case was, that one considerable vomica at least existed in the apex of the right lung, and that *acute* Inflammation of the vesicular structure had arisen all around it.

She was treated by remedies as active as her feeble circulation would admit, chiefly by leeches to the surface opposite the parts where the Crepitation was heard. In four days the Crepitation began to abate, and in six it was gone; and, as it gradually went away, the Respiratory Murmur gradually returned, until it entirely took its place.

But what became of the Cavernous Respiration and Pectoriloquy? These surely remained unaltered; for the vomica could not be so soon cured, although the surrounding inflammation might?

Not so. But the Cavernous Breathing and Pectoriloquy were first changed into Bronchial Respiration and Bronchophony; and these last soon ceased; when nothing was any where heard but the healthy respiratory murmur. The patient was well, and the whole work of reparation was accomplished in a week.

The diagnosis, I have said, was that she had one large vomica at least at the apex of the right lung, and that acute inflammation had arisen around it. I apprehended that she would die quickly, so fearfully rapid was her sinking in the two first days after she reached the hospital. But a week from that time she was well, and a fortnight from that time she was discharged from the hospital.

In this case I was entirely deceived in regard to one supposed ingredient of the disease, the Cavity. And I was entirely deceived *by Auscultation*. Cavity there was none, which is the very thing we expect to find at the apex of the lung. But Inflammation there was, and of the acutest kind, and nothing but Inflammation, which is the very thing we *do not* expect to find there.

It is quite certain that the Cavernous Breathing and Pectoriloquy were formed in a bronchial tube passing through a portion of lung which had become consolidated by inflammation. As the lung began to be more permeable, the Cavernous Breathing and Pectoriloquy lost their distinctness, and were changed into Bronchial Respiration and Bronchophony; and when the lung became entirely free, the healthy murmur was re-established alone and unmixed with any unnatural sound whatever.

Let it, however, in justice be remarked, that although Auscultation misled me both in my diagnosis and prognosis, yet it betrayed me into no error of treatment. Nay, but for Auscultation, I might have treated the case less precisely and effectively. It pointed out to me the very part where the disease was, and told me that Inflammation was at least an ingredient of it; things which could only have been conjectured without the aid of Auscultation.

There are certain conditions of the Lungs now familiarly understood (or at least familiarly spoken of, as if they were understood) by medical men, which had been little investigated, and were little known,

before Auscultation directed attention to them; and yet, both pathologically and practically, they are of the highest import. Laennec seems to have been first led to make accurate inquiry into the nature of Bronchial Dilatations, and Dilatations of the Vesicular Structure of the Lungs, for the sake of verifying certain Auscultatory signs. And all who concern themselves with Auscultation, and seek in like manner to verify its signs by dissection, will soon feel their obligation to him for his elucidation of these subjects.

Bronchial Dilatation may take place in one or in several branches, or in almost every branch, throughout the lung; it is, however, most frequently met with in the upper lobe, and nearer its anterior surface.

A Bronchus may dilate, and still preserve its natural cylindrical form. That, which would not naturally receive more than a knitting-needle, becomes large enough to admit a crow-quill, or a goose-quill, or even a little finger. It seldom happens that the common bronchial trunk is sensibly dilated, while the branches become larger than the trunk from which they are given off.

But a Bronchus may dilate and not preserve its natural form. It may dilate so as to take the form of a cavity, having the same size and shape as a vomica; it is distinguished, however, from a vomica, by the structure of the Bronchus being traceable into it; its mucous membrane, its fibrous membrane, and sometimes vestiges of its cartilaginous rings.

Again, a Bronchus may so dilate as to form several cavities; that is, it may dilate and then contract itself again to its ordinary calibre, and dilate and contract again and again at several spaces in its course. Thus, upon dissection, the lungs have sometimes appeared to be beset with vomicæ or abscesses full of matter, which, upon examination, have turned out to be so many cavities formed from the dilatation of several bronchial ramifications.

When the Bronchi are numerously and extensively dilated, they so compress the intermediate pulmonary structure as to preclude the admission of air into its vesicles; and thus it becomes squeezed together and flaccid, exactly resembling lung which has suffered from pleuritic effusions.

There seems good reason to believe that the Bronchi become dilated by the long-continued residence and accumulation of morbid secretion within them; and that the bronchial trunks are less frequently dilated than their branches, because this morbid secretion is accustomed to linger within them for a shorter time, being more easily dislodged by forced expiration, *i. e.* by Cough.

I know no instance of dilatation of the Bronchi where it has not followed or accompanied some disease specially characterized by abundant Bronchial Secretion; such as protracted Hooping-cough, Chronic Bronchitis, or Catarrh.

Surely this little pathological sketch will at once shew what the Auscultatory sounds must necessarily be which accompany dilated Bronchi. If they be enlarged uniformly through their whole course, they must give rise to Bronchial Respiration and Bronchophony, and much more so, if they pass through compressed and impervious lung. If they be formed into cavities, they must occasion Cavernous Breathing and Pectoriloquy; and when those cavities contain fluid (as they generally do) they must produce Gurgling Respiration and Gurgling Cough.

But Bronchial Respiration and Bronchophony, Cavernous Respiration and Pectoriloquy, Gurgling Respiration and Gurgling Cough, have been dwelt upon and explained as the almost certain signs of condensed lung and pulmonary abscesses; of lung, condensed by the products of common Inflammation or of specific disease; of abscesses formed by Inflammation, or left after the evacuation of tubercular matter. And of such conditions they must still continue to be the *almost certain* signs. It is only when they are interpreted by the special circumstances of some particular case, that they can be construed into a different meaning, or serve to indicate Dilatation of the Bronchi.

But the circumstances are *seldom special enough* to turn aside the Auscultatory signs from their most common object, and to make them point to another which is of very rare occurrence. Hence I am persuaded that the most experienced and most skilful physicians generally fail to form a just diagnosis in cases of Dilatation of the Bronchi, and seldom fail to form a wrong one; the Auscultatory signs and every attendant circumstance conspiring to lead them into error.

Chronic Bronchitis is the most frequent cause and concomitant of Bronchial Dilatation.

Now Chronic Bronchitis itself is often with great difficulty distinguished from Pulmonary Consumption. Its attendant emaciation, its copious puriform sputa, its abiding hectic, are all phthisical symptoms. And it is only after repeated examinations that we are able to exclude the idea of Phthisis when we find no Auscultatory signs of a cavity.

There is large Crepitation extensively diffused through the whole lungs, and that only; shewing that the copious puriform sputa come from the mucous lining of the Bronchi, and from it only.

But, suppose that in such a case there was Pectoriloquy withal, and Pectoriloquy in several places, or Gurgling Respiration and Gurgling Cough in several places, no human penetration could distinguish that disease from Pulmonary Consumption; and yet there might be neither tubercle nor vomica in any part of the lungs, but a dilatation of the Bronchi into the form of cavities.

A man, 46 years of age, had been liable to Catarrhs for several years, and for one year had suffered a slight habitual oppression of the chest. He had once spit blood in December of the preceding year. In the following February, upon the occasion of his contracting a fresh cold, an expectoration came on, which was copious and puriform, and very fœtid. Finally, a week before his admission into the hospital, he suffered a severe pain in the left side of the chest, which came on for the first time after his being wet through, and ever afterwards he was constrained to keep his bed.

He entered the hospital at the end of March. He was in a state of Orthopnœa, and his countenance expressed the greatest anxiety. He rejected, by an easy cough, a fœtid expectoration, consisting of yellow thick globules, mixed with a large quantity of serum, upon which they floated. The pain was so great over the whole of the *left* side of the chest, as not to allow the use of Percussion.

Auscultation found the Respiratory Murmur every where strong and clear on the right side, and much more feeble every where on the left; and on the left side, in the region of the mamma, and a little above the inferior angle of the scapula, there was a manifest Pectoriloquy.

Nobody entertained the slightest doubt in this case that the disease was Pulmonary Consumption, and that a vomica was formed. The patient remained in the hospital until his death,—nearly three months. In the meantime the pain of the left side, the fœtid expectoration, and the Pectoriloquy, all remained, to which, at length, Diarrhœa was added. It was remarkable that every evening he suffered a chilliness, followed by burning heat, but without perspiration.

Upon dissection, the left lung hardly crepitated at all, although it floated in water. In its upper lobe was a cavity large enough to admit a middle-sized nut, which contained a fluid of the same kind with that which was expectorated. A bronchial tube, as large as a goose-quill, entered into it; and dissection traced a continuation between the walls of the Bronchus and the walls of the cavity: in both the same mucous membrane red and thickened, the same fibrous membrane, and some traces of cartilaginous rings.

Here was no vomica, but nothing more than a considerable dilatation of a Bronchial tube. In the same lung several other Bronchial ramifications were dilated in the same way; suddenly they acquired three or four times their natural size, then contracted themselves again, and then enlarged again, thus in effect forming cavities. The pulmonary tissue between the dilated Bronchi was compressed as by pleuritic effusion *.

This case did not occur at St. Bartholomew's, but at La Charité; I was not the person deceived, but Andral: the possibility of deception, therefore, you will the more easily conceive. I have abridged the case from the "Clinique Médicale."

I am very far from saying that a just diagnosis of Bronchial Dilatation cannot be made during the life of the patient, or that Auscultation cannot contribute essential aid towards it; only I am persuaded that the physician must have very favourable opportunities of watching his patient, and that the case must be less complicated than such cases usually are, before he can arrive at it. The case, too, must be one in which the Bronchi are enlarged, still preserving their natural form, and not dilated into cavities.

But Laennec was led also by Auscultation to make inquiry into the nature of dilatation of the pulmonary vesicles; and this subject, as well as that of Bronchial Dilatation, he has made his own by the accuracy of his research.

The surface of the lung will sometimes present to the naked eye the same appearance which is given to it by an ordinary magnifying glass. The pulmonary vesicles will appear of the size of millet-seeds, or hemp-seeds, or raisin-stones.

The vesicles thus dilated sometimes preserve the level of the lungs, and sometimes transgress it a little.

This appearance results in part from the dilatation of single vesicles, and in part from the union of several, produced by the rupture of their immediate partitions.

Sometimes a transparent vesicle, as large as a nut-kernel, will rise very much above the surface of the lung, and seem to spring from a pedicle, or stalk. But this appearance is merely owing to its simple constriction just at the point of its emerging above the level of the surface.

Where the air-cells appear thus dilated at the surface of the lungs, the same condition is found to exist within. In order to see this condition in the lungs to the

* Andral, Clinique Medicale; Mal. de Poitrine, vol. i. 24.

best advantage, you should inflate them, and then let them dry. Afterwards, when you have divided them with a sharp knife, you will find, by examining the cut surface, that the air-cells are almost always more dilated within than they appear to be on the surface; and you will see, moreover, that of the air-cells some are simply dilated, and some are ruptured and united together. The smaller Bronchi sometimes partake of the dilatation of the air-cells to which they lead: but this event, which one would think likely to happen constantly, does in fact happen very seldom.

In a Lung, of which the air cells are dilated, there is something very peculiar to the touch. It has the feel of a downy cushion, not the crepitant feel of healthy Lung. It is softer than healthy Lung, and the same degree of pressure evidently displaces in it a larger quantity of air at once.

Now all this description belongs to conditions in which the air still remains within its proper vessels, those vessels being permanently and unnaturally distended; or distended and ruptured, moreover, into each other.

Hitherto I have abstained from using any name but what was necessary to the description. I have used none but *Dilatation of the air-cells or vesicles.* But this same Dilatation of the air-cells, in which the air *is still contained within its proper vessels*, is called by everybody Emphysema; and Emphysema let it be called; only take care that the misnomer does not convey an erroneous idea. I have no fancy for disputing about names; but this I would remark, that you might just as well call an Aneurism a Hæmorrhage, as a simple Dilatation of the air-cells, or the Rupture of the air-cells into each other, an Emphysema.

If an Aneurism bursts, then follows a Hæmorrhage; and if an air-cell bursts, then follows an Emphysema. And, in fact, distended air-cells often do burst, not into each other, but so as to allow the air to escape into surrounding texture.

One form of this Emphysema proper (as it may be called) is, when an air-cell bursts near the surface of the lungs, and air is effused beneath the pleura pulmonalis. Thus a vesicle of any size may be formed, from the egg of a sparrow to the egg of an ostrich, or even larger still. The air, now extravasated beneath the pleura, is capable of being displaced by pressure.

Another form of Emphysema proper, is described by Laennec; which I never saw. It is occasioned by rupture of distended air-cells; not on the surface whence air escapes beneath the pleura, but in the interior of the lung, whence air escapes into the pulmonary tissue and lacerates it, and forms a cavity in it.

The cavity thus formed is capable of receiving a moderate-sized nut. It is generally found about an inch deep beneath the surface of the lung. It is permanently blown up with air, and sometimes contains blood. The air-cells which immediately form the walls of the cavity are effaced, and do not retain their natural rounded form, either to the eye or the magnifier. But the air-cells at a little distance all around are still distended with air. The air, under these circumstances, is not necessarily effused into the interlobular structure; although it has escaped from its own vessels, it is still limited to this cavity.

There is one very curious circumstance attending this form of Emphysema. It is this: the pulmonary cavity, which is about an inch from the surface, being permanently blown up with air, exercises a great stress upon surrounding parts; under this stress they yield the more readily in that direction where there is the least resistance—viz. in the direction of the surface; and *there* a bump is forced up, correspondent in size to the interior cavity, and just over the part where it is situated.

The Dilatation of the Air-Cells has been explained to depend upon the forcible incarceration of air within them; and a cause capable of producing that incarceration has been found in Inflammation of the smaller Bronchial Ramifications which conduct them.

There is an affection called by the French "*Catarrhe Sec.*" It is characterized by habitual cough, or cough going and coming for years, and accompanied by little or no expectoration. But what expectoration there is consists of little pieces of hard, tough, pearly phlegm.

Upon this chronic affection attacks of a more acute kind are liable to be engrafted from time to time, accompanied by fever, and producing an increase of the expectoration; and when they subside, often leaving the habitual disorder worse.

In short it is one form of (what is called) Asthma; and the essential morbid conditions in which it consists are a congestion and thickening of the mucous lining in the small ramifications of the bronchi, and a secretion by it of this little glutinous pearly phlegm.

Now the congestion and thickening of the mucous membrane in that situation, and the residence of tough phlegm upon it, may be obstacles sufficient to prevent the easy return of air *from* the vesicles, while they may *not* be sufficient to prevent the access of air *to* them. The force of inspiration is evidently far greater than the force of

expiration; and the former is capable, moreover, of being augmented by an effort of the will, in a much greater degree than the latter.

It is not difficult to conceive how, more air being thus forced into the air-cells by each *inspiration* than each expiration can expel from them, there would result a constant imprisonment of air within them, and their consequent dilatation and their possible rupture.

I am, however, surprised that such congestion and thickening, and morbid secretion of the mucous membrane in this particular situation, should be insisted upon by Laennec as almost the sole cause of dilatation of the air-cells; for I suspect that all the ordinary mechanical impediments to breathing, whether within or without the lungs, have the common effect of raising hindrances and obstacles, *especially to the act of expiration*, and thus become capable of dilating the vesicles. It has occurred to me to witness their dilatation to the greatest extent, and their rupture and an extravasation of air beneath the pleura to the greatest degree, in cases of excessive deformity of the chest, arising from curved spine. I have found the same, of less degree and extent, in combination with tubercles and vomicæ; and I have found dilated vesicles, or ruptured vesicles, in clusters, at the edges of the lungs, where there has been no concomitant disease of the lungs to account for them.

And now for the auscultatory symptoms of Dilatation of the Air-cells, or of their Dilatation and Rupture together, as far as they have hitherto been described. These symptoms are of a very remarkable kind: they are derived both from Percussion and from Auscultation, but from neither singly. The positive intimations of the one are now in direct opposition to those of the other; yet do they point to the same thing and illustrate it *especially by their contrast*. Percussion gives a sound which is loud and clear; while to the ear, or the stethoscope applied to the same parts, either all is dull, and there is no respiratory murmur at all, or one rather suspected than distinctly audible. The chest does not give the same answer when you knock as when you listen.

Now it is not the clear sound upon Percussion that indicates Dilatation of the air-cells, or Emphysema; it is not the little sound, or the no sound, to the ear and the stethoscope, that indicates it. Either of them taken alone would denote something else; but *simultaneously*, they denote Dilatation of the Air-cells, or Emphysema.

There is nothing more interesting in the whole subject of Auscultation, than the various ways in which Percussion and Auscultation aid each other. They aid each other by their correspondent results, and they aid each other by their contrasted results. Each is thus made to go as far again by the help of the other, as it could possibly go alone.

Hitherto, in the course of our inquiry, I have spoken of Percussion occasionally only, introducing it to confirm the signs derived from Auscultation by its correspondent intimations. In inflammatory and tubercular diseases of the lungs, what Auscultation found pervious, Percussion found resonant; what Auscultation found condensed, Percussion found dull.

But now I introduce Percussion, not to confirm, by *its correspondent* intimations, the signs derived from Auscultation, but to rectify, by *its contradictory* evidences, what, if taken upon the sole attestation of Auscultation, would be false.

It is necessary for the sake of obtaining the full use of both these methods of appealing to the same sense in diseases of the chest, to understand them in their *disagreements*; to know *why* what one finds resonant, the other should find dull.

Percussion, by the resonance or non-resonance that attends it, *simply* intimates that air is or is not contained within the chest beneath that part of it upon which the Percussion is made. It intimates so much, and no more, with certainty. It gives no notice respecting any condition in which the air exists, or respecting any situation which it does or does not occupy. Whether it be moving about or at rest, there is the same resonance upon Percussion; whether it be contained in the bronchi, or in the air-cells, or in the cavity of pleura, there is still the same resonance, or so nearly the same, that no one would venture upon a diagnosis of its situation *merely* from a difference of sound elicited by striking the chest.

But Auscultation gives no intimations *absolutely* concerning the existence or non-existence of air within the chest. The air must be there under certain conditions for Auscultation to be able to detect it at all; for Auscultation to detect it, the air must be in motion. If it be at rest, Percussion can detect it, but Auscultation cannot. It must also be within the respiratory passages, or in situations with which they freely communicate; if it be beyond them, Percussion can detect it, but Auscultation cannot.

It is from air in these situations and under these conditions; from air in the respiratory passages, and from air put in motion by the act of breathing, that Auscultation conveys to us all the sounds diagnostic of so many varieties of

pulmonary disease. For, though air be still in the respiratory passages, if it be imprisoned there, and no motion reach it from the act of breathing, as in the case of dilated air-cells; or if it escape from the respiratory passages, as in the case of ruptured air-cells and emphysema beneath the pleura; then Auscultation can convey to the ear no sound, and is useless for the purposes of diagnosis.

But in this case Percussion still produces a resonance, and tells us (what is the fact) that there *is* air; yet Percussion does not tell under what circumstances or in what situation it is.

By comparison, however, and contrast of the results drawn from both methods, we arrive at conclusions in this case, to which neither could carry us separately. The chest is *resonant* to Percussion in every part, a sure evidence that in every part there is air!—but at the same time the chest is in *several* parts dull to Auscultation. Yet here is no contradiction to the fact that air is still in these parts: but upon comparison, it is a sure diagnostic sign that, being there, it is beyond reach of the respiration to give it motion; in short, that it is either imprisoned within the air-cells, or extravasated beyond them; and that we have to deal with a case of Dilatation of the Air-cells, or a case of Emphysema, or a case of Pneumothorax, of which I shall hereafter speak.

These, then, being the Auscultatory signs of dilated air-cells and ruptured air-cells, and of air extravasated out of its proper vessels, or (in one word) these being the signs of Emphysema, it would seem to be of easy detection. And existing *alone* and unmixed with other morbid conditions, it unquestionably must be so; but, in point of fact, it seldom, perhaps never, does exist alone.

There are certain conditions of parts frequently met with, which can hardly with propriety be called morbid. Although they are departures from what is natural and healthy, they exhibit no progressive morbid action. The blood-vessels are laying nothing down, and the absorbents are taking nothing up. There is only a yielding of parts to accommodate themselves to some pressing necessity, and a consequent change of natural capacity and size. The common biliary duct, from the passage of a gall-stone, the ureter from the passage of a renal calculus, dilates at the time, and remains dilated ever afterwards. So too the bowel, above the seat of a stricture, by gradually giving way to the pressure of its contents, will become permanently enlarged, and take the form of a pouch. These several states are all incidental to diseases, but are not diseases themselves.

The like conditions are exemplified, in the lungs, by dilatation of the bronchi, and dilatation and rupture of the air-cells, which grow out of preceding diseases, but hardly bear the character of diseases themselves.

But, although Emphysema may not itself come up to one's notion of what is understood by real disease, yet do real diseases constantly compass it round on every side. Diseases conduct to it, and diseases arise from it; and those that go before, and those that follow after, all remain and exist concurrently with it: so that I do not know any instances of more complicated thoracic affection than that of which Emphysema may, and generally does, form a part. There may be distortion or disease of the Dorsal Vertebræ; Chronic Bronchial Inflammation; Tubercular Depositions, or Vomicæ; any or all of these together, may have been concerned in imprisoning air within the vesicles, and so causing their dilatation or rupture; and having caused it, they still remain to augment it. Then there is the Emphysema itself; and, super-added to these, and arising out of one or all of them, may be dilatation of the right cavities of the heart.

Imagine what must be the complexity of symptoms from such complexity of disease! To omit others, think what the Auscultatory signs must be! For the Tubercular Cavities, the Bronchial Inflammation, the Dilated Heart, and the Emphysema itself, each have their own. And I do not say that you might not, from among all the rest, pick out the Auscultatory signs, which denote the existence of Emphysema. But if you did, it would be a great triumph of diagnosis.

There is (what is called) Interlobular Emphysema, in contradistinction to Pulmonary Emphysema, whose forms we have been considering.

Interlobular Emphysema is an extravasation of air into the cellular substance which intersects and separates the Pulmonary Lobules. This substance in its natural state is of so close a texture, that the infiltration of air into it could not have been thought possible. Yet the fact is certain. And then its real *cellular* texture becomes expanded and displayed. Thus lobule becomes separate from lobule, a space of half an inch or an inch being sometimes left between them, which is occupied by air that fills the intervening cellular tissue.

This tissue is more abundant and more cellular, and contains more air, as it is nearer the surface of the lung, and goes on

diminishing and containing less air as it penetrates deeper. Thus it is a good deal like the natural segments of an orange, which contains more juice just beneath the rind, and less as it approaches the centre.

From the manner in which the interlobular partitions run parallel to each other, it must be obvious that, when several are infiltrated with air at once, there will result a separation of various pulmonary lobules entirely from each other, like little islets.

When this interlobular Emphysema is near the root of the lungs, it soon reaches the mediastinum, whence air escapes into the cellular texture of the whole body.

Interlobular Emphysema, unlike the other forms described, has nothing to do with dilated air-cells: no dilated air-cells are found accompanying it, and where their rupture has taken place, cannot be traced. The pulmonary lobules, which are (as it were) blown apart from each other, and have air infiltrated all around them, are themselves in a healthy state.

The Emphysema from dilatation or rupture of the air-cells is a chronic affection resulting from causes which are tardy in their operation; but the Interlobular Emphysema takes place in a moment, and is the result of accident. Any violent effort which holds or intercepts the breath may cause it; the striving of parturition, the straining to unload the bowels, or to lift a heavy weight. I have myself seen it produced by the convulsive struggle of hooping cough; at least I presume so; for I have seen the subcutaneous cellular tissue about the neck of a child become blown up with air after a fit of coughing; but this happened before I had Auscultation to help me in inquiring the conditions of the lungs.

The Auscultatory signs of this Interlobular Emphysema are said to be such as cannot be mistaken, and strictly pathognomonic. They may be so; but I never had an opportunity of verifying them.

All this air effused must ensure a clear resonance upon Percussion. Then there are, besides, the "Frottement Ascendant" on Inspiration, and the "Frottement Descendant" on Expiration; and the "Large dry Crepitation." I wish I could determine the degree of certainty which belongs to these Auscultatory signs: but I cannot, having had no opportunity of investigating the circumstances of such rare cases.

There are many at this time who believe that the only way in which they can come at the knowledge of a theory is by destroying it, or taking it to pieces. This is exactly what children do with their playthings.—Hufeland.

IODINE, AND ITS POWER OF PRODUCING SALIVATION.

To the Editor of the Medical Gazette.

Sir,

I must, with your permission, once more trespass upon your columns, in order to reply to Mr. Horne's note. A more minute statement of the case referred to in my former communication, might have spared your pages both reply and rejoinder.

The patient in whom I observed salivation during the administration of iodine, had, I stated, taken that remedy for *five weeks* before the increased secretion appeared. The mercurial treatment had been relinquished *four months* before the commencement of this plan. During the interval no mercury had been given, the purgatives selected having always been either aloetic or saline. These particulars will, I imagine, be conclusive in proving that no substance previously given could have had a share in producing the effect.

I did not pretend to originality in my communication, merely wishing to corroborate Mr. Winslow's statement by the relation of a similar occurrence. In Johnson's journal for the current month, another instance is recorded to have been presented in the practice of Dr. Mackall, of Maryland; nor does the editor treat the fact as unprecedented. In your own journal may also be found papers, by Dr. Clendinning, in which the hydriodate of potash is stated in a few instances to have occasioned *severe* ptyalism, with affection of the gums, teeth, and breath. Dr. Mackall wished to account for what he saw, by supposing the iodine to have aided the *absorption* of mercury, which in his case had *immediately* preceded the remedy in question. This opinion, however, appears scarcely tenable, when we remember that it is the absorption of *living* molecules, not of other *medicines*, which is excited by iodine. If, then, calomel remained in the bowels, iodine could hardly promote its reception into the circulation; if already in the circulation, as seems more probable, then such assistance could not have been required. But Mr. Horne thinks, that though iodine cannot cause salivation *vi propria*, it yet does so by proving an "excitant to excite that action of the salivary glands which had not become previously developed." This is conceding the very point in question—I ask no more.

As to Mr. Horne's scepticism upon the power of minute doses of *calomel* to produce ptyalism, I presume he here stands quite alone. I do not, however, intend to

make any comments upon this part of his letter, being anxious to intrude upon your patience only so far as is necessary for replying to the question that he has addressed to myself.—I am, sir,

Yours respectfully,

GEORGE E. ELY.

Chatham, Jan. 5, 1836.

MORE SPECIMENS OF PHRENOLOGY.

THE heads of Lacenaire and Avril, the murderers who were lately guillotined, have been submitted to our examination. The forehead of Avril is very low, the facial angle acute, and those craniological developments are observed which indicate the thief and the assassin. But if in this instance the phrenologists may boast the truth of their system, in the head of Lacenaire they met with a stumbling-block not easily to be surmounted. Lacenaire, whose cold-blooded cruelty and want of feeling, under the most frightful circumstances, has astonished and disgusted all France, was phrenologically endowed with all the qualities of a good, kind, mild, sensible, and religious man, holding injustice and robbery in horror, and a hundred thousand leagues from being an assassin. This would be what we should be taught by phrenology, if we had not facts to correct the conclusions it would lead us to. Thus there is a marked development of all the interior and superior parts of the cranium, and as remarkable a smoothness of the two sides, and particularly in those parts which are said to correspond with robbery and murder. The organs of goodness and theosophy are remarkably prominent.—*Gazette Médicale.*

M. Leuret gives an account, in the *Gazette Médicale*, of a celebrated phrenologist of Paris, who, not long since, made a sad blunder. He selected a man-servant of an admirable *conformation*. Destructiveness, secretiveness, acquisitiveness—none; benevolence, conscientiousness—perfect; conscientiousness in particular was so well developed, that no body ever reckoned after this honest man; he was a model—as faithful as "man Friday." The phrenologist's wife, however, who wanted *faith*, soon found out our model of a servant to be an arrant rogue, and discharged him forthwith: upon which the husband, utterly unable to comprehend how these things could be, rubbed his eyes and was much confounded. But at last he discovered the cause. There must have been *transposition* of the man's *organs!* It was secretiveness that had usurped the place of conscientiousness—just as the heart is sometimes found on the right side, and the liver in the situation of the stomach. Good!

COLLEGE OF PHYSICIANS.

THE evening meetings will not commence this season quite so early as on former occasions. The first, we understand, will take place on Monday, the 22d February,—when Sir Henry Halford will read a paper.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

January 21, 1836.

George Brain List, Isle of Wight.
William Rose, High Wycombe.
Charles Malleson.
William Collins Engledue, Portsmouth.
Golding Bird, London.
William Petty Ruddock, Leeds.
William Foster Duck, Stocksley.
Eusebius Rouse, Great Torrington.
Henry Gibson, Hull.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Jan.* 19, 1836.

Abscess . .	1	Inflammation .	42
Age and Debility .	89	Bowels & Stomach	7
Apoplexy . .	9	Brain . .	5
Asthma . .	46	Lungs and Pleura	7
Cancer . .	4	Insanity . .	5
Childbirth . .	2	Liver, diseased .	2
Consumption .	73	Measles . .	7
Convulsions .	36	Mortification .	2
Croup . . .	4	Paralysis . .	3
Dentition or Teething	2	Rheumatism .	1
Diarrhœa . .	3	Scrofula . .	1
Dropsy . .	16	Small-pox . .	28
Dropsy on the Brain	15	Sore Throat and Quinsey . .	2
Epilepsy . .	1	Stricture . .	1
Erysipelas . .	2	Unknown Causes	10
Fever . . .	7		
Fever, Scarlet .	9	Stillborn . .	11
Gout . . .	2		
Hooping Cough .	7		

Increase of Burials, as compared with the preceding week . . 68

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Jan. 1836.	THERMOMETER.		BAROMETER.	
Thursday . 14	from 33	to 49	29·84	to 29 68
Friday . . 15	39	40	29 41	29 58
Saturday . 16	27	39	29·99	30·14
Sunday . . 17	24	41	30·15	30·25
Monday . . 18	26	43	30·18	29 92
Tuesday . . 19	29	37	30 12	39·25
Wednesday 20	20	41	30·20	30·08

Prevailing wind, N.W. and N.E.

Except the 16th, 17th, and 18th, generally cloudy, with frequent rain.

Rain fallen, ·625 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

DR. MACFARLANE's communication in our next.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, JANUARY 30, 1836.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

By Jon. Pereira, Esq., F.L.S.

Lecture XVIII.

The order *Hemiptera* (ημι, half, and πτερον, wing), has received its name from the circumstance of some of the insects having elytra, one-half of which is coriaceous, or crustaceous; the other, or posterior half, membranous. But in a considerable number of hemipterous animals, the character just mentioned is wanting. To this order belongs the

Coccus Cacti.

History.—This insect was first brought into Europe in 1526, by the Spaniards, who called it *cochinilla*; a term which, according to Fee, is the diminutive of *cochon*, a pig.

Natural history.—One of the families of the order Hemiptera is denominated *Gallinsecta*, from the similarity of the females at one period of their existence to small galls. This family contains a single genus, *Coccus*, one species of which is the cochineal (*Coccus cacti.*)

The male cochineal insect is small, of a dark or bluish-red colour, and has a pair of snow white wings which cover the body longitudinally. At the posterior extremity of the body are two white caudal bristles, between which lie the genital organs.

The females are larger than the males, and without wings. Their bodies have the same dark or bluish-red colour, but covered with a whitish dust. The dorsal surface is convex; the abdominal, flat. They fix themselves firmly on the plant which serves them as a habitation, and never quit this spot: here they couple, and increase considerably in size. Each insect lays several thousand eggs, which proceed from the body through an aperture placed at the extremity of the abdomen, and pass under the belly, to be there hatched. Death then ensues; the body of the mother dries up; its two membranes become flat, and form a sort of shell or cocoon, in which the eggs are inclosed, and from whence the little cochineals soon proceed. The female only is of commercial value.

Cultivation.—The cochineal insect is a native of South America, and is domesticated and reared with the greatest care in Mexico. It feeds on a plant called the *Opuntia cochinillifera*, or the *Nopal* (fig. 87.)

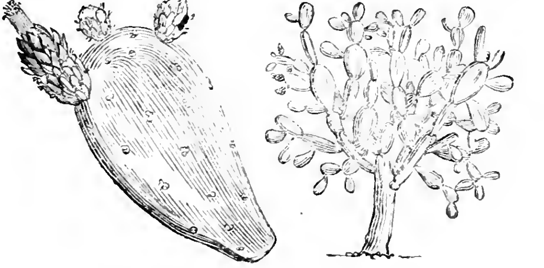


Fig. 87.—*Opuntia Cochinillifera.*

Plantations of these are cultivated for the nourishment of the insects. Here the impregnated females are placed; this operation being denominated *sowing them.* Young ones are soon developed; and some months afterwards, when the females have become fecundated and enlarged, the harvest commences. The insects are brushed off, and killed by immersing them in water, by exposure to the sun's rays, or in the vapour-baths of the Mexicans.

Three harvests are made annually: the first being best, since the impregnated females alone are taken; in the second the young females also are collected; and in the third both old and young ones, and skins, are collected indiscriminately. Before the rainy season commences, branches of the nopal plant, loaded with infant insects, are cut off and preserved in the houses of the Mexicans,; the animals would otherwise be destroyed by the weather.

Commerce.—From one district of Mexico alone (Oaxaca), the annual exportation of cochineal has been estimated at 500,000*l.* The annual importation into this country is calculated in some books at 150,000 lbs., worth 275,000*l.*; but this statement is overcharged. I find by reference to the Custom-house "Trade List," that for 1834 and 1835 the duties were paid for the following quantities:—

	lbs.
1834	100,292
1835	135,235

The value of this (in bond) varied from 6s. 9d. to 9s. per lb., to which 6d. per lb. is to be added for duty; so that if we call the average importation 120,000 lbs., at 10s. per lb., the value will be only 60,000*l.* On an average one pound of cochineal contains 70,000 dried insects, so that the number of these consumed annually in Great Britain is, at a moderate calculation, 8400,000,000!!

Physical properties and varieties. — Two kinds of cochineal are distinguished in commerce, the silver and the black.

1. *Silver cochineal.*—This has a purplish grey colour; but in all the furrows and depressions we observe a whitish powder, which, examined by the aid of a lens, appears like fine wool; it is considered the best kind, and is called by the Spaniards the *Cochinilla jaspeada.*

2. *Black cochineal* has a dark-brown colour, and is termed by the Spaniards *Cochinilla renegrida*, or *grana nigra.*

Both kinds are about one or two lines long, yield a dark-red powder, are inodorous, and have a bitterish warm taste, tinging the saliva violet-red. In burning they evolve an animal odour, and leave a greyish-white ash. By infusion in water they swell up, shew their ringed character, and even their feet, giving the liquid a red colour.

Purity.—The goodness of cochineal is determined by mere inspection. The silver variety is usually preferred, and hence it is sometimes imitated by shaking the black variety in bags with powdered talc. A magnifying glass will, however, readily distinguish the little plates of talc from the woolly character of the silver cochineal

Chemical properties.—Two analyses have been made of cochineal; one by John, the other by Pelletier and Caventou.

Analysis by Pelletier and Caventou.

Peculiar animal matter.
Fatty matter, composed of stearine and elaine.
Odorous acid.
Carmine.
Salts (phosphates of lime and potash, chloride of potassium, and carbonate of lime.)

The colouring matter called here *carmine*, is that which John denominated *cochenillin.* The following are some of its properties:—

It is a purplish-red substance, unalterable in the air, very soluble in water, soluble in alcohol, but insoluble in æther and oils (fixed and volatile). Its colour and solubility are variously affected by combination with different substances. Thus chlorine and iodine render it yellow. Acids do not precipitate it, but lighten its colour, and, in fact, make it yellowish. [The action of gallic, sulphuric, and nitric acids, on an infusion of cochineal, was here shown.] Alkalies and salifiable bases deepen its colour, and change it to violet. Some of its compounds with bases are soluble, others not. Its affinity for the hydrate of alumina is most powerful: if a solution of alum be mixed with an infusion of cochineal, and then potash or ammonia added, the alumina is thrown down in combination with the carmine, forming what is termed a *lake.* A number of salts act on it: thus the acetate of lead and protochloride of tin form precipitates with it, the first violet, the second red-coloured. Some salts, as the chloride of gold, change the colour of the solution, but do not cause a precipitate. Muriate of iron darkens the solution.

Physiological effects and uses.—Cochineal has been said to be diuretic, sudorific, antispasmodic, and anodyne, but without the least proof. A mixture of salt of tartar (potassæ subcarbonas, Ph. L.) and cochineal is a popular favourite in hooping-cough. The only real value of cochineal

is as a colouring matter, for which it is employed both in powder and solution.

HYMENOPTERA.

The order *Hymenoptera* has received its name from the membranous character of its four wings (υμην, membrane, and πτερον, wing); in this respect, however, it agrees with the order Neuroptera. But in the former the nervures of the wings divide and subdivide, like blood-vessels, while in the latter they form a kind of network, or lace. The only hymenopterous insect which I shall notice is the bee, or

Apis Mellifica.

History.—This animal was very anciently known, and is frequently referred to in the Old Testament. In all ages it has been an object of admiration and attention, on account of its industry, curious economy, and policy.

Natural history.—The honey-bee is an animal too well known to require much description; it lives in societies, which are called *swarms*, consisting of from fifteen to twenty, or even thirty, thousand individuals. In a state of nature they reside in hollow trees; but they are almost universally domesticated, and are preserved in *hives*.

They undergo a perfect metamorphosis: the *ovum*, or egg, is milk-white, semi-transparent, and attached to an angle of the *alveolus*, or cell. In about three days the *larva* is developed, and lies horizontally in its cell, being supplied with food by the working bees. In about five days it attains its full growth, when the cell is closed up with wax, and the insect spins its silky cocoon, which occupies it from twenty-four to thirty-six hours; and in about three days it becomes the *pupa*, in which state it continues for about seven days and a half (more or less, according to the sex), and is then metamorphosed into the *imago*, or perfect insect.

In this perfect form, the animals possess the following characters:—They have visible mandibles and palpi, with four unequal wings, the nervures of which branch variously; thus constituting the hymenopterous insect. The females and neuters are furnished with a retractile *aculeus*, or sting. The first joint of the tarsi of the posterior pair of feet is very large, compressed, quadrangular, and furnished at its internal face with silky down. By means of this little brush, these animals are enabled to collect the pollen of plants. The colour of the animal is blackish; the abdomen having a transverse greyish band, formed by the down at the base of the third and following segments.

Each society, or swarm, is composed of three classes of individuals — namely, a female, males, and neuters. 1st. The female, commonly called the *queen bee*, is narrower and longer than the others. 2dly. The males are termed *drones*; and in each hive there are from 800 to 1000: in size, they are intermediate between the female and the workers, and are not provided with stings, like the others. Towards autumn, when they can be of no further use, they are destroyed by the neuters. 3dly. The neuters are commonly termed *working bees*, and are by far the most numerous, since in each hive there are from fifteen to thirty thousand. Though termed neuters, they are in reality to be regarded as females, whose ovaries are not devoloped, in consequence, as some have supposed, of the nature of the aliment with which they are supplied while in the larva state.

On the *internal organization* of these animals I must be very brief. The *respiratory* system consists of ramified tracheæ, as in other insects. The *alimentary canal* is composed of an œsophagus (which enlarges at one part, forming the *crop*, or sucking stomach), a proper stomach, and small intestine, which terminates in the large intestine. The *biliary vessels* open immediately behind the stomach. The *salivary system* is very highly developed, the common canal of which opens into the proboscis. The *nervous system* is similar to that of other insects, consisting of a part analogous to the cerebro-spinal axis of the vertebrata, and a single and double sympathetic system. The *male genital* organs are two testicles, each having a vas deferens which terminates in a vesicula seminalis: from the conjoined extremities of the vesiculæ proceeds a common duct, terminating in the penis. The *female genital* organs consist of two ovaries made up of tubes, each tube containing about twelve eggs: the two oviducts from these ovaries terminate in a vagina, into which also opens a duct from a roundish vesicle. The *poison apparatus* is found only in the females and neuters: it consists of two thin convoluted secreting organs, opening into a pyriform receptacle, from which a small duct passes to the sting, which consists of two portions placed side by side, barbed at the extremity, and contained in a sheath. [Drawings illustrative of the anatomy of the bees were shewn.]

The effects produced by the sting of a bee are pain, redness, swelling, and hardness of the part. If a swarm attack an individual, the consequences might be fatal. Those who have tasted the poison, declare it to be hot and acrid. The best mode of treating these cases is as follows:—If the sting be left within the wound, immedi-

ately remove it; then rub the part with the saliva, or with oil and hartshorn: some recommend honey as a local application.

Bees furnish two products useful in medicine—namely, wax and honey. The first constitutes the sides of the cells or alveoli of the honey-comb, in which the eggs and honey are deposited; the latter (the honey) is to nourish the animals during the winter.

Honey.

Production.—Honey is secreted by the nectariferous glands of flowers, and is collected by the working or neuter bees, who take it up by suction, or lapping, and pass it into the dilatation of the œsophagus, denominated *crop*, *sucking stomach*, or *honey-bag*; beyond which, we presume, the honey does not pass, as it has never been found in the true stomach. When the animal arrives at the hive, the honey is disgorged by a kind of inverted peristaltic motion, and is probably somewhat altered in its properties by the secretions of the crop.

Physical properties.—Honey varies in its taste and odour according to the age of the bees, and the flowers on which they have fed. A hive which has never swarmed is considered to yield the best, which is, therefore, called *virgin* honey. The flavour of Narbonne honey, which is so much admired, is said to arise from the labiate flowers on which the animals feed; to imitate this, a sprig of rosemary is sometimes added to the honey obtained from other places.

Purity.—Flour, it is said, is now and then mixed with honey. It may be readily distinguised by its insolubility in cold water, and by the blue colour produced by the addition of iodine.

Chemical properties.—The constituents of honey vary somewhat according to the food of the bees, the season, the age of the animals, the mode of extracting it from the combs, &c. It must, however, be regarded at all times as a concentrated solution of *sugar*, mixed with *odorous, colouring*, *gummy*, and *waxy* matters. The saccharine matter is of two kinds: one crystallizable and analogous to the sugar of grapes; the other incrystallizable, and similar to the uncrystallizable brown syrup of the sugar-cane. Guibourt has found also mannite, which differs from sugar in not fermenting when mixed with water and yeast.

Physiological effects.—I have already more than once referred to the variable qualities of honey, arising from several circumstances, the most important of which is, the nature of the plants on which the bees feed. To this circumstance is owing the good quality and flavour of honey of certain districts: thus the environs of Narbonne and Mount Ida, in Crete, yield honey of the best quality, owing to the great abundance of labiate flowers. On some occasions, honey has been found to possess poisonous properties, arising (as is supposed) from the bees collecting it from poisonous plants. In Xenophon's account of the "retreat of the ten thousand" this celebrated general tells us, that some of his soldiers having eaten honey, in the neighbourhood of Trapezus (now called Trebizond), were seized with vomiting and purging, and became delirious, but that none died. Tournefort, who visited this part of the world more than 2000 years subsequently, ascribes these poisonous properties of the honey to the bees feeding on the *Azalea pontica*, which covers the mountains of this part of the world. Bees are said to gather poisonous honey from *Allium ursinum*, and, on the Alps, from *Aconitum lycoctonum*. In America, Augustus St. Hilaire found poisonous honey which had been collected by a species of wasp.

Disregarding these occasional and remarkable effects of honey, we may consider this substance as usually being, in its local operation, demulcent and emollient, while its constitutional influence is that of a nutrient. Its glutinous character renders it sheathing, or emulcent; but it has besides a relaxing or emollient action, shown by its diminishing irritation in the parts to which it is applied. Its saccharine and mucilaginous constituents, of course, render it nutritive. It acts mildly on the mucous lining of the alimentary tube, promoting its secretion, and having thereby a laxative effect. It is very apt to excite indigestion and colic; effects which are said to be more especially produced by fresh honey, and to be less easily induced after clarification. As these phenomena are not constant, they may arise, in some cases, from particular conditions of the honey; but frequently they arise from peculiarities on the part of the patient, which are either accidental or constitutional.

Uses.—Honey is frequently employed as a topical agent. Thus it is a popular application to promote the maturation of small abscesses and furunculi, for which purpose it is usually mixed with flour, and applied on linen or leather. It sometimes forms a constituent of gargles, partly on account of its taste, partly for its emollient operation; as a vehicle for the application of other more powerful agents to the mouth and throat, especially in children. As a demulcent and emollient, we sometimes employ it in internal diseases—as pneumonia and abdominal in-

flammation. I have sometimes found honey, mixed with barley water, to which a little of the juice and peel of the lemon are added, a very pleasant and useful demulcent, taken warm, to allay troublesome coughs.

Preparations (MELLITES).—Syrups, prepared with honey instead of common sugar, are denominated by the French writers *mellites.* They are semi-liquid, or viscous, and are usually made with clarified honey (*mel despumatum*)—that is, honey which has been melted and skimmed in a water bath. Mellites are of two kinds —simple and acid.

(*a*) *Simple mellites* are prepared by mixing powders or aqueous liquids with clarified honey: thus, the *mellite of borax* and *mellite of roses.*

(*b*) *Acid mellites* or *oxymels* are prepared by mixing acetous liquids with honey, as in *simple mellite* (or oxymel), and *mellite of squills.* Simple oxymel is sometimes employed in gargles,—sometimes as an expectorant.

Wax.

History and production.—Wax is an organic product, found both in vegetables and animals. In the former we meet with it either as a distinct layer, or in the form of a glaucous powder on the surface of the stem, leaves, or fruit. As an example of a *stem* secreting wax, I cannot refer you to a better example than the *Ceroxylon Andicola,* (fig. 88,) called also *Palma de Cera,* a tall palm of from 160 to 180 feet in height, which grows, as its name indicates, on the Andes.

FIG. 88.—*Ceroxylon Andicola.*

The stem (which is swollen in the middle) shows in its whole length rings, resulting from the fall of the leaves: the spaces between these are smooth, yellowish, and covered with a mixture of one part wax and two parts resin, which the inhabitants use for the manufacture of candles, &c. From the resin of this tree Bonastre has obtained a substance he terms *ceroxyline.* This resinous and waxy layer seems to occupy the place of an epidermis: the rings themselves being real cicatrices, are never covered with wax.

The South American tree called *Palo de vacca* (*Galactodendrum* of Kunth) yields a milky juice, which appears to be a waxy emulsion; so also the juice of *Asclepias gigantea.*

The *leaves* of many plants have a coating of wax, which in some cases gives them a glaucous, in others a varnished appearance. The substance called *chlorophylle* seems to be a kind of green wax, which is the source of the green colour of plants. Wax is also a constituent of the pollen of plants. On the surface of *fruits,* as the plum, it gives the appearance commonly denominated *bloom.* The fruit of *Myrica cerifera* yields nearly a quarter of its weight of wax: the berries are about the size of a pepper-corn. To obtain the wax, they are boiled in water, and compressed, by which it is squeezed out, and floats on the surface of the water, from which it may be skimmed off. In the United States of America, wax is thus prepared and employed for making candles. It differs from common bee's wax by its green colour.

In animals we find wax sometimes as an excretion, sometimes as a constituent of some of the internal parts. We are told that the larva of the *Cicada limbata* (fig. 89), or white wax insect of China, is covered with a waxy powder, which is communicated to the trees upon which these insects are found, and is collected by the natives, who hold it in high estimations a medicinal a substance. It is described in Donovan's "*Insects of China.*"

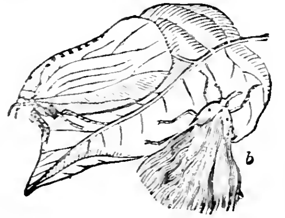


FIG. 89.—*Cicada limbata.*

Leopold Gmelin has found wax in the brain of man and of the ox.

Source of bees'-wax.—Bees-wax was at one time supposed to be merely the pollen of plants elaborated by bees. Bonnet, how-

ever, so early as 1768, asserted it to be a secretion from the ventral scales. Hunter and Huber have subsequently proved the correctness of this assertion. The latter writer, indeed, proved that the pollen is not at all essential to the production of wax, for bees fed on honey and water equally secreted it, and formed the usual waxy cells.

Preparation of yellow bees-wax.—The honey is extracted from the comb partly by allowing the latter to drip, partly by subjecting it to pressure. The comb is then melted in water, by which the impurities subside, and the wax is allowed to cool in moulds.

Properties of yellow wax.—Yellow wax has a remarkable and peculiar odour; its colour is more or less yellow, but varying in degree; its specific gravity varies from 0·960 to 0·965. It is said to be sometimes adulterated with suet, which gives it a fatty feel and disagreeable taste. Resin may be recognised by its solubility in cold alcohol; bean or pea meal, by its insolubility in oil of turpentine.

Wax bleaching.—To bleach yellow wax the following is the method adopted:—It is first melted (either in a copper vessel, or in a large vat or tub, by means of steam); while in the melted state it is drawn off through a stop-cock into a trough, called a *cradle*, perforated at the bottom with holes. This cradle is placed over a large water-tank, at one end of which is a cylinder, almost wholly immersed. The melted wax running through the holes in the cradle is allowed to fall on the top of the cylinder, which is kept constantly revolving on its axis, so that each stream of melted wax is solidified and converted into a kind of ribbon, and conveyed on the surface of the water to the other end of the tank. These ribbons of wax are here lifted out, and conveyed in baskets to the bleaching grounds, where they are placed on tables, and exposed to the air for one or two weeks (according to the state of the weather), being turned every day. The wax is then remelted, re-ribboned, and re-bleached; it is subsequently refined by melting in water acidulated with sulphuric acid.

Chlorine has been tried as a bleaching agent for wax, but it is said to render it harder and more brittle. I once tried this process, but could discover no alteration in the properties of the wax. Chevallier has employed animal charcoal; but the objection is the difficulty of separating the carbonaceous particles from the wax.

Properties of white wax.—Its colour is yellowish white; I have never met with pure wax perfectly white. The circular cakes of commerce, as well as wax candles, always contain spermaceti, which the dealers add to improve the colour. Pure wax is solid, brittle, inodorous, or nearly so, insipid, fusible, and at a much higher temperature decomposable. Its specific gravity varies from 0·8203 to 0·965.

Composition.—The ultimate constituents of wax are carbon, hydrogen, and oxygen.

	YELLOW WAX.	BLEACHED WAX.	
	Ure.	Gay-Lussac and Thénard.	Saussure.
Carbon ..	80·69	81·784	81·607
Hydrogen	11·37	12·672	13·859
Oxygen ..	7·94	5·544	4.534
	100·00	100·000	100·000

Wax is a fatty substance, and is composed of two principles, namely, *cerin* and *myricin*. The relation which these bear to each other, as well as to other fatty bodies, will perhaps be better understood by this table.

FATS
- saponifiable ..
  - by saponification yielding volatile fatty acids .... *Phocenine, Butyrin, Hircine.*
  - by saponification yielding fixed fatty acids
    - liquid at ordinary temperatures — *Elaine.*
    - solid at ordinary temperatures — *Stearine, Cetine, Cerin.*
- not saponifiable
  - but resulting from the saponification of other bodies .... *Æthal, Ceraine.*
  - not resulting from ditto .... *Ambreine, Myricine, Cholesterine.*

1. *Cerin.*—By reference to the preceding table, you observe Cerin is a solid saponifiable fat, furnishing by saponification fixed fatty (margaric, and perhaps oleic) acids. It is distinguished from Stearine and Cetin (with the latter of which it most closely agrees) by the products of its saponification.

Products of Saponification.

Of Cerin.	*Of Cetine.*	*Of Stearine.*
Margaric and Oleic Acids Ceraine.	Margaric & Oleic Acids. Extractiform Matter. Æthal.	Stearic Acid Glycerine.

Now you will observe that both cetine and cerin yield by saponification a peculiar fatty matter. That produced by cerin, and which has been called *Ceraine*, is distinguished from æthal, by its higher melting point (158°), and its less solubility in alcohol.

Cerin dissolves in 16 parts of hot alcohol, but is deposited as the liquid cools: in cold alcohol it is almost insoluble. It dissolves more readily in æther than in alcohol.

2. *Myricine* is distinguished from cerin by its incapability of forming a soap when mixed with alkalies. It is a white, fusible, solid, soluble in 123 parts of hot alcohol, and is deposited as the liquid cools.

The properties of cerin and myricine, however, require further examination, for Bucholz says myricine is softer and heavier than cerin, whereas John says it is harder and lighter. Some persons are of opinion these are not primitive constituents of wax, but formed in the process for procuring them.

Effects.—The local effects of wax are emollient and demulcent; the remote effects nutritive.

Uses.—It has been given internally, in the form of emulsion, in diarrhœa and dysentery, especially when ulceration of the alimentary tube is suspected. Hufeland and Wedekind, among the moderns, may be mentioned as persons who have used it in these cases. It has sometimes been employed as a masticatory, but its action is only mechanical.

Wax is, however, much more commonly used externally, sometimes as a mild sheathing or protecting application, sometimes as a basis for the application of other agents. It is a constituent of all cerates which take their name from it.

The vapour arising from wax placed on red-hot iron has been inhaled in phthisis.

Application.—The usual mode of exhibiting wax internally is by melting it, and forming an emulsion by means of soap, the yolks of eggs, or mucilage. Some recommend that it should be previously melted with some bland oil, as the oil of sweet almonds. The dose of it is from a scruple to two drachms.

We now arrive at the fourth and last division of animals denominated

Radiata, Zoophyta, or Cycloneura.

The animals of this division are called *Radiata*, from the form of their body. The term Zoophyta, sometimes used as synonymous with Radiata, is, however, only partially applicable. Dr. Grant calls this division the Cyclo-neura, from the particular form of the nervous system. The following are the classes which he admits.

	Classes.	Examples.
Radiata, or Cyclo-neura	1. Echinoderma	Asterias, or Starfish.
	2. Acalepha	Medusa.
	3. Polypiphera	Coral.
	4. Poriphera	Sponges.
	5. Polygastrica	Vibrio.

Polypiphera.

The polypipherous animals have received their name from the circumstance of their bearing tubes called *Polypes.* They consist of two parts, a skeleton, and a fleshy portion.

The *skeletons* vary in their consistence and nature, and also in their position relatively to the soft parts: thus, some are soft and flexible, others hard and calcareous. In the *Tubipora*, the skeleton is external, and forms a tube containing the fleshy portion of the animal: in the *Red Coral*, on the other hand, the skeleton is internal and solid, being cover-

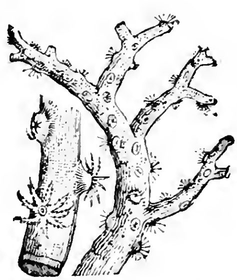


Fig. 90.—*Red Coral.*

ed externally by the soft flesh. In some of the polypiphera which have internal skeletons, the fleshy crust penetrates to some distance into the porous skeleton:—in these madrepores you have evident marks of this:—in red coral, on the contrary, you observe none of these calcarous cells.

The *fleshy* portion of the polypiphera may be external or internal. In either case there are developed from it fleshy tubes, each of which is surrounded at its external orifice with radiating filaments, called *tentaculæ*. The tubes called *Polypes*, are the organs by which these animals obtain their food, and are characteristic of the class.

Pharmacological Polypiphera.—The calcarous internal skeleton of several polipipherous animals, as the red coral (*Corallium rubrum* seu *Isis nobilis*) and the *Madrepora oculata*, were formerly employed in medicine. As they consist principally of the carbonate of lime, they offer no advantages over common chalk. The colouring matter of the red coral is peroxide of iron.

Poriphera.

The animals of this are distinguished from those of the preceding class by the absence of polypes. Dr. Grant has termed them poriferous animals, from the circumstance that on every part of their surface are found *pores*. As in the Polypiphera, we may divide the parts of which the Poriphera consist, into the skeleton and soft or fleshy parts. The *skeletons* consist of crystalline spicula (siliceous or calcareous) or of horny tubular filaments. The *fleshy* part consists of cellular tissue, which is so extremely soft, and the globules or cells of which have so little connexion with each other, that when the living animal is torn, the fleshy substance runs down from the lacerated parts like oil. On the surface of these animals we observe innumerable apertures or orifices; some are exceedingly minute, these are termed *pores;* others are considerably larger, and are termed *vents*, or *fæcal orifices*. The pores lead to canals which ramify throughout all parts of their texture, anastomose into larger and larger branches, and ultimately terminate in the fæcal orifices. It appears from the researches of Dr. Grant, that the water (containing the matters necessary for the existence of the animal) enters by the pores, circulates through the canals, and is expelled by the fæcal orifices, carrying along with it particles which separate from the sides of the canal.

Spongia officinalis.—This is the only poriferous animal employed in medicine. Its skeleton consists of horny tubular filaments, so disposed that they form a network. The fleshy substance is so soft, that when torn, it runs down like oil or the white of eggs.

This animal has been long known, though its nature was not perfectly understood; for a long time it was regarded as a vegetable substance. It grows in the sea, and is particularly abundant in the neighbourhood of the Greek islands.

Its principal constituent is a horny-like substance, which, according to Mr. Hatchett, consists of gelatine and coagulated albumen. Besides these animal matters, various salts are found in the ashes of sponge, such as the carbonate of lime and magnesia, phosphate of lime, iodide of sodium, &c.

Burnt sponge (*spongia usta*) has long been known and used in medicine. It has been employed in scrophula and bronchocele. Any benefit which may be derived from its use is referable to the iodine which it contains, and, therefore, it has now almost fallen into disuse. Burnt sponge lozenges, however, are still kept in the shops.

Burnt sponge should be prepared from unwashed sponge, for the washing removes a great part of the iodine on which the activity depends.

We have now finished the Animal Materia Medica, properly so called; but there are certain medicinal agents obtained by the decomposition of animal substances, which I think it necessary to notice,—namely, Hydrocyanic Acid, Ammonia, and Empyreumatic Oil.—In my next lecture I propose to examine the first of these.

REMARKS ON PARTIAL AMPUTATIONS OF THE FOOT AND HAND;

With Cases.

By John Macfarlane, M. D.,
Senior Surgeon to the Glasgow Royal Infirmary, &c. &c.

The propriety of attempting to save a part of the foot or hand, in cases of injury or disease, by having recourse to partial amputation, has been long known and acted upon, both in this country and on the Continent.

Garengeot, prior to the year 1720, when the first edition of his "Traité des Operations" was published, amputated a part of the foot for gangrene,

and recommends a similar procedure in every case to which it may be applicable. He also practised the removal of single metatarsal bones for caries, by a partial longitudinal, instead of a complete transverse section of the foot. M. Le Dran removed diseased metatarsal and metacarpal bones, with their corresponding toes and fingers.*

In the year 1636, Heurnius excised with success the cuboid and third cuneiform bones, shewing, at a still earlier period, that the principles which at present guide us in preserving sound portions of the foot and hand, were then understood and practised.† M. Moreau de Bar, in a case of diseased foot, excised the cuboid and third cuneiform bones, a part of the os calcis, and the posterior half of the fifth metatarsal bone.‡ B. Bell distinctly recommends amputation of the diseased parts alone, where all the metatarsal bones are not affected, even "should we be obliged to remove them all except one or two.§ Hey narrates a case of caries, in which he amputated the 3rd, 4th, and 5th toes, with their metatarsal bones, the greater part of the cuboid, and a small portion of the astragalus.‖ Sir Charles Bell describes the operation of excising metatarsal bones, and says "the careful and nice extraction of the spoiled bones from the hand or foot should be oftener done than it is."¶ Sir A. Cooper, in his valuable work on Dislocations, relates an interesting case of injury of the hand, where the fore-finger alone was preserved, which was afterwards found to be of great use to the patient.** Similar partial operations were also performed by Baron Larrey, in cases of gun-shot wounds. Guthrie recommends, in similar circumstances, the preservation, whenever it is practicable, of such of the metatarsal and metacarpal bones, with their toes and fingers, as may be uninjured.†† Hennen, whose authority is highly respectable, says, "cases will occur, where the hand or foot are only partially injured. By taking advantage of the joints and sound teguments, we may succeed in saving the limb*.

In the year 1823, Mr. Key, surgeon to Guy's Hospital, performed partial amputation in a very interesting case of injury of the foot. All the toes, with their metatarsal bones, except the great toe, were removed, along with the middle, external, and cuneiform bones, a sound flap having been obtained from the plantar surface of the foot.†

In this city, within my own observation, which extends to a period of more than 20 years, I know that when similar cases occurred, the practice has been adopted by almost every surgeon who officiated in our Infirmary, and by many private practitioners: and I have reason to believe that the importance of the treatment has been inculcated, and the different operations exhibited on the dead body, from year to year, to the pupils of our medical schools, by more than one of our surgical teachers. This statement will, I feel assured, be confirmed by the members of this society, many of whom have been connected with our Infirmary for years, and who, besides having had intimate acquaintance with the prevailing practice of the place, are so well qualified to estimate the correctness of these observations, and of others which are to follow. In fact, the propriety of the practice has been so long known, and so generally, I may say, universally, acted upon in this city, that many of the gentlemen who now hear me may deem these remarks and historical data superfluous. It does appear, however, in the authenticated notice lately published, of the last meeting of the British Association, in Dublin, as well as in some of the medical journals,‡ that Mr. Whatton, of the Manchester Infirmary, read a paper before the medical section, in which he seems to have claimed the credit of reviving, if not of originating, the operation of a partial longitudinal, instead of a complete transverse amputation of the foot; which assumption, it would appear, met with high approbation, and was received as a discovery so novel and important, that nothing less than a vote of thanks was proposed to, and actually bestowed upon, its pretended author.

* See the Operations in Surgery, translated by Gataker, 4th edition, 1778; also Observations in Surgery, translated by Jurgeon, 3rd edition, 1758.
† Velpeau, sur la Médecine Operatoire, tome I.
‡ Velpeau, ut citat.
§ System of Surgery, 7th edition, 1801.
‖ See his Surgery, 2nd edition, 1810, p. 55.
¶ Operative Surgery, vol. 1st, 1807. See also, Illustrations of the Great Operations of Surgery, London, 1821.
** Fourth edition, p. 462.
†† On Gun-shot wounds. London, 1815.

* Military Surgery, 1818, p. 258.
† Averill's Operative Surgery, 3rd edition, 1830, p. 227.
‡ Med Chir. Rev. for Oct. 1830, p. 493.

I entertain a higher opinion of the Dublin surgeons, (some of whom were the loudest applauders of Mr. Whatton's paper) than to believe that they are ignorant of so important a part of surgical history and practice. The approbation which they seem to have bestowed on the author, on the living and walking specimen of his success which he produced, and on his casts, which they begged he would permit them to deposit in their Museum, is, I think, not to be attributed to ignorance on their parts, but to the national enthusiasm which pervaded the *native savans* on that memorable occasion, and which seems to have led them to greet with an Irish welcome all who came in the name, and for the purposes of science. It does not become us, however, who are sitting coolly at a distance from, and who were not exposed to the fascinating influence of the late scientific and festive scenes in Dublin, to allow the generous compliments of our Irish brethren, as to the novelty and originality of Mr. Whatton's operations, to pass altogether unnoticed. It is on this account that I have presumed to bring the subject before this society, being anxious to vindicate the surgical character of this city, and to show to others, that the practice of partial longitudinal amputation of the foot and hand, recently claimed as a novelty, has been followed amongst us for many years.

Instead of collecting, which a little trouble would enable me to do, an account of all the cases in which this mode of operating has been employed by the different surgeons in this city, I shall content myself with a short detail, interspersed with practical observations of such cases only as have come under my own observation.

I. Of the Foot.

It is of the utmost importance in fractures or caries of the metatarsal bones, if we desire to preserve in whole or in part the function of the foot, to remove only the injured or diseased parts, and to retain such as are sound. When partial amputation is required, we have been advised not to disarticulate the metatarsal from the tarsal bones, especially in the centre of the foot, but if the injury or disease will admit of it, to saw them across anterior to the articulation*.

* Boyer, Traité des Maladies Chirurgic., tome xi., p. 200.

Hey never attempted to take out a single metatarsal bone from the centre of the foot, partly from a fear of not being able to secure the bleeding vessels in a deep and straightened wound, and when caries existed, from an uncertainty as to the extent of the disease. The former objection is too trivial to be noticed; as to the latter, doubts may exist regarding the number and extent of the bones implicated, although this may generally be determined by accurate examination; in injuries, however, if the parts are examined before much tumefaction or tension has taken place, the number and extent of the fractures may be readily ascertained.

Case II.—*Caries of the Second Metatarsal bone, cured by Excision.*

J. W., aged 18 years, of a scrofulous habit, had a small spongy ulcer over the dorsum of the second metatarsal bone of the right foot, near its middle, which had originated nine months before I saw him, and was attributed to injury. The bone was found to be extensively carious; but in consequence of the diffuse swelling, from the infiltrated and indurated state of the integuments, some difficulty was experienced in ascertaining the condition of the neighbouring bones. After careful examination, believing that these were sound, I proceeded to remove the disease by dividing the metatarsal space on each side of the affected bone, from the separation of the toes upwards, disarticulating at the tarsal joint. The edges of the wound were easly approximated and retained in contact by a few broad straps of plaster and a bandage. In about three weeks the wound was healed, and soon thereafter the patient could walk without impediment.

When two or three of the metatarsal bones require to be removed, ought we to approximate the parts, so as to fill up the vacant space, or allow them to remain separate? As regards the hand, the propriety of accurate coaptation, in similar circumstances, cannot be doubted; but I am not satisfied that we will not obtain a more useful foot by allowing the toes to remain in their natural position. If we bring them together, which can only be done by force, we must partially displace the remaining metatarsal bones, causing them to approach obliquely at their free extremities, while at their base they are far

separated. We thus cramp and confine the movements of the foot, according to the true Chinese fashion, and of course narrow the surface upon which the patient must rest in walking.

These remarks suggested themselves to me some years ago, on examining the foot of a mechanic who had had the second and third metatarsal bones and toes removed, and the parts brought together, by the late Dr. George Monteath. There was then, and the last time I saw him there continued to be, a good deal of lameness, which I thought might have been less had the wound been differently managed.

If the disease is confined to the metatarsal bone of the great toe, which is the one most frequently affected with caries, either complete or partial excision will be required. Attempts to preserve sound portions of this bone, with the toe, should always be made, when practicable; because a greater degree of permanent lameness will result from the loss of the first metatarsal bone than from any of the others. It is necessary, however, that we should examine accurately the extent of the disease, and be satisfied that in removing a part of the bone all the caries is included, or the operation will prove unsuccessful. If the disease is confined to the distal third of the bone, this part may be excised, and the toe allowed to remain, with a prospect of its being useful; but if the middle or proximal thirds are removed, the chances of success are still greater, because the toe in a great measure retains its natural support. When it is found necessary to remove the bone entire, the corresponding toe should be also amputated, to prevent the lameness which, when preserved, it generally produces, becoming displaced and inverted by the irregular and uncertain action of its muscles and its detached exterior tendon.

Case II.—*Caries of the middle part of the First Metatarsal Bone cured by partial Excision.*

M. A., a delicate scrofulous woman, 21 years of age, was admitted into the Royal Infirmary on the 12th of April, 1832. Seven months previously, a small indolent tumor formed over the inner edge of the first metatarsal bone of the right foot, near its middle, which suppurated and gave rise to a fistula. Through this opening the bone was found carious, and so soft that the probe readily passed into its substance. The disease appearing to be confined to the centre of the bone, it was determined to remove this part alone, its two articulating extremities being left *in situ*. This was easily done by making a free incision in the direction of the toe, dissecting back the soft parts, and dividing the bone with the forceps, after having ascertained that all the unsound parts were included. After some suppuration which continued about three weeks, the wound was perfectly closed, and in a few weeks longer the power of the foot was fairly restored.

In very young subjects it is generally necessary to excise the entire bone, even should only a part of its surface be exposed and carious; because in them the cancelli, in which the disease often originates, may be more extensively softened and loaded with the cheesy deposit of scrofula than we can possibly ascertain by external examination.

Case III.—*Caries of the First Metatarsal Bone cured by Amputation.*

G. N., æt. 9, was admitted into the Royal Infirmary on the 15th September, 1831. Behind the ball of the left great toe there was a puffy swelling, and in the centre of this was a small fistulous opening which led to diseased bone. Twelve months was the duration of the disease, and its orign was attributed to a slight injury.

On the 18th an exploratory incision was made along the inner edge of the metatarsal bone, which was found more extensively diseased than the introduction of the probe, or the discoloured and tumefied integuments, indicated. The toe, with the entire metatarsal bone, was therefore amputated, which was done by extending the former incision back to the tarso-metatarsal joint, and around the root of the toe, then dissecting back the integuments, and disarticulating from the os cuneiforme. The cure was complete in about a month, but nearly a year elapsed before the lameness was removed.

When the caries is confined to one or more of the tarsal bones, and an operation is deemed necessary, it becomes a question of importance how far we are justified in removing the affected parts alone, without their metatarsal bones and toes. I confess my experience on this point has not been such as to ena-

ble me to give a satisfactory opinion. I have only once removed, in a case of injury produced by a heavy body falling on the outer side of the foot, two of the tarsal bones, the metatarsal bones and toes being allowed to remain. In this case the cuboid and third cuneiform were comminuted, and the integuments covering them extensively lacerated. With difficulty all the fractured portions were cut out; and after severe inflammation of the foot and leg, with copious suppurations, the wound closed. I lost sight of this patient before the utility of the foot in walking could be ascertained.

If the disease is confined to the cuboid, or any other tarsal bone, situated at the outer or inner side of the foot, excision will be more easily performed than when the central bones are affected. These are so deeply seated, and firmly wedged together, as to render their removal extremely difficult and painful. Nevertheless this has been sometimes had recourse to with success; but I suspect we will rarely meet with cases where the disease is confined to one or two of the tarsal bones, and where we can ascertain this by external examination. This was more easily and satisfactorily done in the following case than in any I ever met with.

Case IV.—*Caries of the Cuboid Bone, and of the anterior articulating surface of the Os Calcis, amputated with the 4th and 5th toes.*

A. M., aged 19, admitted September 12th, 1835. On the dorsum of the right foot, over the cuboid bone, there was an ulcerated opening the size of a sixpence, of an indolent appearance, with a good deal of surrounding tumefaction. The probe detected caries of the cuboid bone, and of the anterior articulating surface of the os calcis, the other bones being apparently sound. The ulcer had exexisted for seven months; but several years before this she had sprained her foot, and was afterwards subject to pain and swelling of the part.

After a variety of local treatment, which was totally ineffectual, I received the sanction of a consultation to amputate the diseased parts, which I did on the 13th of October. I commenced the operation by making a transverse incision in the dorsum of the foot, from the outer edge of the third toe, at its metatarsal joint, to the distal end of the metatarsal bone of the little toe. A corresponding incision was made in the sole; and at the junction of these two another was carried along the outer edge of the foot to the os calcis. The dorsal and plantar flaps having been detached as far as the third toe, it was found necessary, in order more effectually to expose the diseased parts, to make a transverse incision in the upper flap, opposite the calcaneo-cuboid articulation. The bistoury was then carried backwards, between the third and fourth metatarsal bones, and around the cuboid, which was removed with the fourth and fifth metatarsal bones and toes. The anterior articulating surface of the os calcis being destitute of cartilage, and in a state of caries, was removed by the cutting pliers, as was also a small rough portion of the external cuneiforme. Only one vessel, apparently the external plantar, required a ligature. Three points of suture were introduced, and the usual dressings applied.

On the 16th the edges of the wound were closely adhering; but on the 18th, a few minutes before the hour of visit, arterial hæmorrhage suddenly took place to the amount of fifteen ounces, which, however, ceased spontaneously, and did not return. The wound gradually healed, and when she left the Infirmary, about a fortnight ago, her foot was sound and neat: she had complete command of the motions of the remaining toes; and I have no doubt that in a few weeks longer she will be able to walk with little or no impediment.

Were I to meet with a similar case, in which the extent of the disease could be so distinctly ascertained, I would attempt the removal of the affected tarsal bones alone, leaving the metatarsal bones and toes, with their tendons. I am not prepared to admit that the toes thus preserved, from being deprived of their posterior support and connexion with the arch of the foot, would be of very great use in walking; still it is possible that the vacant space might be filled up; and as the toes and metatarsal bones could not be far displaced from their natural position, a more useful foot might in this way be obtained.

When all the anterior tarsal bones are diseased, a complete transverse operation is required. This operation, which receives the name of its author, Chopart,

has of late years had various objections started against it; such as the liability of the flap to slough when taken from the sole of the foot, the falling back of the stump, and projection forwards of the astragalus, by the action of the muscles of the calf, thus producing lameness; and in a case alluded to by Velpeau, requiring the tendo-Achillis to be cut*, and the supervention of high local inflammation and its accompaniments, by which the ankle-joint is sometimes involved, and a second amputation required. I have seen this operation performed frequently with success, and useful limbs preserved, without any of these untoward occurrences happening; and in two cases to which I particularly refer, the patients, by wearing a boot of a particular construction, were enabled to walk easily and comfortably.

Case V.—*Disease of the anterior Tarsal Bones—Chopart's operation successful.*

J. M'N., aged 10, was admitted into the Royal Infirmary on the 2d April, 1835, and came under my care on the 1st of May following. Four months ago an abscess formed on the dorsum of right foot, which was punctured, and gave rise to an ulcer of considerable size, through which several of the tarsal, and the proximal ends of some of the metatarsal, bones were discovered to be carious. The whole dorsum of the foot was inflamed, swollen, and had a doughy feel.

After the use of incisions, and a variety of other means both local and constitutional, the disease appeared to gain ground; and on a careful examination, it was believed that all the bones forming the two anterior rows of the tarsus were affected. Accordingly, on the 26th of May, and with the sanction of a consultation, I amputated the foot at the articulations of the calcaneum and astragalus with the cuboid and scaphoid bones. I made a semilunar incision with a small Lisfranc's knife, across the dorsum of the foot, about an inch anterior to the point where disarticulation was to be performed. I then transfixed the sole and made a large flap, by cutting down to the separation of the toes, after which the ligaments and remaining soft parts were readily divided. All the tarsal bones removed were carious, softened, and loaded with scrofulous deposits.

This boy was dismissed cured in a few weeks. The flexor and extensor tendons appeared to be adhering to the cicatrix, for he was able to move the point of his stump, and there was no tendency to its displacement from the preponderating action of the gastrocnemii muscles.

Case VI.—*Compound comminuted Fracture of the 4th and 5th Metatarsal Bones and Toes, and of the Cuboid and external Cuneiforme—Amputation.*

W. G., a robust and healthy man, 34 years of age, when assisting to raise a puncheon of spirits by means of a crane, the chain broke, and the end of the puncheon fell from a considerable height on the outer edge of the left foot, producing a contused wound, which extended from the outer edge of the os calcis obliquely forwards to the root of the third toe, with extensive comminution of the fourth and fifth metatarsal bones, and of a small portion of the cuboid and external cuneiforme. The plantar integuments being uninjured, I procured a flap there, which, with a smaller one from the dorsum, was nearly sufficient to cover the wound. The fractured bones and two toes were removed, and the parts dressed in the usual manner.

He had a smart attack of subfascial inflammation, commencing in the back of the foot, and extending to the knee, with high febrile excitement. These were subdued by incisions, leeches, cold lotions, and the use of nauseating doses of emetic tartar; but nearly two months elapsed before the wound was healed. When he began to walk, and for months after, he could not tolerate pressure over the remaining portion of the os cuboides. This was obviated by padding the outer part of his shoe; and the last time I saw him he walked with ease, and without lameness.

II. Of the Hand.

The majority of the observations which I have made regarding partial amputations of the foot will apply to the hand. There are, however, some peculiarities which I shall shortly notice.

* Médecine Opérat. tome i. p. 474.

In this city we are more frequently called upon to perform partial amputation of the hand for injuries than for diseases of the bones, being the reverse of what happens in the foot. We are also, in attempting to preserve a useful hand, more anxious than in the foot, by the accurate coaptation of the parts, to avoid deformity. This leads us, in amputating the middle or ring fingers, to remove at the same time the phalangeal end of the metacarpal bone, to allow the adjoining fingers to come into closer and more seemly contact. For the same reason, in removing one or both of the central metacarpal bones, we attempt to bring the parts into contact, and promote adhesion, instead of allowing them to cicatrize separately. I shall shortly notice two cases to illustrate the propriety of this practice.

Case VII.—*Caries of the metacarpal bone of the ring finger cured by amputation**.

A. M'S. aged 23, admitted September 27th, 1831. A year and a half ago received a blow on the dorsum of the right hand, which produced an abscess over the metacarpal bone of the ring finger. This was punctured, and a fistula established, through which extensive caries of the bone was discovered. As the swelling and discoloration occupied nearly the whole dorsum of the hand, it was with some difficulty I succeeded in ascertaining that none of the other metacarpal bones were affected.

On the 30th the diseased bone was removed, along with the finger to which it was attached, by transfixing the metacarpal space on each side with a French bistoury, and cutting down from the carpal articulation to the first phalanx. The sides of the wound were retained in close contact by sutures and a bandage; adhesion took place, and the patient was dismissed with a neat and useful hand, on the 21st of October.

The metacarpal bone was found on examination to be both necrosed and carious; its proximal half consisted of a thin shell which was lined with granulations, and contained a loose cancellated sequestrum; its distal end was softened, and loaded with pus.

* See Clinical Reports, p. 270.

Case VIII.—*Caries of the mteacarpal bones of the middle and ring fingers, cured by excision.*

E. G., a young unhealthy-looking woman, of a strumous habit, was sent to me from the Highlands, to have her hand amputated. She had been wounded in the back of the hand with a fork, about eighteen months before, which gave rise to extensive inflammation, and deep suppuration, the matter having been allowed to burrow in the parts without any incisions being made. At the time I saw her there were three ulcerated openings over the 3d and 4th metacarpal bones, filled with pale-coloured spongy granulations, through which, on the introduction of a probe, the subjacent bones were found extensively carious. The soft parts on the dorsum were much swollen, and it was not until after repeated and very careful examinations that I could ascertain the limits of the disease. When this was done, I did not hesitate to advise the patient to submit to partial amputation. The operation, as in the last case, was performed without any attempt to preserve a dorsal or palmar flap; the only difference was, that I divided the metacarpal spaces by cutting upwards from the division of the fingers, taking care to avoid the extensor tendons of the fore and little fingers, which, from their position, are very apt to be divided on approaching the carpus. The metacarpal bones were readily disarticulated from the os magnum and os unciforme, and with a little force, the parts were brought into contact: adhesion took place, and a neat hand was obtained, but it was not till several months had elapsed, that she regained free and useful motion of the remaining fingers.

The removal of the injured or diseased metacarpal bones, without their corresponding fingers, has been recommended and practised by MM. Troccon, Roux, Blandin*, Velpeau, Wardrop†, &c. This practice, I believe, is not generally followed, because the fingers thus preserved are frequently productive of annoyance and deformity.

In compound fractures, or dislocations of the carpus, when the injury is confined to one or two of these bones, they

* Dictionnaire de Medecine et de Chirurgie, tome ii. p. 269.

† Medico-Chirurgical Transactions. Vol. iv. p. 309

may be excised, and a useful hand preserved, but when the injury is more extensive, primary or secondary amputation will generally be required. In one case, I removed the scaphoid bone, which was dislocated, and projecting through a lacerated wound of the integuments, the injury being produced by a pistol bursting in the hand. This patient recovered the perfect use of his hand after protracted and extensive suppuration. In the following case, the injury was still more severe.

CASE IX.—*Compound comminuted fracture of the index, ring, and middle fingers, of the 4th and 5th metacarpal bones, and of the Os Unciforme.—Partial amputation.*

W. Y., a healthy young man, 19 years of age. When shooting in the country, on the 1st of January, 1827, his gun, which was overloaded and insecure, burst in his hands. Five hours after the accident, when I first saw him, the following was the state of the injured parts. The little, ring, and index fingers of the right hand were dreadfully lacerated, and their phalanges comminuted; the 4th and 5th metacarpal bones were fractured in several places, and projecting through lacerated openings in the dorsum and palm. The bones and tendons of the thumb and middle finger were uninjured, as also appeared to be the wrist-joint, although there was a good deal of swelling around it.

I proceeded to amputate the index finger at its metacarpal articulation, and then the 4th and 5th metacarpal bones, with the fingers attached to them. In the latter part of the operation, after dissecting off as much sound integuments from the palm and dorsum as I could procure, to cover the wound, I passed the bistoury from before backwards along the metacarpal space between the middle and ring fingers, till it reached the carpus, when its edge was turned out, and the two fractured metacarpal bones separated from the carpus. I then ascertained, for the first time, that the os unciforme, upon which the 4th and 5th metacarpal bones rest, was fractured into three pi ces, and partially displaced. These were removed, and the integuments, which nearly covered the wound, were approximated, and light dressings and a bandage applied. General and local bleeding, incisions, &c. were found necessary to subdue the very high constitutional and local disturbance which ensued. For some time the wrist-joint seemed in danger, but after free suppuration, and sloughing of a portion of the integuments, the swelling subsided, and the parts slowly healed. About a year after the injury, he was able to follow his employment, which was that of a tailor.

It were easy to add from private sources, as well as from the records of our infirmary, many additional cases to those I have detailed, in which partial amputation was successfully employed. I have said enough however, I trust, to convince every unprejudiced person, that in this city, we have long known, and justly appreciated, this very important, though certainly not novel, part of surgical practice.*

Glasgow, Jan. 1836.

ANEURISM—LIGATURE OF THE EXTERNAL ILIAC ARTERY—CURE.

To the Editor of the Medical Gazette.

SIR,

I BEG to forward to you, for publication in your widely circulated journal, the following case of aneurism, where the external iliac artery was successfully tied.—I have the honour to be, sir,

Your obedient servant,

EDMUND FURNER,
House-Surgeon.

Sussex County Hospital,
Jan. 20, 1836.

Daniel Kinch, aged 24 years, footman in a gentleman's family, of a plethoric habit, and very muscular frame, but unusually fat for so young a man, was admitted into the hospital on the 29th of last April, under the care of Mr. Tayler, with a crural aneurism on the right side. He had enjoyed robust health till within seven months of his admission: about that time, whilst carrying a heavy weight, his foot slipped, which compelled him to use considerable muscular effort to prevent falling. He, however, continued his occupation as

* At a numerous meeting of the Glasgow Medical Society, on the 15th December last, when the preceding paper was read, all the members who joined in the discussion fully and distinctly corroborated the statements I have made, as to the prevalence of the practice of partial amputations in this city.

usual, without being aware of having suffered any injury. Some time after he observed a small swelling in the right groin, which not being painful did not excite further notice. About four months from the time of his first remarking the tumor in the groin, he consulted Mr. Tayler, in consequence of general swelling of the right thigh and leg, attended with slight cutaneous erysipelas, and for which he could assign no cause. This, by rest and the application of cold lotion, was so far subdued that the limb was nearly reduced to its natural size. Mr. Tayler's attention was then directed to the tumor in the groin; and having ascertained its character, advised his immediately coming into the hospital, in order to undergo an operation.

At the time of his admission into the hospital, the tumor was the size of a hen's egg, situated an inch below Poupart's ligament, in the course of the crural artery, possessing very active pulsation: when firmly pressed upon, it could be partially emptied of its contents, but regained its original magnitude upon the pressure being removed. The circumference of the upper part of the thigh was four inches greater than that of the sound one, the limb below being increased in proportion. The integuments were of the natural colour; it was unattended with pain or inconvenience in walking; his general health was good, the heart's action regular, and the patient was wholly unconscious of labouring under a disease of importance. He was ordered spare diet, with purging and bleeding at intervals, as a preparatory measure, for three weeks, when he was considered to be in a favourable state for operation.

The operation of tying the external iliac artery was accordingly performed, in the presence of Messrs. Blaker and Lawrence, the other surgeons of the hospital, Mr. Stanley, of St. Bartholomew's, and several other professional gentlemen.

The patient being placed in the usual position, an incision, four inches in length, was then directed, more in the course of Poupart's ligament than is usually recommended. This was found greatly to facilitate the after-steps of the operation, from the increased space afforded by the more transverse division of the abdominal parietes. The peritoneum and intestines being held back by an assistant, the ligature was passed from within outwards, the sheath having been previously opened by cautiously scratching with the ivory handle of the knife. The ligature was a single strong cord of unbleached silk. Upon tightening it, the pulsation immediately ceased. The wound was brought together with adhesive plaster, and the patient ordered an anodyne draught. He complained of numbness in the foot for several days, but there was no difference at any time in the temperature of the limbs indicated by the thermometer. It is unnecessary to give a daily report of the case. The ligature came away on the sixteenth day; from which time he progressively improved, and was discharged August the 12th. The patient remained in the hospital until he was sufficiently recovered to resume his usual occupation, the tumor having almost disappeared. I have lately heard of him; the report was, he had never been in better health; the right limb quite as strong as ever.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'Alembert.

St. Thomas's Hospital Reports. By John F. South, Assistant Surgeon. No. I. *Nov.* 1835.

Our notice of our friends at St. Thomas's was cut marvellously short the week before last, some extracts which we had marked for insertion having been omitted sheerly from lack of space. But this was not the worst of it, inasmuch as we were made to promise the insertion of such portions of the brochure as might be calculated "to *entertain* our readers." The *devil*, no doubt, had a hand in this unlucky phrase; and yet was there no malice in it; we therefore crave pardon of the learned writers for our involuntary offence. The truth is, that finding there was no room for aught else, we wrote a short note to say that we should, at a future time, lay before our readers some portions which we thought would interest them; but as to charging such grave and learned parties with being *entertaining*, it is what we never should have dreamt of.

Mr. Travers has communicated largely to the "Reports," as has Mr. Tyrrell; but the only paper for any part of which we can even now make room, is that by Mr. Green, *On the Treatment of Hydrocele by Setons.* We shall extract one case, and the most important inferences drawn by the author from his general experience.

The following case is selected, not as a particularly favourable one, but because, being the first, the mode of introducing the threads is described more particularly than in the others:—

Thomas Waterman, ætat. 40, weaver, admitted into Isaac's ward, October 25, 1832. A healthy man, of regular habits: is the subject of hydrocele on the right side, which commenced, without any assignable cause, about five or six years since, and has gradually increased in size, but unaccompanied with pain.

Nov. 2d, 1 P.M.—The operation was performed as follows:—A trocar and canula having been introduced, about eight ounces of fluid were drawn off, and during this time the man fainted. The canula still remaining in, a needle six inches in length and as thick as a probe, with a trocar point at one and an eye at the other end, was introduced, armed with twelve threads of ordinary seton silk, into the canula, and having been carried upwards, perforated the tunica vaginalis and integuments near the upper and fore part of the swelling, and was drawn out by that aperture. The canula was then removed, and the ends of the thread tied loosely together over a space of about two inches. After he recovered from the faintness he was sent to bed, complaining of great pain extending up the cord to the loins.

10 P.M.—The pain had become so severe, that the threads were removed, and he soon began to experience relief.

3d.—Has had a restless night; the pain still continues, but is not so severe. His bowels being costive, mist. senn. comp. was ordered.

4th.—Slept better last night, the pain having subsided during the course of the evening; bowels open; complains of thirst; pulse soft and quick; has no appetite. There is slight heat and redness of the scrotum.

5th.—The swelling of the scrotum increasing, but the heat much diminished.

6th.—The scrotum is now as large as it was prior to the operation.

9th.—Much the same.

26th.—Since the last report the swelling has somewhat increased in size, but in other respects he was quite well.

Dec. 7th.—Fluctuation being now apparent, though there is no transparency in the tumor, the hydrocele was tapped a second time, and about six ounces of very dark-coloured fluid evacuated; this high colour, together with the great thickening of its coverings, which has occurred since the last operation, has been probably the cause of the opacity of the swelling. Threads were then introduced as before, and the patient sent to bed.

8th.—Has slept four or five hours during the night. There is considerable redness, but not much heat, about the scrotum this morning; has pain extending along the cord when pressed; the lower part of the scrotum very tender, and he complains of pain in the loins. Pulse quick and soft; skin moist.

1 P.M.—The threads were withdrawn after *twenty-two* hours.

9th.—Passed a rather restless night. The swelling a little increased, accompanied with redness; much heat and pain upon pressure.

11th.—He has slept well; bowels open; pulse regular and quiet. The swelling much the same, but the redness rather less, and he feels himself easy.

Jan. 5th, 1833.—The swelling much diminished. Adhesion has taken place at the lower, though there is still some fluid at the upper part.

Feb. 5th.—Was discharged. For some time previous to his leaving the house, he rubbed ung. iodinæ on the scrotum, which certainly caused partial absorption of the remaining fluid.

* * * *

The foregoing cases are intended as illustrations of a plan of treatment, which, although not altogether novel, may, perhaps, be deemed an improved method of effecting the radical cure of a hydrocele.

The object of the radical cure is that of causing such a change in the tunica vaginalis as will prevent the re-accumulation or re-production of the fluid. I use the term "change" advisedly; for though it is generally stated that the object in a radical cure is to obliterate the cavity of the tunica vaginalis, by causing adhesion of the sides of that membrane, a preparation in the collection of this hospital exhibits a tunic, taken from a person in whom the radical cure was effected by injection, and in whom, after this operation, no fluid was re-produced, with the cavity as perfect as it might be in the healthiest person. Here the change must have been produced by some alteration of the surface; and I can very well conceive that a slight inflammatory action may take place, so as to close the exhalant arteries, and to prevent them

afterwards from pouring their secretion into the cavity, or, at any rate, so as to close a sufficient number of them to prevent any redundancy of the secretion. I very strongly suspect that in many instances of the radical cure of hydrocele no more has been done than you see in this preparation; and, indeed, I think you will agree with me, that if we could always hit the production of that quantity of inflammation which should produce this, and no more, it would be a better plan of treating the complaint than that of causing the obliteration of the cavity. But unfortunately, under all the plans of treatment hitherto adopted, the quantity of inflammation cannot be regulated; unless, indeed, in the cases above cited, a method is offered which may, in some measure, supply the defect, and aid us in adjusting the requisite degree of inflammatory action.

* * * *

Now, generalising these facts, the results of the above and other cases, I may venture, perhaps, to say that the plan of treatment is well adapted to answer the end for which it was intended. In two instances, indeed, the operation failed from the want of a sufficient degree of inflammation, but which simply depended upon the insufficient irritation of the seton threads.

In another case there was a slight suppuration of the cellular membrane of the scrotum, which, however, only interfered with the rapidity of the cure, but was in no other way detrimental to the patient. In another case, however, there was excessive inflammation and a suppurative process in the tunica vaginalis; and the possibility or probability of this occurrence is perhaps the most serious objection to the operation proposed which may be gathered from these cases. It might, indeed, raise in the mind a doubt on the principle itself of the operation. You introduce an extraneous body into the tunic, and you allow it to remain till inflammation is produced, and it is possible that the inflammatory action excited by extraneous bodies may tend to the suppurative instead of the adhesive form of inflammation. As, however, this result was only observed in one case, and as no such disposition was manifested in a number of cases, of which the success was perfect, we are perhaps warranted in drawing a conclusion generally in favour of the effects of the seton. Of course future cases (and as I shall continue to adopt the same plan of treatment such will not be wanting) will decide the point; but otherwise, in respect of having a mode of treatment enabling us to regulate the degree of inflammation, the plan here offered presents great advantages.

I should state that the requisite degree of inflammation is one which is attended with the ordinary symptoms of that process; that is to say, pain, heat, swelling, some redness, and some constitutional affection. There should be, I think, some affection of the pulse, some indication of febrile action in the system, before the seton is withdrawn. As soon as this has been observed, the threads may be removed, and I believe that you may then expect that you have excited inflammation enough to cure the disease. So that it is not whether the seton has remained in ten, twelve, or twenty hours, for this must be regulated by circumstances, but it is whether the requisite degree of inflammation is produced. I should say that twenty hours was about the average time for the seton to remain; but it will vary in different instances.

I might likewise observe that this plan of exciting inflammation by a seton will answer your purpose in various other cases. Ganglions, which you cannot get rid of by bursting them under the skin, or by puncturing them with a surgical needle, and which it would be dangerous to remove, also enlarged bursæ, may be treated by a seton in the same way. Inflammation being excited, and the surfaces of these cysts brought into contact, you obtain adhesion and cure the disease;—I have done it repeatedly. I might speak of its efficacy too in that case which by some has been called hydrocele, or dropsy of the thyroid gland. I was consulted respecting a large swelling in the neck of a lady, which was evidently situated in the thyroid gland. I found that she had had a great deal of surgical advice, and that many plans had been adopted but without success; I had no inducement, therefore, to go through the same routine of remedies. On making an accurate examination, it appeared to me quite clear that there was a cyst containing fluid; and considering it a fit case for the use of the seton, I introduced a canula, by means of a trocar, for the discharge of the fluid. I then carried a seton through the cyst, and allowed it to remain till what I considered a requisite degree of inflamation had been produced. The result shortly was, that she became completely cured, and remains well to this day. So that this is a plan of treatment which may be adopted in cases similar to hydrocele, where you wish to excite adhesive inflammation, and where you wish to have some mode of regulating the degree of inflammation required.

MEDICAL GAZETTE.

Saturday, January 30, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

CROWN AND ANCHOR MEETING.

WE must recur once more, however briefly, to the meeting of students at the Crown and Anchor, and some of the circumstances connected with that proceeding. The many letters we have received strongly confirm our views of the subject, and give us some additional information for which we were scarcely prepared. We regret we cannot find room for these communications—especially for those authenticated by the names of the writers: for one, however, (from an impartial observer), which reached us in good time, we have secured a place.

Our veracious contemporary, of course, makes a grand trumpeting of the affair, and chronicles the mighty deeds he has done. He claps his wings and crows at the thought of having had it all his own way; and even ventures to taunt the great body of the profession for not coming to support him. Is this stupidity or impudence? Knows he not that the fact of *his* intended presence, once announced, was sufficient to banish from the minds of men of any rank or standing all idea of participation in the proceedings—or of advocating the students' cause, which was already taken under such protection? Who were the advertised patrons of the meeting? Mr. Meade, the "grinder most aggrieved," filled the chair; and on the platform were seen the ominous visages of Mr. Wakley, *M.P.*, and his two friends, Messrs. Liston and Carpue. Need it surprise Mr. Wakley that nobody else was present—except the students, who were gathered principally to see the "rare monsters?"

In our last number we find we gave the meeting much more credit for orderly conduct than it was really entitled to. The report in the Lancet—revised as it was by the party who got up the entertainment — fully bears out most of what we have heard respecting the want of harmony which prevailed. Want of harmony is by far too mild a term to express the disunion, disputation, and almost open rupture, which, it seems, occurred. Some of our correspondents inform us of the vulgar and abusive language used by certain of the functionaries on the platform towards gentlemen who signified their dissent from what was going forward. Mr Meade's exhibition, we understand, was not of the most refined sort; well adapted, perhaps, "to split the ears of the *groundlings*," but eminently calculated at the same time, "to make the judicious grieve." Wakley talked the same sort of fustian which he has been in the habit of spouting forth and inditing for years back. But what said the worthy supporters of radical reform in medicine, Messrs. Liston and Carpue? Poor Mr. Carpue, "with his whitened locks," as his friend of the Lancet describes him, how gratifying it is to observe that his occupation is not yet quite gone, and that he still delights in the waters of agitation! His tirade on the College of Surgeons, for not electing on the Council "that great anatomist, unhappy Brookes," must have been very pathetic, edifying, and to the purpose: but we doubt if there was much occasion for informing the meeting, "with tears in his eyes," that the great Brookes died of poverty—"from absolute starvation." When he went so far, he might have told the whole of the story, and made a moral lesson of it to his hearers — it were better, we fancy, than many a tract from the Temperance Societies.

The report of our veracious contemporary puts a few words also into Mr. Liston's mouth, which enable us to form some idea of what it was he said, or attempted to say—for we sought that information in vain in other quarters. It was to the effect that he has now become a "staunch supporter of medical reform." Hear that, ye men of Edinburgh, who once enjoyed the valued presence of Mr. Liston, and never suspected that such a conversion could easily be wrought in him. Mr. Liston an advocate of reform! How curious might it not be, in this instance, to trace effects to their causes, and to ascertain what, besides "our prospects in London," could have produced this speedy change. "The present system," says this gentleman, "does not work well; and I hope that, in a little time, we shall have a much better one in its place." In what respect, might we ask, does not the system work well, so far as regards Mr. Liston? Is it possible that, in addition to the attractive influence of the *M.P.* of Finsbury, which brought the rest of the crowd to the meeting, there was any fellow-feeling between the surgeon of Gower-street and the worthy chairman? Was there any kindred grievance to be complained of? Had there been a recent rejection (a double one) of a candidate in Lincoln's-Inn-Fields, to irritate the feelings of the "celebrated surgeon" of the north, and all at once to drive him into the ranks of radical reform? Mark, we put the case interrogatively,—or hypothetically, if any one likes it better. Were we at liberty to speak all we know, we could readily, perhaps, assign the *true* cause for giving a holiday at Grosvenor Place, and hurrying off into Wakley's arms at the Crown and Anchor. But to return to the business of the meeting.

The getting up of a petition to be presented to parliament by Wakley was, of course, a grand object: and the *ruse* by which such a document was contrived, so as to appear to have the sanction of the Crown and Anchor assemblage, was at once ingenious and appropriate. No attempt was made on the part of the managers to procure the signatures of the students; they knew too well that that would show the feebleness of the whole thing; and the proposition that it should be otherwise, emanating from some non-contents, was immediately scouted. Thus the chairman procured the *privilege* to sign, in the name of the meeting, a petition which, were it exposed in the regular way for signature, would most undoubtedly not have had appended to it fifty names. But what could be more appropriate than that it should be signed by Mr. Meade himself, who in his own person combined the various characters of party chiefly aggrieved, chief patron, organizer, and *primo buffo* of the meeting?

It is gratifying to find that the whole proceeding has been denounced, not only by a large number of the steady and respectable students who attended the meeting out of pure curiosity, and were disgusted with the scene they witnessed, but by the pupils belonging to some of the large institutions, such as King's College, St. Thomas's Hospital, and Guy's. We refer our readers to the sensible resolutions (p. 704) passed by the St. Thomas's students, which contain a temperate disclaimer of participation in the Crown and Anchor affair, and at the same time point out the injustice of the cry raised against the Court of Examiners, without by any means subscribing servilely to the constitution or form of that Court. The proposal that, in the case of rejection, the candidate should have a right of *appeal* and a *public examination*, is not unreasonable, and is, we believe, sanctioned by the usage of some of the colleges in the United Kingdom.

ROYAL INSTITUTION.

Friday, Jan. 22, 1836.

Dr. Faraday on the Silicification of Vegetables.

THE series of evening meetings for the season commenced auspiciously, and to the great gratification of a large attendance of members and their friends, with an admirable lecture from Dr. Faraday. He began by calling attention to the leading properties of silica, its extraordinary intractibility when exposed to some of the most powerful agents—such as pure oil of vitriol, and muriatic acid—yet its solubility when in a certain state of combination with potash and water; its formation of a pasty substance in these circumstances, and again becoming dissolved by the addition of muriatic acid and water. The gaseous form of which it is susceptible, and the impalpable powder to which it may be reduced, were also noticed. But all this, rapidly and beautifully demonstrated, was merely preparatory to a description of the theories that have been proposed for accounting for the extraordinary change operated on vegetable bodies in certain localities. Some splendid fossil specimens were exhibited—in particular some stems of palm-tree, which were shewn to be identical in structure with the recent wood. Mr. Brown contributed largely to the rich supply of materials exhibited by the accomplished lecturer: so did Sir Francis Chantrey; and some fine microscopes for demonstrating the regular arrangement of the vessels in the silicified materials, were submitted to the inspection of visitors in the library. But, as Dr. Faraday said, how had nature operated these wonderful changes? It could not be by heat, for nothing in the original texture had suffered in the slightest degree, as it must have done under the influence of such an agent; it could not be by any ordinary or known solution of silica, for none such could possibly be injected into the delicate parts of a plant without destroying its finer tissues; nor did it seem likely that the process of nature was a tedious one, for the parts had evidently undergone a simultaneous alteration. In short, in the present state of our knowledge, all is doubt and difficulty on the subject; and the only theories which are in any way plausible, namely, those of Drs. Turner and Macculloch, have some insuperable objections to contend with. All the other operations of nature can, in a greater or less degree, be imitated by the ingenuity of the chemist; but there is a chemistry in the process of silicification which yet remains to be discovered,—some principle which, when known, will probably be recognized as a very simple one, however inscrutable and inexplicable it may be at present.

The conclusion of Dr. Faraday's discourse was an eloquent apology for the obstacles that render the path of discovery so difficult: if great discoveries could be attained with comparative facility, their charm and their value would perhaps be greatly underrated; many minor inventions, yet of great importance to society, would escape notice; and, above all, the collateral advantages arising from the discipline of the mind engaged in such investigations, would be totally lost.

CLINICAL LECTURE

ON

MEDULLARY SARCOMA OF THE TESTIS,

ON FRACTURE OF THE PATELLA, AND OF THE RIBS;

Delivered at the Westminster Hospital,

BY MR. GUTHRIE.

MEDULLARY SARCOMA.

THE preparation before you, gentlemen, is an excellent specimen of medullary sarcoma, or brain-like tumor of the testis; a malignant disease, which, in an advanced stage, is denominated, in general, fungus hæmatodes. I removed it some six weeks ago from a private patient, a young gentleman twenty years of age, and apparently otherwise remarkably healthy. Sir Astley Cooper saw him once with me, and I gave him half the testis, on the condition that he injected the whole: it could not, therefore, have been better done. When the tumor was first divided it looked like so much medullary matter replacing the testis and epididymis, the situation of which could not be distinguished. Hardened now, by being placed some days in alcohol, it looks as if the medullary matter had been formed in distinct lobules or cells of various sizes, and not in one homogeneous mass. It is not very vascular, although two or three parts are much more so than others, as you can readily perceive. The tunica vaginalis is adherent to the tunica reflexa, although it has admitted of separation at the upper part. It was neither inflamed nor adherent to the external parts.

The complaint began in the month of April, or rather was then first observed, in consequence of the testis being larger than the other, and as he had not received an injury, nor could attribute it to any other cause, he did not further advert to it, more particularly as he suffered no pain nor inconvenience of any kind, save from

its gradually increasing size, which made him, at the end of about five months, show it to his family attendant, who, after some little time, recommended him to apply to me. The right testis was as large as the closed hands, rather oval than spherical; the spermatic cord quite sound; and as the skin of the scrotum was loose and unaffected, the glands in the groin were free from disease, which I may say, by the way, only takes place in these cases when the external parts are implicated, their absorbents leading to the groin, whilst those of the testes proper go to the lumbar glands, a region in which they cannot be seen or felt unless they are much enlarged. There was no uneasiness in the back. The skin of the scrotum was as natural in appearance as it usually is when stretched over a large tumor. An experienced eye might detect perhaps a more general degree of vascularity, but it was not sufficiently observable to deserve special remark, and I allude to it because the increase of vascularity is sometimes supposed to be a well-marked symptom of this disease, and it is so at a later period of its existence or progress. The testes when poised on the hand gave a peculiar sensation as to weight which it is not easy to describe. It was heavier than serum or other fluid, and less elastic in its pressure. It was not as heavy as a solid mass of the same size composed of a different substance; and taken as a whole, as to weight and poise on the hand, it strongly resembled the sensations arising from a tumor composed of half hardened testis, half serum, firmly bound down upon it by the tunica vaginalis; and yet there was a something peculiar in it I cannot describe. When pressed by one finger it yielded a fair feeling of fluctuation, which did not appear distinct when two were applied in the usual manner at a short distance from each other, or on either side. It was then only soft and springy, but not yielding so as to cause a hollow to be visible, or to make any impression. These trials gave but little uneasiness, and grasping the whole with the hand and making pressure was not more felt, nor indeed so much as by a sound testis. I gave my opinion at once, that the disease was an incurable one, and would be malignant if the parts were allowed to remain. To convince the patient that it was not a fluid, as he supposed, I passed a small trochar into it, but nothing came through the canula but a few drops of blood. This satisfied me it was not a cystic or hydatid testis—a disease equally incurable, but never at any time malignant.

You will naturally ask me for a description of the cystic testis, but one disease is as rare as the other, and as difficult to detect or decide upon with accuracy. It varies only in a slight degree in external characters—in weight, poise, and the springy or false fluctuating feel—from the medullary disease I have described, but that slight degree brings it nearer to a hydrocele, from which it can only be distinguished in some cases by puncturing it. A fairly-educated surgeon—that is, one who has educated himself by diligent attendance on hospital or public practice, after the usual course of study has been gone through—will know that he has something more than is commonly met with to treat, and which an operation can alone cure, and he punctures in order to decide the point whether the swelling contains a fluid or not. It is needless to remark to you, that when a hydrocele is transparent a good surgeon does not want that information to enable him to detect the complaint, and the incapability of the tumor to transmit light is no proof that it is not a hydrocele, or a sac containing a fluid. A puncture with a lancet or trochar solves the mystery. If the case is one of cystic testicle a teaspoonful of fluid like serum escapes, and the discharge ceases. If a probe is pushed through the tube it meets with some resistance, which is easily overcome, and another teaspoonful is evacuated. The disease is a transformation of the whole testis and epididymis into a collection of cysts, each containing more or less of serous fluid, and the best comparison I can make of it when cut into halves, so as to render it intelligible, is to say it resembles a short thick bunch of green Portugal grapes that has been cut down the middle of the stalk, each grape, large and small, having been cut across, and the fluid, when the tumor is punctured, comes from each grape or cyst independently. The finest specimen of this disease I ever saw occurred in the case of an officer whose testis I removed some years ago. I gave Sir A. Cooper one half, the other is in the Military Museum at Chatham. This patient had no return of the disease any where, and I have already said it is not malignant, and relief is only obtained from its weight by removal, for it gives no other offence.

As nothing came through the opening I had made into the testis in question by the trochar but a little blood, I was satisfied it was neither a hydrocele nor a cystic testis, and that it could be nothing but a medullary sarcoma, or a pulpy testis, or, as it was anciently termed, a soft cancer, and that my first impression was correct.

I will not detain you further with any unnecessary particulars, but advert to what may be called the peculiarities of the case;

and I must here regret that I am obliged to speak of feelings and sensations which, although they may be, and are, quite clear to me, may be unintelligible in their niceties to you. I wish I could have shown you the case, and allowed you to have judged for yourselves. I have, in my simplicity or enthusiasm, which you please, asked several private patients to come down here and allow themselves to be shown to you. One gentleman promised to come, with a particular injury of the elbow, but he did not, and I lost my patient, for I never saw him again. You must therefore, gentlemen, make the most you can of what I have said, and I am always happy to find you asking questions which can lead to your instruction. When I can give you a salutary answer I am glad to do it, when I cannot I say so, and am as ready to learn as yourselves. It is stated by Sir A. Cooper, in his work on the diseases of the testes, and he has given proof of the statement being correct, that this disease (medullary sarcoma) begins with an enlargement which is at first accompanied with great hardness, like scirrhus. In this instance there was no hardness, but a gradual increase of size without pain (so says the patient, at least), and it is important to know that this disease, when it takes place without any accident or injury, does sometimes begin so quietly and so insidiously as to be observed only from the increase of the size of the part, without there being any thing peculiarly unnatural in it, which might otherwise attract particular attention. The young gentleman appears to have been in perfect health, and to have suffered from this disease without any apparent local or constitutional cause. Let us hope this is the case, and that he will not be so unfortunate as to have a reappearance of it in any other part of the body. The records or remembrances of surgery are imperfect on this point. When the disease is allowed to run its course, so that the veins of the scrotum become varicose, pains shoot through the part, the spermatic cord has become hard or irregular, the skin adheres to the parts beneath, or has even ulcerated, allowing a fungus to shoot forth bleeding on the slightest touch; an operation under all or any even of these symptoms has only delayed for a short time the evil hour of death. The disease has therefore been often considered an incurable one in all its stages, which I firmly believe not to be the case, and that if the operation is done at an early period success will generally follow its performance.

The last well-marked case of this disease I saw, and the result of which I know (for it is nothing to see a case unless the result is known,) was in the person of a courier, who received a blow from the saddle on the part, to which he attributed the occurrence of the disease; it was removed by Sir B. Brodie some five years ago in St. George's Hospital. The man is now alive and well. Sir Astley Cooper tells me the last case he saw, and in which the disease was removed, was two years ago, and the person is still alive and well. Mr. Keate, and most of my friends in the Court of Examiners of the College of Surgeons, and who are men of the greatest experience in London, can, I believe, all recollect a case or more in which, for some years, no re-appearance of disease took place; but unless these cases occur in private practice it is rarely that the subsequent history can be obtained, and hospital records and remembrances are therefore necessarily deficient. Let us hope, gentlemen, that we shall in time collect, through the medium of clinical remarks or otherwise, a body of evidence which will lead to the determinate result that the operation, if done sufficiently early, will generally, if not always, be successful. I am aware there may be a quibble about the words "sufficiently early," but we may with propriety consider these words to mean the absence of any marked symptoms beyond those I have alluded to as present in the case of the subject of our observations, for it is impossible for any one to know at what time the lumbar glands become affected; and I should be glad if any one could tell me why it is that they ever escape being affected after the disease has existed for a month, nay, even for a week.

We had a case in the old hospital to which I then drew the attention of the students in a particular manner, which might be mistaken for a medullary sarcoma by an inexperienced or inattentive person. The tumor was soft and pulpy, clearly not containing a serous fluid, barely fluctuating, but the skin was not natural; it was shining and soft, although not stretched, and was scaly, indicating the inflammation of a deep-seated abscess. An incision at the lower part evacuated a large quantity of a thick oily brown creamy fluid, and cured the patient. You should never remove a testis without examining it first by a large or deep-grooved needle, by puncture, or by an incision, for no man is free from error, and life is not long enough for any one to learn surgery thoroughly. So say, at least, the ablest I know in the profession.

TREATMENT OF FRACTURED PATELLA.

I have several times drawn your attention to two men lying opposite to each other in Northumberland Ward—one with a fractured patella, the other with a severe

crush of the chest between the wheel of a dray and the wall. The fractured patella I have treated without bandage of any kind. The man was placed on a fracture-bed, the two ends of which were raised so as to resemble the letter V, on the inside of which he lies, the toe of the afflicted side being as high as the head. The elevation of the foot and leg was not, however, sufficient to relax the rectus muscle unless the body was raised also, and it was only when this was done that the broken portions of bone came spontaneously into apposition. A month has elapsed since the accident, and the cure is surgically completed. It will be, however, three months yet before he will be able to use the leg; and even then the point of union, which is ligamentous, will stretch, without great care is taken to prevent it. In an exceedingly well-joined fracture I have seen the uniting substance stretch two inches after the patient had left the hospital, from the want of due precaution in the use of the leg—from, in fact, bending it too soon. The only point for observation in this case is, that the cure has been accomplished by position alone, and without the aid of any bandage.

FRACTURED RIBS.

The case of fractured ribs is not mine, and I cannot therefore say as much about it to you as I could wish. I have drawn your attention to it before, and I have thought it worth my while to visit the hospital every day to follow up the treatment. I hope you have all done the same. Am I right in supposing you have done so? The stethoscope has indicated daily the progress and the subsidence of disease, and I believe the man will get well. The only point I mean to comment on is, the application or non application of a bandage in a case of fractured rib. In this case a roller gave relief at first, but on Thursday last, when I saw him in passing through the ward late in the afternoon, he was in a good deal of distress in breathing, and I suggested to the house-surgeon the propriety of removing the bandage. This recommendation he adopted in the absence of his surgeon, and the man has been amending ever since. Sir W. Blizard was in the habit of saying that a bandage should never be applied either to a fractured patella or to the chest for broken ribs, in which latter direction he was certainly wrong. I have shown you, on former occasions, the great advantage as to ease and comfort to be derived from a bandage in certain cases; whilst in others, such as the present, it does harm. The truth and good practice lie *in medio*, and I am sorry to say I cannot give you a precise direction when you should resort to it—when not. You must ascertain the fact by a trial. I believe, but I am not quite sure, that a bandage is less useful when it compresses broken ribs to which the diaphragm is attached. It tends to prevent both of the ordinary ways of breathing. It was so in the present instance.

LECTURES

ON THE

DISEASES OF THE NERVOUS SYSTEM.

BY M. ANDRAL.

As published in the *Gazette des Hôpitaux*, with the approval of the learned Professor.

HÆMORRHAGE OF THE NERVOUS CENTRES *.

Hæmorrhage of the Cerebellum; Analysis of thirty-two Cases, shewing that Paralysis generally occurs, and that Hemiplegia of the side opposite the effusion is its most common form—Hæmorrhage of the Spinal Cord—Various forms of Paralysis—Muscles of the Eye, Cheeks, Tongue, &c.—Cause of Paralysis—Lesions of Sensibility.

Hæmorrhage of the cerebellum.—This is not a very common complaint, and, so far as I know, only thirty-two cases of it have been published. Two questions naturally arise—1st, Does this form of hæmorrhage produce paralysis? and, secondly, On what side is the paralysis observed in those cases where it exists?

It is very difficult to decide these questions *à priori*, for, in fact, notwithstanding all the discussions upon the subject, nothing very definite is known with regard to the functions of the cerebellum,—and observation alone can enable us to solve the problem. If we would answer the question as to the seat of the paralysis from our first impression, anatomy would lead us to say that the loss of motion must exist on the same side as the organic lesion, because the *corpora restiformia*, which unite to form the cerebellum, do not interlace in the same manner as the anterior pyramids; but this anatomical reason, derived from the presence or absence of crossing in the fibres, has been combatted in the former lecture, in speaking of paralysis of the face. Here, in hæmorrhage of the cerebellum, the paralysis is on the opposite side, just as with regard to the brain.

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* By an oversight, the general heading of the last lecture, page 616, was "Inflammation of the Spinal Marrow," instead of the above.

Let us now proceed to examine the cases which have been placed on record. I have already done this in the fifth volume of my Clinique; but since the last edition of the work the facts have accumulated, there being, as I have already said, thirty-two such on record. In twelve of these the details are not sufficient to enable us to say whether there was paralysis or not: we are merely told that there was a violent attack of apoplexy, rapidly terminating in death. In these twelve cases the lesion was found seven times in the middle lobe. Six of them belong to M. Serres, and are to be found in the second volume of his Anatomy of the Brain; the seventh is recorded by M. Dance, in his Memoir on Acute Hydrocephalus; three others consisted of hæmorrhage into one of the lateral lobes. I also have published a case of hæmorrhage into one of the lateral lobes, but the individual was not seen till after death. Another case belongs to Dr. Abercrombie: there was effusion into the right lateral lobe; there was constant coma; nothing is mentioned with regard to the movements; death at the end of forty hours. Two other cases presented simultaneous effusion into both lateral lobes; the first is altogether deficient in details, the last is contained in the work of Morgagni. There was hæmorrhage of both lateral lobes; the patient was found dead, with the muscles of both upper extremities strongly contracted.

We have now to pass in review twenty cases in which the details are more complete. In three of them only was paralysis absent. The first is due to M. Bayle; the patient suddenly became insensible, but retained the power of movement, for he quickly pulled away the upper and lower extremities when either of them were pinched. He died five days after the loss of consciousness, and on the third day from the attack he was seized with convulsions of the lower extremities and remarkable rigidity of the neck. Considerable hæmorrhage was found into the middle lobe of the cerebellum. The second is to be found in the thesis of M. Michelet, printed in 1827. An individual died two years after an attack of apoplexy; there had been blindness from amaurosis, but nothing is stated with regard to movement; the hæmorrhage was seated in one of the lateral lobes. The third case occurred to M. Droullain: there was no paralysis, but convulsions and rigidity of the neck, as in the case of M. Bayle; there was effusion of blood into one of the lateral lobes.

There remain seventeen cases in which the existence of paralysis was well made out; and of the others there are only two,—that of M. Bayle and that of M. Droullain,—with regard to which it can positively be said that paralysis did not exist. These seventeen cases resolve the first question in the affirmative, namely, whether hæmorrhage of the cerebellum produces paralysis. In all these cases the form of the paralysis was hemiplegia.

Let us now see what was the seat of the effusion. In one case only did it occur in the middle lobe. The observation is due to M. Ginaud, who published it in the Clinique des Hôpitaux, No. 90; there had been hemiplegia of the left side. In the sixteen other cases the hæmorrhage was situated in one of the lateral lobes; and we have to inquire whether the paralysis existed on the same or on the opposite side. In eleven the hemiplegia was opposite to the lesion. Of these eleven cases five are recorded in my Clinique, and others have been related by MM. Serres, Piorry, Rochoux, &c. In one of my cases, as well as in one recorded by M. Chambeyron, there was also hæmorrhage into the corresponding lobe of the brain; which reduces the eleven cases to nine in which the lesion was limited to the cerebellum.

We have formerly seen, with regard to the brain, that the paralysis was sometimes on the same side, sometimes on the side opposite to the organic lesion. Is it the same with respect to the cerebellum? To this question the last series of cases will afford us a rely.

There are two classes of cases to be considered, namely, where the cerebellum alone is affected, and where, while it is affected on one side, the cerebrum is affected on the other. 1st, May the cerebellum be affected alone, and on one side only? A case of this kind has been cited by M. Tarvenier: it is the only one which can be opposed to the crossing of the paralysis. An individual was attacked, in 1812, with apoplexy, characterized by complete paralysis of the left side, and by loss of speech, with preservation of the intellect. In 1820 he was seized with loss of power over the right side, and died in a state of coma, with stertor. In the left lobe of the cerebellum was found an old lesion, consisting of an apoplectic cyst, which corresponded to the *left* palsy of eight years' standing. At the same time, in the middle part of the left hemisphere of the brain, was found a recent effusion, which had caused the palsy of the limbs on the right side. At first sight this case appears complete; nevertheless, M. Tarvenier did not see the patient during his first attack, and took his account from the testimony of the man's wife. The case, therefore, standing alone as it does, is not of sufficient authority to warrant our drawing from it any conclu-

sion favourable to the possibility of direct paralysis from hæmorrhage of the cerebellum.

In the next division are to be placed those cases in which hæmorrhage has taken place simultaneously into one of the lateral lobes of the cerebellum, and into one of the hemispheres of the brain. One such instance is to be found in the thesis of M. Droullain; another was seen by M. Quesne at the Bicêtre; a third is to be found in the work of M. Rostan; and a fourth in the fifth volume of my Clinique. When hæmorrhage takes place into the lobes of the cerebellum alone, the paralysis is on the opposite side. What ought to happen when the cerebellum is affected on one side, and the cerebrum on the other? Clearly the limbs of both sides ought to be paralyzed. Yet, in the four cases above quoted, the power of motion remained entire on the side opposite to the lesion of the cerebellum,—the affection of the brain alone had any influence over the limbs; and the same effect also took place where there was lesion different from hæmorrhage existing simultaneously on the opposite sides of the brain and cerebellum—as for example, in atrophy. The general consequences which follow from these cases are too obvious to require that they should be pointed out.

Hæmorrhage of the spinal cord.—Paralysis may also occur when the effusion of blood takes place into the spinal cord. This kind of case is very simple. In the greater number of instances of hæmorrhage into the cord, double paralysis takes place, and the upper or lower extremities are affected according as the effusion occurs high up or lower down in the spine. Sometimes hemiplegia has been observed where hæmorrhage has taken place into the anterior cords of the spinal marrow; and such paralysis is always direct when only one cord is implicated.

Various forms of paralysis.—Other parts besides the limbs may be paralyzed in hæmorrhage of the nervous centres, and this particularly when the brain is the part affected; for with respect to other parts, the circumstances are not sufficiently ascertained. The muscles of the eye have been known, though very rarely, to be paralyzed; and as all its muscles are not affected at the same time, inasmuch as they do not all obey the same nerves, strabismus has been much less frequently seen in hæmorrhage of the brain than in various other affections of the same organ. The elevator of the upper eye-lid is sometimes paralyzed; and I have known this occurrence precede the hæmorrhage by fifteen days, without any other premonitory symptoms.

When the paralysis affects the muscles of the cheek, it is always on the same side as that of the limbs; the commissure of the lips is dragged to the opposite side by the muscles which are not paralyzed. The paralysis of the buccinator produces certain phenomena when the patients breathe with the mouth shut; the muscle becomes distended, and when the lips are separated the air escapes with a noise, constituting what is called the patient *smoking his pipe.* When he attempts to masticate, it is perceived that he cannot use the necessary muscles. There are also other effects resulting from paralysis of the cheeks, which, however, it is unnecessary to detail. As to the rest, although in the immense majority of cases the face be paralyzed on the same side as the limbs, there are, however, some very rare instances in which the opposite condition has been met with; we must then suppose that a double lesion exists.

Movements of the tongue.—These almost always undergo some modification. We may have complete hemiplegia, with paralysis of the face, without the tongue being the least affected; at other times the movements of the tongue are completely abolished, so that the patient is absolutely unable to put it out. Articulation is impossible; and this does not depend on want of the association of ideas necessary to the formation of language, but here it is the mechanical movementsof the tongue which are impeded. There is also another kind of dumbness, of which it is not within my present purpose to speak. Sometimes the paralysis of the tongue takes place only on one side; if the patient puts it out, it is to one side:—to which side? There is a variety in this respect. It is generally to the paralyzed side; however, in some rare cases the tongue is directed towards the side opposite to that of the paralyzed limbs: this probably depends on differences in the extent and seat of the palsy affecting the numerous muscles of the tongue. The movements of this organ often return before those of the limbs. The speech begins to be re established at the end of ten days, a fortnight, or a month; and it sometimes becomes completely or nearly restored, although the limbs recover none of their power. Most frequently the paralysis of the tongue is not so well marked as that of the limbs; but there are also some cases in which, after the attack, the limbs are scarcely feeble or numb, whilst the abolition of the action of the tongue is complete. We must also remark, that the movements of the tongue may be completely destroyed, although there be no loss of consciousness.

Muscles of the neck.—Paralysis of these is rare; when it does occur, it produces some change in the manner of carrying the head.

Muscles of respiration. — These also are very rarely affected with palsy, unless in cases of apoplexy which prove rapidly fatal; and the less degree of frequency of this paralysis is not at all astonishing in regard to muscles which are on the limits between the life of relation and vegetable life, and which serve as the transition between the muscles of organic and animal life.

The muscles of the larynx.—There are very few examples of this paralysis. M. Moulin, however, has related a case of it.

Muscles of the pharynx and œsophagus. — These muscles (which, however, are under the influence of nutritive life) are sometimes paralysed in very severe apoplexies. This paralysis, which impedes deglutition, leads but to an unfavourable prognosis. It is by this inability to swallow that the attack commences. M. Flandin has related the case of an individual, 28 years of years, who was seized all at once with loss of deglutition, without any other symptom. No suspicion was entertained of cerebral hæmorrhage, but this suddenly manifested itself will all its usual symptoms.

Muscles of the rectum.—These may be implicated in the paralysis; and from this results the difficulty of expelling the contents of the bowels.

Muscles of the bladder. — This form of palsy, which affects chiefly the fibres of the body of the viscus, is sufficently common; which circumstance ought to induce the attendant always to examine the bladder with the utmost care, in all cases of apoplexy, for the sensibility being diminished, the organ may actually burst from excessive distention.

It has been said that the muscular fibres of the stomach may be paralysed in hæmorrhage of the nervous centres; but the statement does not rest on any satisfactory evidence.

Cause of paralysis.—The paralysis, particularly that of the limbs, may remain till death, and thus last for a longer or a shorter time, according to the duration of the case. It may also disappear, but this only takes place slowly. The very rapid disappearance of paralysis is a strong reason for believing that it is not dependent upon hæmorrhage of the nervous centres. When life is prolonged during many years, the paralysis may continue still the same. Persons have been seen who have been attacked with apoplexy at the age of 40, and in whom, at 70, the palsy was as complete as at its onset. When paralysis lasts long, nutrition sometimes becomes impaired, and the limb wastes. This wasting commences, though certainly very rarely, within a month after the first attack, as I myself have seen. Instead of remaining thus persistent, the palsy may progressively diminish; but it is to be remarked that this form—this enduring palsy—is sometimes dependent upon a very slight lesion of the brain. Thus on the death of an individual who had suffered from paralysis for twenty years, only a white hard line was found in the brain, a simple cicatrix having interrupted the nervous continuity.

Lastly, the paralysis may disappear entirely. The hæmorrhagic nucleus in this case passes into a perfect cicatrix, across which the nervous influence admits of being propagated. When it disappears gradually, it does so in a certain manner. If, for example, we suppose the palsy to exist in the cheeks, lips, tongue, and limbs, it will cease first in the tongue, then in the face, then in the lips; and at a time when all the parts primarily affected have recovered their movements, the limbs may still be paralysed, unless in cases which constitute exceptions to the general rule. As to the rest, the palsy of the lower limbs yields sooner than that of the upper. The former, nevertheless, long retain some difficulty in their progression, but in the end every trace of paralysis by degrees may disappear.

In the cases where the power of motion appears to have become entirely natural, are we to suppose that there is always a complete disappearance of the lesion? Not so; for sometimes even then cysts, filled with serum, remain; but these, it is true, are exceptions.

LESIONS OF SENSIBILITY.

These lesions are various, and less constant than those which affect motion. They may manifest themselves first in the encephalon; secondly, in the skin; thirdly, in the organs of sense.

In the encephalon.—Frequently no change of sensation is observed in the brain before an apopleetic attack; but sometimes there is a weight about the head, headache, ringing in the ears, or giddiness, as premonitory of the hæmorrhage.

In the skin.—In most cases its sensibility is in no degree affected before the hæmorrhage; but various other disorders may be met with, such as the limbs easily becoming cold, numbness, formication, &c. These different affections may display themselves only in the skin of the fingers, or throughout the whole extent of the limbs, usually those of one side, viz. that which afterwards becomes paralysed.

Sometimes these phenomena shew themselves in the limbs of both sides at once; and this may be the result of different degrees of congestion. These lesions of the sensibility of the skin shew themselves sometimes only a few days, sometimes several years, before the attack. Thus in the fifth volume of my Clinique is the case of a woman who was attacked with apoplexy at the age of 52 years, in whom not the slightest disturbance of the motive powers had been observed, but who, from the age of 17, had experienced numbness and creeping in the limbs of the right side, which symptoms had at first been irregular and at long intervals, as well as confined to the arm, but which afterwards became constant, and extended to both limbs.

Thus previous to the attack, before the moment at which the effusion occurs, various disorders of the sensibility may take place, so that it is occasionally possible for us to predict on which side the hæmorrhage will supervene, by the side of the body where the premonitory phenomena of the sensibility shew themselves. Let me remark, however, that in general apoplexy is not preceded by any disturbance of this function.

When paralysis has occurred, the sensibility of the affected side is diminished or abolished; and in general, when the disease takes a favourable course, the lesions of sensation disappear before those of motion.

The mucous membranes may participate in the abolition of sensibility; thus in some apoplectic cases the conjunctiva is insensible when touched with the finger, which is in keeping with the same defect in other organs, and depends upon the alteration of the fifth pair of nerves.

Organs of sense.—Sight is sometimes, but not always, disturbed. We see individuals struck down with apoplexy, and affected with paralysis and loss of sensation, where, nevertheless, consciousness and vision remain. Different sensations resulting from disturbance of this function are experienced by patients, who describe them in different ways; some say that they have motes before the eyes; others that they see the light as through a cloud, just as on the onset of cataract,—yet here the crystalline is clear; others see various colours. Sometimes those who at a later period are attacked with apoplexy have the sight modified for a longer or shorter time before the attack, in such a manner that all objects appear double, a symptom which is sometimes transient, being present one day and not another; in other cases it is persistent. In some cases the loss of sight is nearly complete; but such cases are very rare. When the sight is lost, this may take place on one side or on both; and this blindness coincides with the loss of numerous other senses. In various works cases are to be found in which the sight is said to have been preternaturally acute for some time before the attack.

[To be continued.]

ADEQUATE REMUNERATION OF PRACTITIONERS

FOR PAUPER MEDICAL ATTENDANCE.

To the Editor of the Medical Gazette.

Sir,

Having been lately requested to sign a protest of the general practitioners in medicine of this part of the country, against the misrepresentations of the Poor-Law Commissioners, which, although in no way interested, except as the advocate of justice and humanity, I had great pleasure in doing; and having for many months past listened to endless discussions, and read an infinity of remonstrances, and letters, and complaints, on the subject, I still think that the main points and strength of the case have been overlooked; and therefore, should you think this letter of an impartial bystander worth publication, I beg you will give it a place in the first spare corner of the Medical Gazette.

Every one will admit that our Government must have been actuated by the laudable desire of insuring proper medical attendance to the sick poor of the country; and it remains to be seen whether their agents, the Poor-Law Commissioners, have adopted the proper steps for effecting that national object, or whether they have not, on the contrary, sought to slur over the calls of true humanity, and, as I shall presently shew, by practising on the fears of the established medical officer, driven bargains, and on degrading terms exacted an amount of duty from him, which never can be rendered with the effect sought to be attained, because in their rage for union and centralization, they have in many instances removed the sick poor as effectually beyond his eye, and out of his reach, as if the object had been to deny them all medical assistance whatever.

It has ever been easy to be charitable at the expense of others; and the game has too often been played of seeking popular applause to the prejudice of justice. When the Poor-Law Commissioners, therefore,

claimed credit for economical reform, by advertising for the lowest tenders, and calling up the inexperienced needy adventurer from the schools, in want of a place, to underbid the established practitioner who had long been in charge, they handed over the sick poor to serve the purpose of the former's unprincipled speculation, or held him up *in terrorem* over the latter, with the view of imposing conditions upon him which he never could execute in fairness either to the unfortunate paupers or himself; but to which, in order to preserve the practice on which he subsisted, he found himself obliged to submit.

This surely was intimidation, and not justice; far less could it come under the denomination either of charity or humanity. And as it must be the aim of every parental government to protect all classes of its subjects, the numerous medical practitioners throughout the land have a right to invoke its protecting shield against their present oppressors; and this can be extended to them effectually only in the way of a national enactment, providing medical attendance in every parish for all the paupers, whether sick or well, in the list, on equitable terms, at so much annual cost per head; and then the Commissioners, by selecting and appointing the best, instead of advertising for the worst, may exercise, and under proper regulation enforce, that humanity to which they were so falsely laying claim.

As a contract, take it in what way you will, always implies eventual gain to the contractor, every system of tender and underbidding must be bad; and in the long run the sick poor must be the only sufferers.

The system I advocate, were it fairly entertained by the legislature, would be national, humane, and just to all parties. It has for ages, under certain modifications, been practised in the army and navy; it was, as I have witnessed, executed with the best effect amongst all the communities of negroes throughout the West Indies; and I can conceive no good reason why, *mutatis mutandis*, it may not be brought into operation amongst the pauper population of Great Britain. One mighty advantage would be the simplification of accounts, of regulation, and of duty. All would be comprehended, without confusion or dispute, under one principle of action; and if the reciprocity of compensation and duty be justly poised, the sick poor would be duly cared for, and the medical attendant not degradingly, because not unjustly, rewarded.

Any other plan than this must be fraudulent, as laying claim to charity, and that too without effecting the object, at the expense of the medical profession; while it cannot fail to involve the members of this last in perpetual contest with the Poor-Law authorities; the one seeking to impose, the other to avoid, impossible duties, and all to the prejudice of the poor. This would be clear and intelligible to every one; and should it appear so to the profession, I hope they will arise as one man to seek the protection which its enactment would afford. I do not here enter into details to prove the foregoing, or to shew that the best established practitioner of every parish, from his knowledge of the pauper's character, and the responsibility for his own, must ever be the fittest in the first instance, and should never be employed out of its bounds, or, at least, beyond his beat; for the sick poor require to be protected against neglect, as much as their doctor against oppression; but I am ready to do so should my statements be disputed.

Having of late avoided to converse, and ceased to read, upon this tiresome subject (for nothing is so tiresome as the reiterated language of unredressed grievance), I really cannot tell whether the above proposal may not already have been laid before the public through some other channel: if it has, this letter will be superfluous; but if otherwise, its publication in the Medical Gazette may probably in some degree serve the cause of the profession.—I have the honour to remain, sir,

Your most obedient servant,

WM. FERGUSSON, M.D.
Inspector-General of Hospitals, H.P.

Windsor, Jan. 20, 1836.

THE MEADE-AND-WAKLEY MEETING OF MEDICAL STUDENTS.

CONDUCT OF THE PARTIES *.

To the Editor of the Medical Gazette.

SIR,

PERMIT me, through the medium of your journal, to call the attention of the profession, and of the public generally, to some peculiarities that marked the proceedings of the *soi-disant* meeting of medical students, held on Monday evening, in the Strand.

Allured by curiosity, as indeed were a great number of individuals present, knowing little or nothing of the merits or demerits of the questions to be brought under discussion, and having been subsequently led to make some inquiries as to the truth of some of the statements then and there put forth, I think it due to the

* The name and address of the writer have been sent to us.—E. G.

profession, and to the public generally, to offer a few passing remarks, in which I propose to confine myself to what would be called the business of the meeting.

The students were invited to the meeting without their being told more than that the proceedings would be of a nature deeply affecting their interests. We were soon enlightened as to the objects the conveners had in view. A medical student had gone up to Apothecaries' Hall, for examination; he was pronounced incompetent to undertake the duties of his profession, and sent back to complete his studies. Whether the pupil had been careless, or his teacher had failed in discharging his duty, did not transpire; but judge of my surprise to find the said teacher of the rejected student occupying the chair on the occasion. The *onus* of the young man's rejection must rest somewhere. Assuming the Board of Examiners to be impartial—and it was the interest of the board to be lenient rather than severe, as unsuccessful students, it appears, do not pay the fees — assuming, then, the Board to be impartial, the fault must be either with the pupil or the teacher, and both must doubtless feel considerable soreness at the rejection. Was it proper, then, nay, was it decent, that a party concerned should preside — that the defendant should be the judge? But it was so; and he exercised the prerogative accordingly. Could a man so delicately situated be expected to bring an unbiassed mind to the discussion, or treat with fairness those whose conduct he attacked, whose motives he impugned, and, in animadverting upon whom, he forgot that all the parties concerned were, or ought to be, gentlemen. He said this rejection was the second that occurred to his pupils; and although it did not appear that any other teacher had to complain of similar treatment, he considered the rejections a proof of personal pique on the part of the Examiners. This was a miserable subterfuge; for he did not even condescend to inform the students that the gentlemen whom he loaded with coarse vituperations even knew him personally. How important would it have been to have shewn the grounds on which he was personally unpopular with the Examiners!

But let me ask, where did the chairman get his information as to the questions proposed, and the answers given? Could it be any thing but *ex parte*; any thing but the *ipse dixit* of his pupil, embellished and corrected by himself. Well; here is a young man sent up for examination, and failing to answer satisfactorily the queries proposed, he is remanded. Either the Examiner or himself is in error. Will he be likely to retire from the board and take the blame upon himself? Will his teacher, or *grinder*, whose business it is to furbish up students of limited information, be likely to acknowledge himself in error, or will he not rather be inclined to say practically, "I am superior teacher, you are a proficient, and the Board of Examiners must be in the wrong?" So it is; the student retires chagrined and mortified: not willing to confess his stupidity or carelessness, he tells his grinder how cruelly he was overhauled, and the teacher, not willing to advertise his own incompetency as a judge in the proficiency of his pupils, says in effect, "aye, you are a very clever fellow; and as for the Examiners, they are tyrannical, despotic, corrupt," and what ever else he can, in the abundance of his *liberalism*, think of. This young man's partial statements (it is next to impossible they can be otherwise) are taken to his teacher, and by the latter, whose professional reputation is involved, to a public meeting, and there the man is placed in the chair to "play his own fiddle;" his calumnies are applauded, his base insinuations pronounced capital, and his jaundiced view of a case he has made his own, received as gospel. Really the stretch of credulity here exhibited seems almost incredible, and yet it is no fiction. It is difficult to say which is most admirable, the modesty of the man who could obtrude himself at all (much less as chairman) upon the meeting, under such circumstances, or the shallowness and trickery of those who could tolerate such an outrage of all decency.

Next in order, my attention was called to the insulting exhortations of the chairman to his audience, to behave with propriety. For myself, I felt insulted. What! is it to be credited that medical students know no better how to demean themselves, and at a public meeting, too, at which matters affecting their own interests are to be discussed, than so many coal-heavers, that they should be cautioned again and again not to be so uproarious? Surely the chairman must have a sorry opinion of the medical students present; but, as was subsequently proved, his exhortations, so frequently reiterated, were by no means unnecessary. On taking a survey of the motley assembly, I was more than convinced that the young gentlemen present would need a pretty severe examination, and that many, unless in future they betake themselves to study, and leave off raking, would "when weighed in the balance be found wanting." The carelessness of many, the obstreperousness of others, and the positive gross ill conduct of not a few, gave one a most painful and humiliating picture of the march-of-intellect gentlemen of the medical schools.

Any thing like being heard, if opposed to the medical radicals, was out of the question. "Bah," "shut your shop," "show him up," "lift him upon the table," "don't make a fool of yourself," "let's have Wakley, we want to hear him, and go to half play," with an admixture of howls, groans, and cat-calls, distinguished the proceedings of the evening, and proved that the seemingly impertinent advice of the chairman was not out of season.

The mover of the first resolution came ready charged with a mighty grandiloquent speech, but unfortunately he lost the *thread* of his discourse just when boiling over with indignation. "That, that, that," said he, "I don't know what to call it; that monstrosity — that monopoly — that sink of infamy, oppression, and tyranny, shall be as execrable in its death as it has been odious in its life." Having gathered himself up at the *terminus,* and discharged this elegant piece of eloquence, the pop-gun disappeared, and was pronounced very grand. The orator in question proved, "quite entirely" to the satisfaction of the meeting, that medical students of the present day are vastly superior to medical practitioners of twenty, thirty, or forty years' experience.

The trick to cover "the nakedness of the land," by authorizing the chairman to sign a petition to the Commons, in be half of the meeting, succeeded well. An amendment was moved, that each student sign it; but that would have proved too much. "Don't humbug us with signing," said one; "I'm not going to be *hashed* up with such a concern," said another; and if the amendment had been carried, verily there would not have been fifty signatures affixed to the petition, grinders and all.

It was broadly attested that questions were put to students by the Court of Examiners, the only tendency of which was to puzzle and embarrass them; and that it was impossible to say, whatever might be a man's attainments, whether he would pass or not. How does this square with the announcements put forth respecting the establishments of the different teachers? One gentleman says "he holds himself responsible for the success of his pupils," and even the chairman himself modestly tells the public that "the unequalled success which has attended the examination of his (the chairman's) pupils at the above institutions (the College and Apothecaries' Hall), is the best criterion of the efficiency of his mode of instruction." Is this a puff, or is it true?—and if true, the charges of tyranny, corruption, and private pique, with which his speech of Monday was beautifully interlarded, must be something which I care not to name. And then, can it be believed that the medical profession has submitted to such a state of things as the diseased imaginations of the reforming orators depicted on Monday evening, for full twenty years? Impossible.

Last, though not least, a word or two of the lion of the evening, who was evidently kept back lest the young men should have scampered off before the business was done, leaving the self-elected committee-men "alone in their glory."

Mr. Wakley set out by assuring them, that if ever he spent a happy evening it was *that* evening. Perhaps so; but was he ever happy a whole evening? Can a man of his temperament be happy? But letting that pass, what was there to make him happy? Much coarse vituperation—many gross charges made against gentlemen who were absent, and who had no business there, not being medical students; which charges were unsupported by a tittle of evidence. This made the man happy: to be sure, there were the howling and hissing, and clamouring down of all who dared to oppose the chairman's views; added to which, the young gentlemen had begun to burn, not "their midnight oil," so pathetically alluded to by the speaker, but their cigars! With all this, Mr. Wakley was happy, and thereupon began his oration. First, he bepraised himself and his twelve years' exertions, having, as he said, "persevered through much calumny and falsehood," which nobody will deny, and really, had nature blessed him with pathos, he would have had us believe that he had sacrificed great things for the sake of the profession. What insufferable cant is all this! He knows very well that the Lancet pays better than pills and boluses; hence the secret of his services.

Next he began to bespatter the youngsters in a style somewhat after the fashion of Lord Brougham's northern speeches, in which he bedaubed the King. Any thing so fulsome as these eulogiums upon medical students I never heard, and it was easy to see that the man was not in earnest. He made them the very patterns of care, perseverence, toil, self-denial, and martyrdom! Such an announcement from the Lancet hero contrasted strangely with the rakish faces, the gossip, and the smoking cigars, of many around me. Mr. Wakley subsequently "went upon another tack," and there he was at home and *happy!* I cannot follow him through his misrepresentations and low abuse, but I could not but be struck with the effect of one observation. All along it had been assumed that public examinations would set every thing to rights. In unison with this opinion, Mr. Wakley said, "get your new university, gentlemen—your public

examinations, and then let them put such questions to you, and one simultaneous *hiss* from 1500 voices will teach them better." "Hark ye, Harry," exclaimed one wag; "fifteen hundred spectators! that's worse and worse; better be as we are, than have so many folks to jeer us; that's no go, Harry." There seemed to be a great number of the same opinion.

Seriously, sir, what do these gentlemen want? The discontent is not with the students so much as with the grinders. Do the latter covet to be Examiners, or do they not rather want to be both teachers and examiners? Let matters come to this, and they would soon earn their thirty guineas—soon polish up their students.

I am, sir,
Your most obedient servant,
J. B.

London, Jan. 20, 1836.

ST. THOMAS'S HOSPITAL.

MEETING OF STUDENTS.

At a public meeting of the students of St. Thomas's Hospital, held on the 26th January, William Wegg, Esq., in the chair, the following resolutions were proposed, seconded, and carried unanimously:—

1. That the Court of Examiners of Apothecaries' Hall, from the period it was first instituted, and invested with the powers it now possesses, has by the gradual and progressive development of its rules and regulations, thereby exacting from students a certain quantity of useful and practical information, contributed much to the advancement of the medical profession, and, as a necessary consequence, been attended with great good to the public at large.

2. That it has not yet been proved that the Court of Examiners of the Apothecaries' Company have in any case evinced partiality towards, or unfairly rejected, any candidate for the license.

3. That we disclaim all participation in the proceedings of the meeting of medical students, held at the Crown and Anchor Tavern, on Monday, January 18, and in many of the sentiments there expressed; but more especially in the unjust and unwarrantable abuse directed against the members of the Society of Apothecaries.

4. That this meeting regrets there does not exist a second examination, and that second examination a public one, to which any rejected candidate might appeal, as this would at once silence the murmurs of incompetency, and destroy the possibility of private injustice. That this meeting also strongly disapproves the principle of self-election and irresponsibility.

(Signed) W. Wegg, Chairman,
Chas. Edw. Blair, Secretary.

NEW PHARMACOPŒIA.

We are happy to be able to state that there is at length a prospect of the new Pharmacopœia appearing without further delay. We have been informed that the manuscript is completed, and that a portion of it is already printed. All the new remedies of established character have been introduced, and the nomenclature adapted to the discoveries of modern chemistry.

PROFESSORSHIP OF MATERIA MEDICA, KING'S COLLEGE.

Dr. Paris, so recently elected to the above chair, unexpectedly resigned it again about a fortnight ago. Dr. Webster, who has been lecturing *pro tempore*, was requested to give the Spring course, but declined to do so unless the professorship was guaranteed to him, which arrangement not being acceded to, he retired. An application was then made to Dr. Gregory, by whom the ensuing course of lectures is to be given, the chair being left open to the candidate who shall be deemed most eligible.

NEW MEDICAL WORKS.

Rudiments of Physiology. By Dr. Fletcher. Part II. double. 10s.

Case of Loss of the Uterus and its Appendages soon after Delivery. By J. C. Cooke. 8vo. 2s.

A Popular Manual of the Art of Preserving Health. By J. B. Davis. Post 8vo., 10s. cloth.

WEEKLY ACCOUNT OF BURIALS.

From Bills of Mortality, *Jan.* 26, 1836.

Abscess	1	Inflammation	18
Age and Debility	51	Bowels & Stomach	3
Apoplexy	4	Brain	4
Asthma	40	Lungs and Pleura	5
Cancer	2	Jaundice	1
Childbirth	3	Liver, diseased	1
Consumption	61	Measles	4
Convulsions	30	Mortification	2
Croup	2	Paralysis	4
Dentition or Teething	5	Rheumatism	1
Dropsy	19	Small-pox	15
Dropsy on the Brain	15	Sore Throat and Quinsey	2
Dropsy on the Chest	1	Thrush	1
Erysipelas	3	Tumor	5
Fever	7	Venereal	2
Fever, Scarlet	4	Worms	1
Fever, Typhus	2	Unknown Causes	40
Gout	1		
Heart, diseased	3		
Hooping Cough	6	Stillborn	19

Decrease of Burials, as compared with the preceding week . . 80

NOTICES.

Professor Lizars' communication in our next.

"Vindex" should have authenticated his letter: so should "An Observer."

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 6, 1836.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XIX.

I PROCEED now to the examination of those medicinal agents obtained by the decomposition of animal substances, and first of

HYDROCYANIC ACID.

History.—At the commencement of the 18th century the substance called *Prussian blue* was accidentally discovered, and various conjectures were soon offered regarding its nature. In 1746, Dr. Brown Langrish employed laurel-water medicinally. In 1752, Macquer announced that Prussian blue was a compound of oxide of iron, and some colouring principle which he could not isolate; and in 1772, Guyton Morveau concluded this principle was of an acid nature. Scheele, in 1782, removed some of the mystery connected with Prussian blue, by obtaining *hydrous prussic acid* from it, which in five years afterwards Berthollet ascertained to be a compound of carbon, nitrogen, and hydrogen. In 1800, and 1802, Bohn and Schrader discovered prussic acid in laurel-water. Borda, Brugnatelli, and Rasori, first employed the acid in medicine, from 1801 to 1806. In 1815, Gay-Lussac obtained the acid in its pure *anhydrous* state, and explained its composition.

Synonymes and Etymology.—It has been denominated prussic *(acidum borussicum)*, zootic, and hydrocyanic acid: the first name indicates the substance (Prussian blue) from which it was obtained, the second refers to its animal origin, and the third indicates its constituents, hydrogen and cyanogen (so called from κύανος, blue; and γενναω, to produce; because it is one of the constituents of Prussian blue).

Native state.—Hydrocyanic acid is never found native in the inorganic kingdom, (the mineral called, by Haüy, *fer azuré*, by others *native Prussian blue*, is a subsesqui-phosphate of iron); it is, therefore, invariably an organic product. It is readily obtained both from many vegetable and animal substances, but it is not easy to say whether it exists in them already formed, or is the result of the process by which it is procured. Two families of the vegetable division of the organic kingdom have long been remarkable for containing it,—namely, *Amygdaleæ*, and *Pomaceæ*. Thus it has been said to exist in bitter almonds, in apple-pips, in the kernels of the peach, apricot, cherry, plum, damson, &c.; in the flowers of the peach, sloe, and mountain ash; in the leaves of the peach, cherry laurel, and bird cherry; in the bark of the latter, and in the bark and root of the mountain ash. These, however, are not the only vegetables in which the existence of hydrocyanic acid has been asserted: in *Rhamneæ* it is said to have been found in the bark of one species, namely, the rhamnus frangula; and lastly in *Gramineæ*; for we are told by some that the ergot of rye contains it, the truth of which assertion, however, has been denied by others. That from most of the substances now mentioned hydrocyanic acid may be readily obtained, is well known, but in some of the instances it appears the acid does not exist ready formed in the vegetable, but is a product of the process by which it is obtained. This has been particularly observed in reference to the bitter almond, and has been inferred with respect to several others. At present, however, I can only just allude to the circumstance, and hereafter, when speaking of vegetable substances, I shall more particularly explain the grounds for this belief.

Hydrocyanic acid does not appear to be

a natural product of animals: yet Prussian blue has been found in the urine, in the menstrual fluid, and in the sweat, and as this substance contains one of the ingredient of hydrocyanic acid (cyanogen), it is not improbable that under particular circumstances this acid might be formed. We are told that when cheese is exposed to the action of water and the sun, it disengages an ammoniacal odour, and in this state, if treated by alcohol, yields traces of hydrocyanic acid.

Preparation.—It is not necessary, I conceive, in pharmacological lectures, to enter into a detailed account of all the processes by which hydrocyanic acid has or may be procured: it will be sufficient to point out those which are considered the best. Directly or indirectly the acid of commerce is always obtained from the ferrocyanuret of potassium; and I shall, therefore, briefly allude to the mode of procuring this substance.

1. *Production of Ferrocyanuret of Potassium.*—The usual method of obtaining this salt is as follows:—Animal matters (such as blood, hoofs, horns, shavings of leather, &c.) are calcined with pearlash in an egg-shaped iron pot, being well stirred all the time with a flat iron paddle. When the fœtid vapours have ceased, the pasty mass is thrown into water, and the clear solution evaporated. To purify the crystals thus obtained, they are re-dissolved in water, and the solution allowed to cool very slowly. The *theory* of the process is not completely understood: the following will serve to explain the production of the salt:—The heat decomposes the animal matters, part of the carbon and nitrogen of which are attracted by the potassium of the pearlash, and thus cyanuret of potassium is produced. A cyanuret of iron is also formed by some of the carbon and nitrogen uniting with the iron of the pot and of the stirrer. By the union of the cyanuret of potassium with the cyanuret of iron, the ferrocyanuret of potassium is formed.

Mr. Lowe has taken out a patent for procuring pure Prussian blue from the ammoniacal liquor of gas works, in which is found hydrocyanate of ammonia. From the lime-water with which the gas has been purified, and which was formerly allowed to run to waste into the river Thames, we may also procure Prussian blue, but mixed with sulphate of lime. The process, in either case, is to add sulphate of iron to the liquor, and then supersaturate with sulphuric acid: Prussian blue precipitates. As the ferrocyanuret of potassium may be readily obtained by boiling Prussian blue with potash, we may expect ere long to derive all our ferrocyanuret of potassium in this way, and at a cheaper rate.

Composition of Ferrocyanuret of Potassium.

Elementary.		Secondary.	
6 Carbon	36	2 Cyanuret potassium	132
3 Nitrogen	42	1 Cyanuret iron	54
2 Potassium	80	3 Water	27
1 Iron	28	1 Crystallized ferrocyanuret	213
1 Anhydrous ferrocyanuret	186		
3 Water	27		
1 Crystallized ferrocyanuret	213		

The formula for this salt in the anhydrous state, is this:—2 (K Cy) + Fe Cy.

2. *Production of hydrocyanic acid from ferrocyanuret of potassium.*—*Giese's process:*—Dissolve 5 drachms of pulverized ferrocyanuret of potassium in 20 drachms of water; then add 14½ drachms of dilute sulphuric acid, (composed of 10 drachms of water, and 4½ drachms of acid) and distil until the materials become thick or pasty. About 30 drachms of distilled acid are obtained, (or if the quantity procured be less than this, water is to be added to make up the amount) which contain about 3·3 per cent. of real or anhydrous hydrocyanic acid. The process may be performed in a tubulated retort and receiver, employing an adopter; or a large matrass may be used in the place of the retort. For the above quantities I have employed two Florence flasks, one for the receiver, the other for the distilling vessel, connecting them by a glass tube curved twice at right angles. The receiver must be kept very cool, ice or snow being used if they can be procured; and the heat employed to distil the acid should be very moderate. The distilled liquor usually contains a little sulphuric acid, and by standing deposits a small portion of Prussian blue. By a second distillation both these may be readily separated. This process is by far the cheapest, and yields a very constant and permanent product. **Medicinal acid**

may be sold at one penny per ounce, yielding more than a half-penny profit to the manufacturer. The small portion of sulphuric acid which distils over is supposed to contribute to its preservation.

When the dilute sulphuric acid is added to the solution of ferrocyanuret of potassium, water is decomposed: the hydrogen of which, uniting with some cyanogen of the ferrocyanuret, constitutes hydrocyanic acid; while the oxygen of the water, with some of the potassium, generates potash, which, with the sulphuric acid, forms a sulphate of potash; and at the same time a new insoluble salt is formed, composed of cyanogen, iron, and potassium.

Thus far the account which I have given of the decomposition is undoubtedly correct; but if we investigate the phenomena a little more minutely, we are involved in doubt and uncertainty. Gay-Lussac states this new salt is white, and that it consists 9 atoms cyanogen, 7 atoms iron, and 2 atoms potassium. But it appears to me he has deduced the composition from theoretical rather than experimental considerations, and consequently his results are not to be relied on. Mr. Everett, on the other hand, tells us the new salt is of a light lemon yellow colour, and that its composition is 3 atoms cyanogen, 2 atoms iron, and 1 atom potassium: these proportions he has determined from numerous carefully conducted experiments, so that I have no hesitation in adopting his results, except in regard to the colour of the salt. By the following contrivance, I managed to act on the solution of ferrocyanuret of potassium by the dilute acid to the perfect exclusion of every particle of air, and found the salt to be *white*, as described by Gay-Lussac; but I also observed that the application of heat gave rise to the formation of a little Prussian blue. A retort was filled with the solution of the ferrocyanuret, and a curved tube filled with distilled water introduced into the retort (while the beak was under water) as far as the bulb: the dilute acid was then poured down the long leg of the tube (which was a foot above the surface of the water). On the application of heat, a copious *white* precipitate took place; and I observed that to whatever part of the bulb the heat was applied, streaks of Prussian blue were immediately apparent. No yellow or green colour was observed.

The following will explain the results, according to Mr. Everett:—

Reagents.

2 Atoms crystallized ferrocyanuret of potassium	426
6 Atoms anhydrous sulphuric acid	240
	666

Results.

3 Atoms hydrocyanic acid		81
3 Atoms bisulphate potash		384
3 Atoms water		27
New salt (*bi-ferrocyanuret of potassium*)	1 atom cyanuret potassium	66
	2 atoms cyanuret iron	108
		666

The symbolic representation of these results is as follows:—

Reagents.	Results.
2 (2KCy + Fe Cy)	3 (H + Cy)
6 $\ddot{S}$	3 ($\dot{K}$ + 2$\dddot{S}$)
	3 $\dot{H}$
	KCy + 2 (Fe Cy).

The salt which I have here called *bi-ferrocyanuret of potassium* by exposure to air generates Prussian blue.

Production of hydrocyanic acid from bi-cyanuret of mercury.—At Apothecaries' Hall, hydrocyanic acid is prepared from one part bi-cyanuret of mercury, one part hydrochloric acid (sp. gr. 1·15), and six parts water. The mixture is to be distilled until six parts pass over. The acid thus obtained has a sp. gr. of 0·995; and Mr. Hennell tells me the standard strength of it is such, that two fluid drachms dissolve 14 grains of the red oxide of mercury. The per centage strength, therefore, would be about 2·9.

The *theory* of the process is very simple: the chlorine of the hydrochloric acid forms corrosive sublimate with the mercury of the bi-cyanuret; while the hydrogen with the cyanogen produces hydrocyanic acid.

The symbolic representation of the decomposition is as follows:—

Reagents.	Results.
Hg + 2 Cy	2 (H + Cy)
2 (H + Chl.)	2 Hg + Chl.

This process offers no advantages over the one before mentioned, while it has the objection of being more troublesome and expensive; since we have to procure, in the first instance, bi-cyanuret of mercury. This salt is sometimes procured by boiling in water Prussian blue with the nitric oxide of mercury: but it is more cheaply produced by saturating hydrocyanic acid (obtained by Giese's process) with the nitric oxide of mercury, while the resulting salt is much purer and whiter.

Production of hydrocyanic acid from cyanuret of silver. — This is the process proposed by Mr. Everett, and I think is calculated to yield an acid of uniform strength. "Into a phial capable of holding rather more than one fluid ounce, put forty grains of the cyanuret; add seven fluid drachms twenty minims of water, and forty minims of dilute muriatic acid (sp. gr. 1·129); cork closely, shake

several times for the first quarter of an hour, set aside to allow the chloride of silver to fall, decant the clear liquid into another bottle to be preserved for use. Every fluid drachm will contain one grain of real hydrocyanic acid."

The *theory* of the process is this:—The chlorine of the muriatic acid forms, with the silver of the cyanuret, chloride of silver, while the hydrogen, with the disengaged cyanogen, generates hydrocyanic acid. Cyanuret of silver is procured by adding hydrocyanic acid to nitrate of silver: water (holding in solution nitric acid) and cyanuret of silver are the results. The symbolic representation of the decomposition is as follows:—

Re-agents.	Results.
Ag + Cy	H + Cy
H + Chl.	Ag + Chl.

The advantage of this process is its yielding a product of uniform strength; its objections, the trouble and expense. Mr. Everett says that practitioners could obtain an ounce of acid by this process for one shilling, while the manufacturer of the cyanuret could obtain 50 per cent. profit by it. It appears to me, that if the strength of medicinal hydrocyanic acid were fixed, this process would be quite unnecessary; since manufacturers can readily ascertain the strength of their product, and modify it, to meet the directions of the College, with very little trouble.

Production of hydrocyanic acid from cyanuret of potassium. — This process was originally proposed by Dr. Clark, and has been adopted by Mr. Laming. The formula of the latter is the following, 22 grains of cyanuret of potassium, 50 grains of crystallized tartaric acid, 6 fluid drachms of water, and rectified spirit of wine 3 fluid drachms. One fluid drachm of the decanted clear liquor contains 1 grain of pure hydrocyanic acid.

The *theory* of this process is as follows: part of the water is decomposed, the oxygen of which unites with the potassium, forming potash, which combines with the tartaric acid to form bitartrate of potash; the greater part of which precipitates: the hydrogen of the water, with cyanogen, forms hydrocyanic acid.

The symbolic representation of this is as follows:—

Re-agents.	Results.
K + Cy	H + Cy
$\dot{H}$	$\dot{K} + 2\,\bar{T}$
$2\,\bar{T}$	

The professed advantage of this process is the uniformity of the strength of the acid. In this respect, however, I think it inferior to Mr. Everett's, since the cyanuret of potassium is deliquescent and liable to spontaneous decomposition, proved by its hydrocyanic odour. Another objection is the trouble and expense of procuring pure cyanuret of potassium. It is but justice to Mr. Laming to state that his cyanuret of potassium is very pure, and is sold at a moderate price.

Strength of medicinal hydrocyanic acid.—It is to be hoped that the new Pharmacopœia now in preparation will give a fixed standard of strength of this acid. That this is much needed must be obvious to every one: I cannot bring forward any accidents which have happened for want of it in this country, yet danger is to be apprehended from the varying strength of the acid found in the shops. The following dreadful occurrence at one of the Parisian hospitals in June, 1828, will show the importance of what I have now stated:—In the shops of Paris a syrup of hydrocyanic acid is kept, composed, according to the formula of Magendie, of one part of medicinal acid to 129 parts of syrup; while in the Hôpital Bicêtre the syrup was prepared according to the codex—that is, one part of an acid (called Scheele's) to nine of syrup. A physician to the hospital, who had been accustomed to prescribe the former in private practice, ordered his usual dose to be given to seven hospital patients, not knowing that the hospital preparation was of a different strength. Half an ounce was accordingly administered to them successively: the result was fatal to the whole; all were dead within forty-five minutes.

Two methods have been proposed for determining the strength of the acid,—one by observing how much of the red oxide of mercury is dissolved by it, and dividing the quantity by 4. Thus: suppose 100 grains of dilute acid dissolve 10 grains of the oxide, the quantity of real acid in the 100 grains would be $\frac{10}{4}=2{\cdot}5$. The other, and best method, is by precipitating the acid by nitrate of silver. The mode of proceeding is as follows:—weigh carefully in a small phial 100 grains of the hydrocyanic acid, to be examined; to this add a solution of nitrate of silver, until precipitation no longer takes place. There can be no harm in using an excess of the nitrate, but a fallacy if a deficiency be allowed. Collect the precipitated white cyanuret of silver, and dry it. When completely dry, weigh it: every 100 grains are equal to 20·9 of real hydrocyanic acid. For example: suppose the dried cyanuret weigh $13\frac{1}{2}$ grains, by the Rule of Three you easily ascertain the quantity of real acid in the 100 grains of liquid acid examined; for every $131\frac{39}{100}$ grains of cyanuret have been formed from 27·39 parts of hydrocyanic acid,—therefore, you state the question as follows:—

$$\text{If } 134{\cdot}39 : 27{\cdot}39 :: 13{\cdot}5$$

By working this, you will find the result to be 2·007. Hence you say that the 100 grains of liquid acid were composed of—

Real acid	2·007
Water	97·993
	100·000

It must be recollected, that if muriatic acid be present, the result will be fallacious, since chloride, as well as cyanuret of silver, would be formed. The proper mode of proceeding, then, would be to distil the suspected acid from marble (which retains the muriatic acid), collect the product, and test its strength as above mentioned.

In the Dublin Journal for November, 1835, Dr. Geoghegan has proposed, for the detection of foreign acids in medicinal hydrocyanic acid, a salt which we may denominate the *Hydrargyro-cyanuret of the Iodide of Potassium.* If a concentrated solution of the cyanuret of mercury be mixed with a solution of iodide of potassium, a precipitate takes place consisting of white or pearly crystalline plates; these are the crystals of the salt in question. Now, if you take a few of these upon a rod and place them in a glass, adding the hydrocyanic acid you are about to examine, no change is observed if the acid be pure, but if a drop of muriatic, sulphuric, or any other acid, be present, a red (or a yellow, becoming red) precipitate of the bin-iodide of mercury is observed. I have tried this test two or three times, and it appeared to me to answer the purpose very well.

The medicinal hydrocyanic acid sold in the shops as being of Scheele's strength, varies considerably. Mr. Everett states that samples from Allen, Hanbury, and Co. yielded 5·8 per cent.; from Apothecaries' Hall, at different times, from 2·1 to 2·6 per cent.; and from several sources, only 1·4 per cent. You will naturally ask, what ought to be the strength? It is difficult to answer this question, since Scheele's process does not give a uniform result. The standard strength at Apothecaries' Hall, according to the statement of Mr. Hennell, already referred to, is about 2·9. Magendie states, that the *pharmaciens* of Paris imitate it by mixing 40 parts of water with 1 of pure acid, which give the per centage strength 2·5. As prepared by the Dublin College, its strength is about 1·6. That obtained by Giese's process should be 3·3 per cent.; Mr. Laming's acid is 1·66.

Preservation of the medicinal acid.—Medicinal hydrocyanic acid, free from all impurities, soon decomposes; but when mixed with a little sulphuric, or even muriatic acid, it is a much more permanent compound. It is supposed that on this account the acid prepared by Giese's process will keep unaltered for years, owing to the presence of some sulphuric acid. Dr. Christison has had some unchanged for two years and a half, though exposed to day-light. Mr. Barry adds a little muriatic acid to all his medicinal acid, to preserve it.

Hydrocyanic acid should be kept in a well-stoppered bottle, excluded from light; for both air and light hasten its decomposition, though they are not essentially necessary to it. Sometimes the decomposition is marked by a change of colour, but at other times the liquid remains perfectly colourless.

Properties and composition of anhydrous hydrocyanic acid. — Anhydrous hydrocyanic acid is a solid at 0° F. (some state at 5° F.), having then the appearance of crystallized nitrate of ammonia: it readily melts, forming a limpid, colourless liquid, having an intense and peculiar odour; its taste is at first cool, then hot; at 45° its sp. gr. is 0·7058, and at $64\frac{1}{2}$ is 0·6969. In this state it is exceedingly volatile: a drop placed on glass freezes by its own evaporation. It unites with water and alcohol in every proportion. At 79° or 80° F. it boils, forming hydrocyanic acid vapour, which is combustible; and when mixed with oxygen, explodes, yielding carbonic acid, nitrogen, and water. This, therefore, is one method of analyzing it.

Before Combustion.

H Cy *vap.* =27	O =16	O =16	O =8

After Combustion.

$\ddot{C}$ =22	$\ddot{C}$ =22	N =14	& H =9

Inferences.

H Cy *vap.* =27	contains	C *vap.* =6	C *vap.* =6	N =14	H =1	or	Cy =26	H =1

You observe that 2 volumes of hydrocyanic acid vapour, mixed with $2\frac{1}{2}$ volumes of oxygen gas, and exploded by the electric spark, yield 2 volumes carbonic acid, 1 volume nitrogen, and 1 atom of water. Now the carbonic acid and the water contain the oxygen employed; and hence we infer that the two volumes of hydrocyanic acid gas contain 2 volumes or atoms of carbon vapour (found in the carbonic acid), 1 volume of nitrogen, and 1 of hydrogen (in the water). So that the combining proportion by volume, or the atomic volume, of hydrocyanic acid, is to that of hydrogen as 2 is to 1.

The composition of hydrocyanic acid may be thus stated:—

2 carbon	12	or	1 cyanogen	26
1 nitrogen	14		1 hydrogen	1
1 hydrogen	1			
	27			27

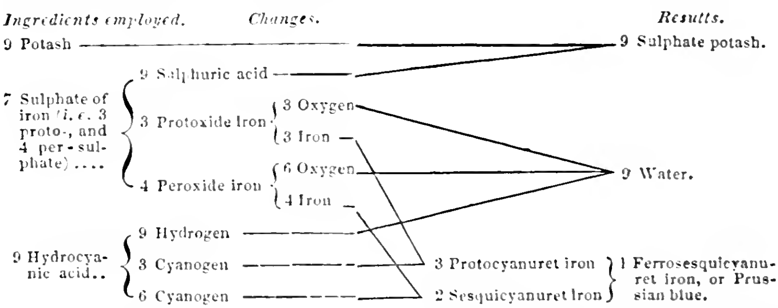
In a few hours pure hydrocyanic acid usually decomposes, and becomes brown; but Dr. Christison says he has kept it unchanged for a fortnight in ice cold water.

Properties of the hydrous hydrocyanic acid.—In this form it is a colourless, transparent liquid, having the taste and smell of the pure acid, but of course in a minor degree. Unless it be very dilute, you may obtain evidences of the combustibility of its vapour thus: heat it in a tube with a narrow mouth,—the vapour which is evolved will be found to burn on the application of a lighted taper. By keeping, it undergoes decomposition, as already mentioned.

Tests.—Hydrocyanic acid may be recognized by the following characters:—

1. The remarkable and well known *odour*. This is said by Orfila to be the most delicate of all methods of recognizing it, since it is very marked when the liquid tests give very slight indications.

2. *Salts of iron.* Add a little potash to the suspected acid; then a solution of some proto and per-salt of iron; the common sulphate of iron of the shops will answer, or the tincture of the muriate, since each of these preparations usually contains both a per and a proto salt. A precipitate is thus obtained, which is liable to considerable variation in its colour, and depending on the quantity of potash, and the kind of ferruginous salt employed; it may be yellowish brown, or greenish, or bluish. Then add dilute sulphuric or muriatic acid, and Prussian blue will appear if hydrocyanic acid were present. The *theory* of this test will be best understood by a diagram; but I may remark that the potash is added to form hydrocyanate of potash, which is decomposed by the ferruginous salt, forming a salt of potash, water, and two cyanurets of iron, which, by their union, form Prussian blue.



Respecting this diagram I have to remark, that all the figures refer to the number of atoms, or proportionals, reacting. You will observe also, that the 7 atoms of mixed sulphate contain 9 atoms of sulphuric acid; for each of the 3 atoms of protosulphate contain 1 sulphuric acid, while each of the 4 atoms of per-sulphate contain $1\frac{1}{2}$ sulphuric acid. I have taken no account here of the dilute muriatic or sulphuric acid used in the testing; in fact, they are only employed to counteract an excess of potash, and are, therefore, not necessary when the proper quantity (which I have assumed in the diagram) of potash has been employed.

3. *Nitrate of silver* is frequently a very useful test; it throws down a white precipitate of the cyanuret of silver, which is distinguished from the chloride by two characters, namely, its solubility in boiling nitric acid (by which nitrate of silver and free hydrocyanic acid are formed), and its evolution of cyanogen gas when heated, known by its burning with a violet or reddish flame. Nitrate of silver is exceedingly valuable as a *negative* test; that is, if no precipitate be obtained on the addition of this salt to a liquid, we may safely conclude the absence of hydrocyanic acid; but if a precipitate take place, we are not, therefore, to infer the

presence of the acid, since many other substances may produce similar precipitates. The *theory* of the action of the test is simple: free nitric acid, water, and cyanuret of silver, are the results. The interchange of elements will be comprehended by these symbols:—

Re-agents.	Results.
$H + Cy$	$\dot{H}$
	$\overset{\cdot\therefore\cdot}{N}$
$\dot{Ag} + \overset{\cdot\therefore\cdot}{N}$	$Ag + Cy$

4. *Sulphate of copper* has been much praised as a test for hydrocyanic acid, but will not bear a moment's comparison with the salts of iron. The mode of applying it is this:—Supersaturate the suspected liquor with potash; then add a solution of sulphate of copper. A greenish blue precipitate is obtained, which, by the addition of a few drops of muriatic acid, becomes (if hydrocyanic acid be present) white. The *theory* of the test is this: by the mutual action of the hydrocyanic acid, excess of potash, and sulphate of copper, we obtain sulphate of potash, oxide of copper, and cyanuret of copper. The formation of these will be understood from the following symbols:—

Re-agents.	Results.
$H + Cy$	$\dot{H}$
	$2\,(\dot{K} + \dddot{S})$
$2\ \dot{K}$	$\dot{Cu}$
$2\,(\dot{Cu} + \dddot{S})$	$Cu + Cy$

To separate the oxide (which gives the greenish appearance to the precipitate), a few drops of muriatic acid are employed: the undissolved cyanuret is white. The objections to the test are, the result not being sufficiently striking, and that a person unaccustomed to manipulation may fail in obtaining the characteristic appearances, by employing either too much muriatic acid (which will dissolve the cyanuret), or too little potash, when no cyanuret is produced.

Physiological effects.—Let us take a general review of the operation of this acid, first on plants, secondly on animals generally, and thirdly on man.

1. *Effects on plants.*—Hydrocyanic acid appears to act as a poison to all plants on which it has hitherto been tried. The phenomena observed are, sometimes a change in the colour of the leaves and flowers, and a diminished contractile power, or at least diminished sensibility to external impressions. Thus if we immerse a stem of *Berberis vulgaris* in dilute hydrocyanic acid, the stamina are no longer capable of moving when irritated. If, instead of the barberry, we employ a stem of the sensitive plant (*Mimosa pudica*), the leaves become insensible to external irritation. Plunging a seed in the dilute acid effectually stops germination. In those parts of a plant poisoned by the acid, the milky juice does not flow from the cells or vessels containing it. By chemical means it has been shewn that the acid becomes absorbed. Ammonia has in some cases appeared to favour the recovery of plants which had been exposed to the vapour of the acid. We shall find that all these phenomena are precisely analogous to those which are produced by the action of the acid on animals.

2. *Effects on animals generally.*—Hydrocyanic acid appears to be an energetic poison to all classes of animals. It is hardly necessary to refer to the effects of individual experiments in proof of this statement; it will be sufficient to add that its action has been tried on the following classes:—

(*a*) *Mammalia*, as man, dogs, horses, rabbits, hedgehogs, moles, cats, mice, rats, porpoises, bears, wolves, lambs, &c.

(*b*) *Aves*, as sparrows, ravens, common fowls, sparrow-hawks, owls, linnets, finches, starlings, magpies, vultures, falcons, &c.

(*c*) *Reptilia*, as lizards.

(*d*) *Amphibia*, as frogs.

(*e*) *Gasteropoda*, as snails.

(*f*) *Annelida*, as leeches.

(*g*) *Crustacea*, as crabs and lobsters.

(*h*) *Insecta*, as flies, bees, wasps, and the stag-beetle.

(*i*) *Polygastrica*, as the vibrio aceti.

The general effects were very similar in all cases, but its action on the cold-blooded animals was much slower, and less evident, than on the warm-blooded. I have given a frog as much acid as I believe would have been sufficient to poison a man, but without any obvious effects. If the heart of a frog be immersed in dilute hydrocyanic acid, it will continue to beat for several minutes.

3. *Effects on man.*—I propose to examine the effects of different doses under three heads:—

(*a*) *Effects of medicinal doses.*—The effects which are sometimes observed resulting from medicinal doses are these: bitter and peculiar taste; increased secretion of saliva; irritation in the throat; frequently nausea; disordered and laborious respiration (which is sometimes quickened, at other times slow and deep); pain in the

head, giddiness, obscured vision, and sleepiness. The vascular system is in some cases not obviously affected, but in others much disordered, though the kind of disorder is not uniform; sometimes the pulse is quickened, at others reduced in frequency. In some instances faintness is experienced. Drs. Macleod and Granville have noticed salivation and ulceration of the mouth during its medicinal use.

(*b*) *Effects of poisonous doses: when fatal, causing slow death.*—Immediately after swallowing the acid a remarkable bitter taste is experienced; this is soon followed by a sensation of faintness and giddiness, with salivation, and succeeded by tetanic convulsions and insensibility; respiration difficult and spasmodic; odour of hydrocyanic acid in the breath; pupils usually dilated, sometimes contracted; pulse small or imperceptible.

When recovery takes place it is usually very rapid, and the whole period of suffering seldom exceeds half an hour. However exceptions, to this exist, in which the symptoms have been prolonged for several hours.

The following case, related by Dr. Geoghegan, is an interesting illustration of these effects:—A gentleman, aged 21, having been for some time subject to an uneasiness in the stomach, not actually amounting to gastrodynia, after having tried many remedies in vain, was induced to have recourse to hydrocyanic acid. He commenced with one minim of the Dublin Pharmacopœia, sp. gr. 0·998: this dose he repeated twelve times the first day, without any perceptible effect. On the following day he took half a drachm, with the same result. The third day his dose was a drachm, which he repeated the fourth day. On the fifth day he took a drachm and a half, still no effect of any kind. On the sixth day he increased his dose to two drachms. In about two minutes after taking this latter quantity, he experienced a sensation of extreme bitterness in the mouth, and having walked a few paces, was affected with great confusion, headache, and loud ringing in his ears. He now with difficulty retraced his steps, and leaning forward on a table became insensible and fell backwards. In this state he remained altogether between three and four minutes, during which time he was violently convulsed. Two drachms of the spiritus ammoniæ aromaticus were diluted with a little water, and applied as quickly as possible to the mouth, but as the teeth were clenched it could not be swallowed. The solid sesqui-carbonate of ammonia was then applied assiduously to the nostrils; its beneficial effects were soon apparent, and he was shortly able to swallow a little fluid. Sensibility now speedily returned, and vomiting supervened, from which he experienced great relief; and at the expiration of half an hour he was quite well, with the exception of pain and feeling of distention in the head, which continued for the remainder of the day.

After he had become insensible, and while leaning on the table, his thighs became rigid, and were drawn up on the abdomen; and as he was about falling, he was caught, and placed on the ground. The upper extremities were then observed to be also rigid, and on drawing them from the side they forcibly reverted to their former position; the eyes were shut, the teeth clenched, and the muscles of the face violently convulsed. It is deserving of notice that the old complaint was completely removed by this extraordinary dose.

(*c*) *Effects of poisonous doses: quick death.*—In these cases the death is so speedy that in the human subject the symptoms have hardly been observed. The phenomena are probably similar to those noticed in animals,—that is, imperceptible pulse, breathing not obvious, or there may be two or three deep, hurried inspirations, insensibility, and death. Convulsions may or may not be present. Dr. Christison states that twenty-five grains of the strong acid, applied to the mouth, killed a rabbit within ten seconds. I once caused the instantaneous death of a rabbit by applying its nose to a receiver filled with the vapour of the pure acid: the animal was killed, without the least struggle. If a drop of the pure acid be placed on the throat of a dog, or applied to the eye, death takes place in a few seconds. Inhaling the vapour decidedly produces death more quickly than any other mode of applying the acid. The presence or absence of convulsions, as connected with the time within which death occurs in these cases, is sometimes of great moment. Some years ago the life of a prisoner almost turned on this point. The following is an outline of the case, which you will find more fully developed in Dr. Christison's work:—An apothecary's maid-servant, at Leicester, was found one morning dead in bed. The body lay in a composed posture—the arms crossed over the trunk, and the bed-clothes pulled smoothly up to the chin. At her right side lay a phial, from which about five drachms of the medicinal hydrocyanic acid had been taken, and which was corked and wrapped in paper. It was suspected that she took the acid to occasion miscarriage, and that the apprentice was accessory to its administration, in consequence of which he was put on his trial. Now here was the question:—Could the deceased, after having drunk the poison, have

time to cork the phial, wrap it up, and adjust the bedclothes before insensibility came on? It was supposed that if the death were of that slow description to allow of these acts of volition, convulsions would have occurred and the bedclothes would have been found disordered. On the other hand, those cases in which no convulsions occur usually terminate too quickly to allow of the above acts.—Judging from my own observation of the effects of this acid on animals I should say these supposed acts of volition were possible, but not probable.

There are two points connected with the action of the acid which are interesting, more particularly in a medico-legal point of view,—namely, the time in which the poison begins to operate, and the time in which it proves fatal. No absolute answer can be given to either of these questions, since the strength and quantity of the acid exhibited, and peculiarities (not known or understood) affect the result. Very strong acid in large doses begins to operate immediately on touching the throat, and death is almost instantaneous. The dilute acid, on the other hand, sometimes does not begin to act for several minutes, and death may not occur for nearly half an hour. Of the seven patients killed in one of the Parisian hospitals, some did not die for forty five minutes. But I have not found the same quantity of the same acid kill in the same time in different individuals of the same species; so that I admit individual peculiarities as affecting the result.

Modus Operandi.—(*a*) *Theory of the local operation.*—The action of hydrocyanic acid on the nerves of the part to which it is applied seems proved from the following fact mentioned by Dr. Christison, that Robiquet's fingers became affected with numbness, which lasted several days, in consequence of having exposed them to the vapour of this acid.

Some symptoms lead to the conclusion that it acts as a local irritant. They are, the acrid impression made by the vapour on the nose and throat, the ptyalism, the vomiting and purging, and the traces of gastro-enteritis sometimes noticed.

(*b*) *Theory of the remote operation.*—That hydrocyanic acid becomes absorbed is proved by its having been detected in the blood, and the whole body having a hydrocyanic odour. But it does not follow that the remote operation is a consequence of this absorption.

The following reasons have been assigned for believing that the remote action of the acid is the result of absorption:—(*a*) Applied to most parts of the body it acts as a poison, but the degree of its energy is in proportion to the absorbing powers of the part. Magendie says a drop mixed with a few drops of alcohol, and injected into the jugular vein, kills the animal instantly, as if he had been struck by lightning. Inhaling the vapour of the pure acid produces almost instant death: it is more active when applied to the serous membranes than to the stomach. Applied to the conjunctiva it acts most energetically, and causes temporary opacity of the cornea, most probably from the cold and consequent condensation, just as pressure produces this effect. When applied to the skin, the epidermis not being removed, its action is usually very slight, in consequence, apparently, of the obstruction this lamina offers to absorption. It appears, that applied to the cut extremities of the nerves, or to a fissure in the spinal marrow or brain, no effect is produced. (*b*) This poison is equally active when applied to the tongue or stomach, after the nerves of these parts have been cut; while, on the other hand, (*c*) the poison is inoperative when applied to any part the circulation of which has been obstructed. The only answer which need be given to all these arguments is, that in many cases death so speedily follows the application of the poison, that it is almost impossible to conceive it to be caused by absorption.

Whether by absorption or by sympathy, its principal action is exerted on the nervous system. The pain in the head, the insensibility, and the coma, seem to prove its operation on the brain, while the tetanic convulsions evince its action on the spinal marrow. The affection of the respiratory and vascular systems is probably the result of its influence over those parts of the spinal marrow which supply these systems with nervous influence.

Dr. Christison has quoted the following case from Wedemeyer to prove the independent action of the acid on the spinal marrow:—"In a dog, the spinal cord was divided at the top of the loins, so that no movement took place when the hind legs were pricked: hydrocyanic acid being then introduced into a wound in the left hind leg, symptoms of poisoning commenced in one minute, and the hind legs were affected with convulsions as well as the fore legs."

Is hydrocyanic acid an accumulative poison? This question is usually decided in the negative, but the correctness of this conclusion is, to say the least of it, doubtful.

It is evident that hydrocyanic acid belongs to that class of medicines which I have ventured to call *cerebro-spinants.* Dr. A. T. Thomson terms it a *sedative.* By some it is classed among *narcotics.*

Lesions of texture.—The post-mortem appearances in cases of poisoning, are the

following:—Glistening state of the eyes, but which, however, is not a constant phenomenon, nor is it peculiar to this poison; the odour of the acid is oftentimes very obvious in the blood, brain, chest, or stomach; from the experiments of Schubarth, it appears to occur principally when death is speedy—that is, within ten minutes after the poison is taken; and *vice versâ*, when life is prolonged to fifteen or thirty minutes, it is unlikely to be met with, since it is so rapidly discharged from the lungs. The venous system is usually gorged with blood, while the arteries are empty: the blood is fluid, dark or bluish black, and viscid or oily. The internal coat of the stomach and rectum, and sometimes also of the air-tubes, is red and easily removed in spots. The vessels of the brain and spinal marrow frequently gorged; sometimes with serous or sanguineous effusion. The lungs in some instances natural, in others gorged with blood.

It has been stated by Magendie, that after death by the strong acid, the muscles are not sensible to the galvanic influence. But this occurs very rarely: indeed I have never observed it, though Dr. Christison has occasionally met with it. Certainly it does not take place when the dilute acid is used; for during several years past, when I have wished to show the influence of the galvanic agency on the muscular fibre, I have always killed animals for the purpose by the dilute acid, and I have never failed in powerfully influencing the voluntary muscles; nor have I once met with a case in which the heart had ceased to beat when the chest was laid open immediately after death.

Cause of death.—In speaking of the cerebro-spinants on a former occasion, I alluded to the immediate cause of death in poisoning by the hydrocyanic acid. In most cases death occurs from the obstructed respiration, as I then endeavoured to show; but in some instances it appears to arise from the sudden stoppage of the heart's action. Sometimes death takes place when we cannot refer it to either of these causes. In the case of the rabbit killed by the vapour of the acid, as already mentioned, death was too speedy to be referred to asphyxia, and on opening the chest I found the heart still beating.

Uses.—We are, I believe, indebted to the Italian physicians for introducing hydrocyanic acid into the list of articles used in therapeutics. In France one of the principal promoters of its use was Magendie, and in this country Drs. Granville and Elliotson. I shall examine its uses under two heads.

(*a*) *Local uses.*—As a local agent hydrocyanic acid has not been very extensively employed. The following are the principal cases in which it has been proposed:—

In *chronic skin diseases*, especially impetigo, prurigo, and psoriasis, it has been recommended by Dr. A. T. Thomson to allay pain and irritation. Schneider, of Dusseldorf, has employed 1½ drachms of hydrocyanic acid, six ounces of spirit, and as much rose water, in scaly diseases attended with severe itching, especially in eruptions upon the genital organs. On several occasions I have tried hydrocyanic washes in prurigo, but without obtaining any obvious relief. Dr. Elliotson says he has found it efficacious in sores behind the ears, and in scabs of the face; and adds, to an irritable face it is very soothing, if employed before and after shaving.

In *cancer of the uterus*, lotions containing this acid have been applied to allay the pain by Frisch, of Nyborg. Osiander has also employed, in the same disease, cherry-laurel water, the active principle of which is this acid.

In *gonorrhœa*, injections containing hydrocyanic acid have been employed with benefit. Schlegel has tried also the cherry-laurel water with the same result. Lastly, the dilute acid has been proposed as an effectual and agreeable mode of destroying vermin.

(*b*) *Remote uses.*—By the Italian writers hydrocyanic acid is regarded as a powerful asthenic or *contra-stimulant*, and therefore peculiarly useful in all diseases attended with excitement. Hence its employment by them in inflammatory affections. Subsequent experience has shown it is of little or no value in these cases. The following are some of the disorders in which it has been recommended:—

In gastro-enteritic affections.—In Dr. Granville's work on Hydrocyanic Acid we are told, "that between the years 1780 and 1796," Hufeland, Haller, Thuessen, Swediaur, Sprengel, and others, used laurel water in stomach complaints, dyspepsia, &c. In 1814, Sprengel, in his Pharmacologia, recommended prussic acid, free or combined, in these affections. In 1819, Mr. (now Dr.) A. T. Thomson detailed a case which led him to infer that this acid would be an important agent in the treatment of dyspeptic complaints: and in 1820, Dr. Elliotson published numerous cases illustrative of the efficacy of the hydrocyanic acid in affections of the stomach. The cases he found relieved by it were—1st, those in which pain at the stomach was the leading symptom; 2dly, those in which the gastrodynia was accompanied by a discharge of fluid, constituting what is called pyrosis, or the water-brash; 3dly, when the excessive irritability of the stomach produces vomiting; and

4thly, those disorders of the stomach which, in some of their symptoms, resemble affections of the heart. I would add to these, analogous affections of the intestines, characterized by pain (constant or periodical), spasm, or purging. I have employed this remedy in numerous cases of painful affections of the stomach and bowels; and though in by far the larger proportion of instances no relief was obtained, yet in some the beneficial effects were most astonishing. I shall not easily forget the case of a gentleman, a relative of one of my pupils. He had suffered for several months excruciating pain in the bowels, commencing daily about two o'clock, and only ceasing at night. It was apparently a consequence of an ague. He had been under the care of several country practitioners, and had tried a number of remedies (including opium and sulphate of quinia) without the least benefit. I advised the employment of the hydrocyanic acid, and accordingly five minims were administered at the commencement of a paroxysm: the remedy acted like a charm: all the unpleasant symptoms immediately disappeared. Several doses of the acid were given before the time of the succeeding paroxysm, but the disease never returned: and after employing the acid for a few days longer he went back to the country completely cured.

Very recently I prescribed the acid for a lady who had suffered for months with gastrodynia, and who was persuaded, from her sensations, she had some organic disease. The remedy acted in the same surprising manner: in a few hours, to the astonishment of herself and friends, this patient was apparently quite well, and she has since had no return of her complaint. As I have before mentioned, in many precisely analogous cases the acid gaves no relief. I know not what are the pathological differences between the successful and unsuccessful cases, and, therefore, the employment of the remedy is to a certain extent empirical. In all the painful affections of the stomach and bowels in which I have employed it, the results have been either the most perfect success, or no obvious effects. I have met with no cases of partial relief.

I have seen hydrocyanic acid given with advantage to allay the vomiting and purging in that kind of cholera which, to distinguish it from the Asiatic or malignant, we now call English or mild. Dr. Prout has found it useful in gastrodynia connected with colica pictonum.

As a remedy in affections of the pulmonary organs, hydrocyanic acid was at one time in great repute. It was said to be capable of curing slight inflammations of the lungs without the necessity of blood-letting: in incipient phthisis, we were told, it either suspended or cured the disorder, while in confirmed cases it smoothed the approach of death; hooping-cough was curable by it; and all the symptoms of spasmodic asthma yielded, we were told, to this acid.

Experience, gentlemen, has shown the value of all these statements. We still bleed in pneumonia: there are now as many victims to phthisis as formerly, despite of prussic acid. Hooping-cough runs its usual course, and spasmodic asthma is little influenced by this energetic poison. Occasionally I have seen the cough and sweating, in phthisis, temporarily relieved, but this is the utmost observed from its employment. Cases of genuine spasmodic asthma are rare, but in two instances in which I have seen the acid employed, no relief was obtained. In allaying cough (especially the kind called spasmodic) I have, on several occasions, found it useful, but having so frequently been disappointed in my expectations, I now rarely employ it in any pulmonary diseases. I have, however, never observed any ill effects arise from its use in these cases, though others have.

It has been employed in *affections of the nervous system*. Cases of hysteria, epilepsy, chorea, and tetanus, have been published, in which this remedy has been found beneficial. I have seen it employed in the three first of these affections, but without obvious relief. It has been proposed in hydrophobia, and I think, were I to meet with another case of this disease, I should try it, employing very large doses, so as to produce serious symptoms, with the view of making a powerful impression on the nervous system, and hoping thereby for beneficial results. But of course the practice would be quite empirical, and as likely to be hurtful as beneficial. This, however, must be the case with any powerful remedy employed, since we are quite in ignorance of the pathology of the disease, and, therefore, have no theory or reasoning to guide us.

In various painful diseases, hydrocyanic acid has been employed internally, as an anodyne—for example, in cancer, tic douloureux, rheumatic pains, &c; and though cases have been published in which its efficacy has been asserted, I believe I only speak the experience of the profession generally when I say that in most instances it is of little or no value.

As an *anthelmintic* it has been extolled by Brera, but the following fact mentioned by Dr. Elliotson, will, I imagine, show you its true value:—"I have frequently employed it perseveringly without expelling one worm, when a dose of calomel has instantly brought away hundreds."

Preparations.—There are only two prepa-

rations of hydrocyanic acid which I think it necessary to notice—namely, a mixture and a lotion.

(*a*) *Hydrocyanic acid mixture.*—The best mode of exhibiting this acid internally is in the form of mixture. I generally give from three to five minims of the acid (said in the shops to be of Scheele's strength) diluted in about an ounce of some mild vehicle (simple water answers very well), and repeated three or four times a day. Gum or syrup and some flavouring ingredient (as orange-flower water, used on the continent) may be added. Some persons give it in almond emulsion.

(*b*) *Hydrocyanic acid lotion.*—Two fluid drachms of the dilute acid of the shops may be employed mixed with half a pint of distilled (or rose) water as a lotion in skin diseases. Frequently about half an ounce of rectified spirit is added, and Dr. Thomson recommends, in addition to this, sixteen grains of acetate of lead. The external use of this acid, in all cases (more especially if there be sores) requires great caution. You must carefully watch its effects on the nervous system and on the pulse. In some cases it causes giddiness and faintness; and Mr. Plumbe says, in two instances it produced intermission of the pulse.

Treatment of poisoning by hydrocyanic acid.—There are four valuable agents in the treatment of animals poisoned by hydrocyanic acid—namely, chlorine, ammonia, cold affusion, and artificial respiration.

(*a*) *Ammonia.*—We are indebted to Mr. Murray for the discovery of this remedy, the value of which has been admitted by Buchner, Orfila, Dupuy, and Herbst. If the patient be able to swallow, the liquor ammoniæ, diluted with eight or ten parts of water, should be exhibited, and the vapour of ammonia or its carbonate inhaled: the latter practice is most important. Orfila says that ammonia is of no use when introduced into the stomach, but that the inhalation of the vapour will sometimes preserve life. I would recommend you to use it in both ways, taking care not to employ it in so concentrated a form as to excite inflammation of the air-passages or of the alimentary canal. In the absence of ammonia the inhalation of the vapour of burning feathers might be employed. I may here remark that ammonia cannot be useful by its chemical properties merely, since hydrocyanate of ammonia is a powerful poison.

(*b*) *Chlorine* is regarded as a more powerful antidote than ammonia. It is recommended as an inhalation, but when the patient can swallow, a dilute solution may also be administered by the stomach. As chlorine gas, or its aqueous solution, is not frequently at hand, you may employ the chloride of lime or of soda, one or both of which is now to be met with in most chemists' shops. A weak solution of either of these may be swallowed; and by adding a little muriatic acid, abundance of free chlorine may be disengaged from them for the purposes of inhalation. The same cautions are necessary in its use as mentioned for ammonia.

(*c*) *Cold affusion* has been strongly recommended by Herbst, and is admitted by Orfila to be a valuable remedy, though inferior to chlorine. Herbst says that its efficacy is almost certain when it is employed before the convulsive stage of poisoning is over, and that it is often successful even in the stage of insensibility and paralysis.

(*d*) *Artificial respiration* ought never to be omitted. Of its efficacy I am convinced from repeated experiments on animals. I once recovered a rabbit by this means only, after the convulsions had ceased, and the animal was apparently dead. It is an operation easily effected, and will be found a powerful assistant to chlorine or ammonia, by enabling it to get into the lungs when natural respiration is suspended. To produce respiration, make powerful pressure with both hands on the anterior surface of the chest, the diaphragm being at the same time pushed upward by an assistant. Inspiration is effected by the removal of the pressure and the consequent resiliency of the ribs.

Mode of recognising this acid in cases of poisoning.—Hydrocyanic acid is a substance which readily undergoes decomposition, and, therefore, it is not likely to be met with in bodies which have been interred for many days. It has, however, been recognized in a body seven days after death, notwithstanding that the trunk had not been buried but had been lying in a drain. In recent cases the acid is readily distinguished by its odour, with which, in some cases, the whole body is impregnated. The tests for this acid which I have already mentioned, will sometimes detect the poison in the filtered contents of the stomach; but the foreign matters present may in some instances prevent the action of these reagents being observed. The best mode of proceeding, then, is to introduce these contents into a tubulated retort, add some sulphuric acid to neutralize any ammonia which might be generated by the process of putrefaction, and distil by means of a vapour or water bath. Test the distilled liquor in the way mentioned in a former part of this lecture.

It has been suggested that hydrocyanic acid may be formed during the process of distillation by the decomposition of the animal matters. But, as Dr. Christison

has properly observed, the objection appears only to rest on conjecture, or presumption at farthest. You will bear in mind also that cheese, under certain circumstances, has been found to contain this acid, as I have already mentioned.

OBSERVATIONS

ON

THE VENEREAL PRACTICE OF BERLIN.

By Alexander Ure, M.D., M.R.C.S.

Late House-Surgeon to the Royal Infirmary at Glasgow.

[Continued from p. 511.]

The condylomata lata are small roundish prominent tumors, commonly of the size of a lentil or pea, with a broad sessile base. They are of a dirty reddish brown or puce colour, and are not, like the acuminata, united in clusters, but uniformly insulated, always maintaining their individuality, constituting small circumscribed eminences. Their usual situation is round the anus and flexures of the nates, upon the glans and frænum, the external margins of the labia, and the integuments of the penis and scrotum. In contradistinction to the former description, these are covered with the common epidermis, or they may more strictly speaking be regarded as a thickening of that tissue which Desruelles has called a hypertrophy of the epidermis, whence the specific name of epidermic might be conveniently substituted for latum, as at once pointing out their formation. Their surface is dry, smooth, and even, emitting no sensible secretion except when excoriated or ulcerated, in which latter condition, if not identical, they have a strong analogy with the *papules muqueuses* of French writers. They seem also congenerous with a symptom of venereal affection, very rare in Europe, but extremely frequent in equinoxial regions, and particularly the West Indies, called *pian.* It consists of moist pustules that show themselves on the bend of the arm, the groin, and the axilla, highly contagious, so that when they attack nurses, they are soon communicated to the children carried in their arms, and manifest themselves on the thighs of the latter.

Epidermic condylomata implanted about the anus might, without due discrimination, be confounded with hemorrhoids. The peculiar livid colour of the latter, together with the previous history of the case, will prevent such an error being committed.

Condylomata may be considered as an accidental tissue, resulting from an increased activity of nutrition in a part which causes its anatomical elements to be morbidly developed, so as to produce an abnormal growth. The increased activity which thus disposes the cellular tissue and capillaries to extend and form new productions, is certainly, in the instance of the epidermic or condyloma latum, referrible to an original and essential affection of the mucous membrane, namely, inflammation with puriform discharge.

In every case which has come before my notice, their development was preceded and accompanied by unequivocal signs of disorder of the mucous membrane, whether in the form of gonorrhœa, balanitis, or leucorrhœa.

No doubt is entertained as to their contagious nature, or that the vehicle of the contagion is the matter secreted. It is probable that the same principle which Jüngken has demonstrated to exist in purulent ophthalmia—a disease of a similar nature to gonorrhœa, and characterized by the development of vegetations on the conjunctiva—may from analogy be extended to the affection under consideration, namely, that the more puriform the secretion, the more contagious it is*.

The following is a well-marked example of the facility with which epidermic condylomata may be communicated by the mediate contact of the matter.

Two females from the country, sisters, the one aged 22, the other 15 years, were admitted as patients with condylomata lata, accompanied with purulent discharge. The younger sister had, besides, ulceration of the throat. Both were simultaneously affected with discharge from the vagina, to which the condylomata succeeded, while sleeping in the same bed with their mother, at that time labouring under like symptoms contracted from her husband.

The endermic, or acuminated condylomata, are believed to be closely allied to exanthematous diseases, and to depend on some constitutional taint; yet it is admitted that they are often a primary affection, co-existent with blennorrhœa, without previous venereal con-

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* Ueber die Augenkrankheit welche in der belgischen Armee herscht, vom Professor Jüngken. Berlin, 1835.

tamination. Kluge and Fricke agree in regarding them as parasitic growths which must pass through a peculiar course of vitality before they can be made to disappear.

As I have been led into a longer disquisition on the subject of condylomata than was originally contemplated, I shall not enter on the disputed question of their general or local nature, but simply content myself by mentioning that M. Ricord has expressly stated, that when, from other causes than impure sexual intercourse, blennorrhœa is established in the urethro-genital mucous membrane, it is preceded by an erysipelatous inflammation, and attended by the development of small papillæ the size of a pin's head, or granulations, or incipient vegetations*.

Hence it is most likely that when the irritation is quite superficial, we shall have the production of epidermic vegetations; when, on the contrary, it is deeper felt, endermic vegetations will make their appearance.

It appears well established, that the condylomata acuminata, when once called into being, are capable of propagating themselves under peculiar circumstances, in a manner which is not to be distinguished from contagious propagation. Thus it is frequently found that patients admitted with probably one or two vegetations on one side alone of the nymphæ or vagina, will, in a short time, have a crop of similar excrescences on the opposite surface, even although the strictest attention to cleanliness be observed, if due precaution be not taken to insulate the condyloma, so as to prevent the matter that exudes from getting there.

Parts in which a process of ulceration has formerly existed seem strongly predisposed to the imbibition of the morbid fluid, and consequent formation of condylomata.

Treatment of Ulcers.—The cure, properly so called, is preceded by a preparatory cleansing of the bowels for a couple of days, by means of solution of sulphate of magnesia, called the "mistura anglicana." Baths are seldom, and bleeding hardly ever, had recourse to. When ulcers are the only symptom, the patient is ordered half a grain of calomel twice daily. The diet is restricted to three portions of *soupe maigre*, and two ounces of white bread, in the 24 hours. When the sores put on a healthy aspect, then a more generous allowance is given. Much stress is laid on the starving system; and Rust expressly states, that patients should be permitted only as much nourishment as is absolutely necessary for the purposes of life, or maintaining the self-existence of the organism, in order that the parasitical principle of vitality (the lues) present in and dependent on the organism, may be extinguished*. Repose of the body is enjoined, by confinement to bed, as far as can be consistently done, and an equable temperature is kept up in the apartment.

By rigid adherence to the above plan, true syphilitic sores, in the course of from ten to twenty-one days, lose the surrounding hardness and other distinguishing marks, and shew a healthy granular surface.

Simple sores are commonly cicatrized before a fortnight has expired. Where, from constitutional or other causes, over which medicine has no direct control, the sore assumes an indolent character, and cicatrization advances slowly or not at all, local applications are resorted to, such as epithems imbued with a dec. flor. anthem., or sol. sub plumbi; failing these, in some rare instances, the actual cautery is applied. It is occasionally found that, in spite of every topical means that can be put in practice, small, round, perfectly superficial sores, situated on the lower parts of the internal labia, resulting from mere abrasion, will, from their surface being kept constantly moist with the exuding secretion of the part, remain for days, nay even weeks, stationary, retaining a fine florid aspect, but without any disposition to cicatrize. In such cases, by merely uncovering the part, and leaving it freely exposed to the air, the sores will be found completely skinned over after the lapse of a few hours. This same practice I have seen successfully pursued, for the healing up of sores caused by suppurating bubo, when there only remained a small spot in the middle, which had ceased to emit any appreciable discharge.

In reference to the sores produced by rupture of the caruncles at the vaginal orifice, it is of consequence, for speedy reunion, to bring the partially detached caruncle into contact with the abraded sur-

* Journal des Connaissances, Fev. 1834.

* Aufsatze and Abhandtungen, p. 385.

face from which it has been torn, and maintain their apposition by means of little compresses of charpie, keeping the patient in bed with the thighs closely approximated. Although such lesions would undoubtedly heal spontaneously, yet the above precautions are indispensable where it is desirable to prevent irregularity of surface, and a recurrence of the accident under like circumstances.

Where the sores are accompanied with other signs of diseased action (such as bubo, ulceration of the throat, &c.), and where the patient's constitution is good and his health unimpaired, corrosive sublimate is exhibited in preference to, calomel, in the form of pills, either in uniform small doses, as the eighth or fourth of a grain twice daily, or else the old pill of Hoffman, as in the formula of Dzondi, whose method enjoys at present a high reputation in the treatment of venereal disease. His prescription is as follows:—

> ℞ Hydrarg. Mur. Corros. gr. xij.; solve in aquâ distill q. s.; adde Micæ panis albi, Sacchari albi, aa. q. s. ut fiant. Pil. gr. j. No. ccxl. Consperg. Pulv. Cinnam. aut Lycopodii.

Of these pills, four are taken the first day, the third day six, the fifth day eight, and so on, advancing in the arithmetical progression of two on the alternate days, until the patient has come to take 30=1½ gr. sublimate; but if necessary, the medicine may be continued longer and increased farther, as long as it can be tolerated and does not produce any bad effects *. Dzondi has gone as far as three grains; and Handschuh tells us that he has known eight grains taken without inconvenience. The medicine must be taken all at once, and that immediately after dinner, so that it may be assimilated with the chyme and readily absorbed: the larger quantity, however, may be given in divided doses. Such sores as remain unhealed after the cure has been duly gone through, are not to be regarded as syphilitic in their nature, and must be ascribed to some other efficient cause—as scrofula, psora, herpes, &c. His work contains much absurd and fanciful hypothesis.

Gangrenous sores are treated in the old way, by cinchona and mineral acids internally, and externally by vin. camphor.

Condylomata of either sort, when few in number and without other complication, with the exception of puriform discharge, are treated by large doses of calomel, according to the method of Weinhold.

Weinhold's fundamental principle is, that calomel, unless given in great doses and at long intervals, produces ptyalism, which interferes with its action, weakens the patient, and thereby "*disannuls the wonderful, recondite, deep-laying, and hitherto inscrutable, operation of the medicine on the abnormal reproduction* *;" he has introduced what he has chosen to dignify by the title of the great quicksilver cure — *die grosse quecksilber-cur*. The following is his fantastic mode of administering calomel:—The patient takes, a couple of hours before going to bed, on an empty stomach, a powder consisting of 10 grains calomel with 15 sugar; which he follows up with two cupfuls of warm bouillon. After half an hour has elapsed he repeats the same, and if robust, after a third half hour, a farther half dose; so that he swallows, in all, from 20 to 25 grains of calomel, and half a dozen cupfuls of beef-tea. Hereafter he retires to bed. On the following morning he is allowed one or two cups of moderately strong coffee, on the back of which two or three watery stools are expected: should such, however, not take place, it is provided that on the tenth or twelfth hour from his having taken the calomel, a powder, consisting of from 15 to 20 grains jalap, with as much cre. tart., shall be administered, so as to occasion at least one evacuation; after which, rarely any effect on the mouth will be observed.

It is only in persons who have employed often considerable quantities of mercury that salivation ensues; and this is applicable to all the other modes of treatment, we are told. If the patient can remain a few hours at home, in the morning, it will be desirable, in order to promote the cutaneous transpiration; but generally, unless the weather is bad, he can pursue his ordinary avocations. On the evening of the fourth day, the same dose is repeated, divided

* Neue zuverlässige Heilart der Lustseuche von Dr. K. Dzondi, Prof. an der Universitätzu Halle. 1826.

* See Handschuh, p. 193.

into three parts. On the seventh day, the third dose, on the tenth the fourth, on the thirteenth the fifth, on the sixteenth the sixth; after which, on the 19th or 22d, the seventh or eighth dose, dose, with the cure is completed. At the third and fourth dose, it is mentioned that the stomach and bowels become so used to the stimulus of the calomel, as will make it needful to add five or six grains of jalap to the calomel to produce a single evacuation. There is no need of observing any particular difference in the diet*.

The endermic condylomata are expected, under the above treatment, to assume quite a different appearance from what they originally presented; their surface, from being moist and smooth, should become dry and shrivelled, and from a bright glistening red be transformed to a dull earthy or coriaceous hue.

Before the above changes have taken place it is not deemed advisable to extirpate them, which may then be immediately performed by snipping them away with a scissors curved on the flat, and cauterizing deeply the little wound with a pencil of lunar caustic, having previously wiped away the blood from it with a sponge. The cauterization is always accompanied with the most lively pain, seeing that the corium must be fairly perforated by the escharotic, otherwise a speedy relapse will be sure to follow.

Where the condyloma has a broad base, removal by ligature is preferred to that by excision.

Under the use of calomel, given as above directed, condylomata lata disappear without surgical interference. One female afflicted with them was cured after two doses, another after six. The pre-existent and concomitant leucorrhœal discharge is at the same time believed to be modified in its properties, becoming a benign mucous discharge in lieu of a contagious puriform secretion†, which can be soon stopped by the employment of dec. ulmior flor. anthem, &c.

If, on the other hand, the excrescences are more numerous, of longer standing, and accompanied by other signs of venereal contamination, as bubo, &c., the method of Dzondi is straightway put in practice. In the case of endermic condylomata, where the desired local changes are tardy in manifesting themselves, the shrinking and arescence of the surface are to be promoted by means of various topical astringents, such as the sol. nit. argenti or subacet. plumbi, as recommended by Rust and Gräfe, and kreosote, which I saw tried in a few instances. The last seemed to answer only when the condylomata were small, of recent date, and implanted on a smooth lubricated surface, as the internal portion of the nymphæ and prepuce. After a few applications of the pure kreosote, their surface acquired a cream-coloured tint, not unlike the film produced by nitrate of silver on an ulcerated part, and lost something of its unevenness; but no farther effect was remarked on continuing to repeat the application. However, Fricke, who has had great experience in venereal practice, mentions that kreosote frequently applied to the condylomata, not only effectually caused their disappearance, but seemed to prevent their relapse*. If it be asked, why not at once proceed to try to check the process of morbid vegetation, and remove the products, without having recourse to any internal remedies, they answer that in so doing chancres would appear on the wound, or the throat, or nose. This opinion I myself heard asserted by Professor Gräfe in his clinic†.

* Von den Krankheiten der Gesichtsknochen und ihrer Schleimhaute. Halle, 1828.

† The fluids secreted during gonorrhœa and leucorrhœa are held to be of two kinds—a *gutartig*, or benign, and a *bösartig*, or noxious.

* Fricke, as above stated, regards condylomes to be parasitical growths, which must attain their full maturity before they are ripe for removal. Hence such as are rebellious to every means of destruction he allows to grow unmolested until they have reached a certain size, and seem to stop growing, at which stage he finds the ordinary local means succeed in discussing them permanently. From three to fourteen days are requisite.—*Casper'. Wochenschrift*, 1834, No. I. S, 29.

† The pertinacity with which men of acknowledged reputation not only accredit, but promulgate, doctrines which are shewn to be most flimsy and absurd, is no where more palpably displayed than in the present instance; for not only has Fricke demonstrated that condylomes will yield neither to internal nor external means, until they have reached a certain stadium, when the simplest topical astringents suffice (*lib. cit.*); but Handschuh, in one of the best German treatises on this class of diseases, cites Dease's Observations on Venereal Warts, in the Medical and Philosophical Commentaries of Edinburgh, vol. iv. part 3, 1777, in which cases are brought forward of syphilitic excrescences, that did not vanish when the patients had taken so much mercury that they often died of phthisis in consequence of repeated salivation.

The condylomata lata, or epidermic form, when few in number, are soon dispelled by a few doses of calomel, according to the formula of Weinhold, or sometimes even without medicine, by regulation of diet and the digestive organs, and keeping the parts clean. If, however, no attention has been paid to them, and they have progressively multiplied over a wide extent of surface, or have acquired a hemispherical form and porcelanous look, indicating a peculiar inveteracy of character, they are extremely difficult to get rid of. The plan of Weinhold fails to produce any effect upon them, and the bolder method of Dzondi has to be repeatedly renewed, and aided by escharotic applications, of which the *potassa fusa* claims the preference*. Excision is never required for the removal of condylomata lata, even in their most obstinate forms.

The cure lasts, in recent cases, from a fortnight to a month; in what might be called chronic cases, from six weeks to two months are required to secure immunity from their return. In the case adverted to near the beginning of this paper, of the young woman who had been six months under treatment, the condylomata were in immense numbers, occupying not only the internal surfaces of the genitals, but all the surrounding integuments.

It is believed that the method of Dzondi, followed by excision, although in most cases efficacious in the eradication of the endermic vegetations, affords no permanent security against their relapse. For example:—A man towards the latter end of March presented himself at the visit, on account of several small groups of acuminated condylomata on the glans and inner folds of the prepuce, which began to form a fortnight prior to that date, without any assignable cause. Yet three months before that date he had been dismissed from the hospital cured of similar vegetations by means of a full sublimate course, followed by excision.

[To be continued.]

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* The following solution is much employed for these condylomata likewise:—

R. Hydrarg. Mur. Cor. dr. j.; Camphori, dr. ss. solv. in Sp. Vin. Unc. Apply twice a day.

OPHTHALMIC SURGERY.

TREATMENT OF EVERSION OF THE EYE-LIDS, AND OF HARE'S EYE.

To the Editor of the Medical Gazette.

SIR,

THINKING that a short description of the very ingenious operation for eversion of the eye-lids, and for hare's eye, proposed and successfully performed by Professor Jaeger, of Vienna, might be interesting to some of your readers, I have prepared the following account of it for the Gazette, should you deem it worthy of a place in your able journal. A circumstance that must add to the interest taken in this operation is, that many of the cases to which it is an ample remedy, were classed in the list of incurable diseases by Professor Beer, and other eminent operators. I have seen no account of the operation in any work in the English language. I am indebted for the knowledge I possess of it to personal observation of the practice of Professor Jaeger, and to Dr. Dreyer's account of the operation, in his thesis, entitled "Nova Blepharoplastices Methodus," some parts of which I have verbatim translated.

I am, sir, very respectfully,
Your obedient servant,
WILLIAM BROWN, M.D.
Lecturer on Operative Surgery of the Eye.

317, Argyll Street, Glasgow,
January 1836.

Operation proposed by PROFESSOR JAEGER *of Vienna, for the cure of bad cases of Ectropium and Lagophthalmus.*

When the deformity is considerable in cases of ectropium and lagophthalmus produced by a cicatrice of the skin, both the transverse and perpendicular diameters of the eye-lid are faulty in their dimensions; the perpendicular diameter, or the breadth of the eye-lid, is shortened, and the transverse diameter is elongated. The object of Professor Jaeger's operation is to increase the breadth of the eye-lid, and to reduce the transverse elongation to a proper length. This two-fold aim is accomplished by attending to the following indications: 1. Unnatural adhesions to the edge of the orbit are to be loosened by dividing cicatrices. 2. The abnormal

length of the palpebral margin (the distance between the external and internal commissures of the eye-lid) is to be diminished to the normal length by excising a quadrilateral shaped portion of the whole thickness of the lid. 3. The breadth of the eye-lid is to be restored by detaching the integuments from the bone in the neighbourhood of the orbit, and drawing them upwards or downwards according to circumstances. 4. The wounds made are to be united by sutures. 5. The position of the new eye-lid is to be maintained by the proper application of plasters, compresses, and bandages. 6. After the operation, the patient is to be treated conformably to the principles of surgery.

The older operators only regarded the perpendicular diameter of the eye-lid, the dimensions of which they unsuccessfully attempted to increase. Sir William Adams was the first to point out and perform a successful operation for reducing the longitudinal diameter of the eye-lid to its normal length. His operation consists in the excision of a triangle-shaped piece from the edge of the eye-lid. Adams also attempted, but he failed as many before him had done, to increase the perpendicular diameter of the eye-lid by incisions in the skin. Fricke and other eminent German surgeons have succeeded in rectifying the faulty breadth of the eye-lid by the transplantation to it of a portion of skin from the brow or cheek, but in their operations they have made no provision for diminishing the horizontal diameter. Both objects are effected by Jaeger's operation.

Jaeger's operation is not intended for simple cases of ectropium. It is meant as a remedy for those cases which are the result of a cicatrice of the skin producing considerable loss of substance in the eye-lid, and where the transverse diameter is too much elongated to admit of the lid coming into contact with the ball of the eye. Where the tarsus is diseased in structure, and abnormal in its course, and where the eye-lid is adherent to the cheek, the edge of the orbit, or to the supra-orbital region, this operation is particularly applicable.

Professor Jaeger performs this operation in lagophthalmus, when much of the eye-ball is exposed, and though the disease be unattended with eversion of the affected lid.

It is also recommended as a suitable operation after the extirpation of excrescences or tumor of considerable size from the eye-lid, when it has been found necessary to remove a portion of the palpebra along with the tumor. In cases of this description, the form of the eye-lid may very frequently be restored by the operation about to be described.

During the performance of this operation, the patient may be seated on a low chair, with his face towards the light. A child may be placed in the lap of an adult. Two assistants are requisite. One of them supports the head of the patient on his breast, and elevates the upper eye-lid when the operation is to be performed upon the under one; the other assistant hands the instruments to the operator, and sponges away the blood. The instruments necessary are two fine scalpels, one of which is to be straight and double-edged, and the other single-edged and convex upon its cutting surface. A dissecting hook and forceps, a straight forceps and scissors, are also requisite. In addition, the operator is to be provided with needles, ligatures, court plaster, adhesive plaster, lint, graduated compresses corresponding in size to the circumference of the orbit, and a double-headed roller four or five yards in length.

Before proceeding to the operation, the difference in length of the transverse diameter of the everted and sound lid is to be accurately ascertained by measurement. In the operation, the palpebral margin of the everted eye-lid is to be made to correspond in length with that of the sound lid on the other side of the face.

The next step in the operation on the upper lid consists in taking hold of the everted palpebra about the centre of its edge with the dissecting forceps, or hook, and drawing it downwards so as to put the cicatrice on the stretch, by which the palpebra is adherent to the margin of the orbit. With the convex scalpel, a deep incision is now to be made, which is to take its course about midway between the margin of the everted lid and the superciliary arch. The incision is to be commenced and terminated in sound skin. The wound is to be carried through the whole thickness of the eye-lid, so as to form a slit through which the eye-ball may be seen, the palpebral margin falling down

and leaving that opening through which the eye-ball appears. The length to which this incision is to be carried must depend on the circumstances of the case. In case of wounding the eye-ball, a horn spatula may be inserted between it and the eye-lid, should the operator deem a precaution of this kind necessary.

The narrow strip separating the natural rima palpebralis from the artificial opening formed by the incision just described, is the part from which the reduction of the transverse diameter of the eye-lid is to be made. The size of the portion which ought to be removed is already known from the measurements made before the operation was commenced. The superfluous elongation should be removed from the centre of the strip, and the lines upon which the incisions are to run may be marked with a black-lead pencil or ink. With the aid of scissors and straight forceps this part of the operation is easily effected.

A straight double-edged scalpel is now tobe used for the purpose of separating the integuments from the os frontis. The forceps is to be used in taking hold of the upper lip of the wound, and for separating it a little from the edge of the orbit; the scalpel is now to be introduced upwards and slightly outwards between the posterior surface of the orbicular muscle and the anterior surface of the frontal bone. Having been pushed onwards to a sufficient depth, the blade is to be carried with a sawing motion towards the temple and external canthus, and then towards the median line of the os frontis, without enlarging the original wound of the palpebra, transfixing the skin, or injuring the periosteum. By this process the skin and muscle covering the supra-orbital region and the angles of the orbit are separated from the subjacent parts, and rendered capable of undergoing a change in their position. The depth to which the scalpel will require to be carried, and particularly the extent in the transverse direction to which the integuments ought to be detached, must always be proportionate to the loss of the palpebral substance, and to the varying mobility of the frontal coverings.

The wounds are now to be united by the interrupted suture. The tarsus is to be transfixed by each ligature; but the conjunctiva is to be carefully avoided. The perpendicular wound, by which the undue elongation of the transverse diameter of the eye-lid was shortened, is first to be united by means of two ligatures. An assistant is then to draw downwards, towards the centre of the orbit, the integuments recently detached from the supra-orbital space and from the angles of the orbit, so as to approximate the edges of the wound made in the transverse diameter of the eye-lid. The first ligature is to be inserted in the middle of the wound, in order to act as a central point of attraction upon the surrounding integuments. Should the upper lip of the wound not much exceed the lower lip in length, lateral ligatures may be immediately inserted; if on the other hand it exceed to the extent of forming a fold, this must be removed by the scalpel or scissors, in order that the edges of the wound may be nicely adjusted. The number of ligatures that will be required cannot *à priori* be determined.

Coaptation of the wound having been effected in this manner, the eye-ball is covered by integuments obtained partly from the supra-orbital region, but chiefly from the angles of the orbit; the supercilium will, however, be somewhat more depressed, and describe a smaller and less convex arch than it formerly did.

The operation upon the under eye-lid consists in removing a triangular piece from its edge, after the plan of Adams for the cure of ectropium, and in detaching the integuments from the edge of the orbit by a similar process to that already described for increasing the perpendicular diameter of the upper lid.

The action of the ligatures is to be supported by interposing narrow strips of court plaster. The wounds are then to be covered with small pieces of lint; and the graduated compresses, which have been already described, are to be placed upon the supra-orbital region, or cheek, according as the operation has been done for the restoration of the upper or lower lid. Over the graduated compresses long strips of adhesive plaster are to run, being applied in such a manner as to draw the integuments towards the palpebra, and to approximate them to the bones. When the upper eye-lid has been operated upon, the adhesive plasters may extend from the nape of the neck to the cheek. A roller may be applied to assist the action of

the plasters, if it be deemed necessary. In the after treatment nothing ought to be omitted likely to effect union by the first intention.

SOME REMARKS

ON THE

MODIFICATIONS OF THE SENSES.

To the Editor of the Medical Gazette.

SIR,

HAVING observed in your journal of the 26th of December some remarks by Mr. Noble, on the postscript appended to the paper which you did me the honour to publish a short time since, on the physiology of the fifth pair of nerves, I feel it desirable in reply to offer a few explanatory observations, inasmuch as I perceive Mr. Noble has not only mistaken the contents of the postscript, but drawn conclusions from his own interesting experiments which I conceive to be rather erroneous.

Mr. Noble says, "I have cautiously abstained from expressing any mere views, but confined myself to the facts, and the suggestions of what appeared the legitimate induction." Mr. Noble's own remarks at the conclusion of his paper will decide this point; he there observes, "I make no comment: this case is offered to the profession as an additional fact, tending most strongly, in the opinion of the present writer, to the conclusion, that the function of taste is not a modification of common sensation, and that, in accordance with all the analogy which physiology affords, we must look for a separate nervous supply."

Now, Mr. Editor, allow me to ask what are the contents of this paragraph but *comments* or inferences, deduced by Mr. Noble from his own experiments, including, moreover, two exceedingly important physiological hypotheses; the first assuming that the function of taste is not a modification of common sensation; and the second, that to account for it we must look for a separate nervous supply."

I must beg leave to offer a few remarks on these hypotheses, which I shall endeavour to shew cannot be deduced from any experiments, as they are not in accordance with what we know concerning the nature of the senses, and the connexion subsisting between them. The impression made by the contact of bodies on the senses is by no means limited to those of touch and taste. In order that an impulse be communicated to the other senses, whether by distant bodies acting through a medium, or otherwise, it is still a necessary condition that there be immediate contact. The susceptibility of certain nerves to be excited by the minute and attenuated forms of imponderable matter is, therefore, only an exceedingly delicate power of the perception by touch, of which all the other senses must be considered as merely modifications; and whereas common sensibility is fitted only for the perception of some of the grosser, but still important, conditions of matter. such as extent and impenetrability, even this sense may be so exalted as to serve the purpose of other senses, such as seeing and hearing, should they have been originally wanting, or subsequently extinguished. For instance, some polypi evince a consciousness of the presence of light, although they are utterly destitute of visual organs.

It is on record that blind persons have ascertained the colours of bodies by touch alone. I have just been informed by my friend Dr. Grant, of Richmond, that a case of this kind came under his observation, wherein a blind man was able to distinguish the various colours of woollen cloths by the touch. Another case of the same kind occurred in one of the daughters of Sir Robert W****n. This lady, who had the misfortune to lose her sight, could distinguish the colours of worsted thread, and whilst working could easily discriminate the different shades*. A third instance of the same general nature, although different in its kind, may be mentioned in the case of Mr. Arrowsmith. This gentleman, who was totally deaf, could derive great pleasure from the vibrations caused by musical instruments in surrounding solid bodies, by merely placing his hand in contact with them. These cases afford interest-

* Other examples of this curious faculty in the blind will be found in M. Rodenbeck's interesting paper in the present number.—E. G.

ing illustrations of the affinity between the impressions of touch and those of the other senses.

Mr. Noble's second hypothesis, included in his remark, "that to account for the sense of taste we must look for a separate nervous supply," seems to be derived from the circumstance of the tongue containing separate endowments of taste and touch, and assumes that some other than the lingual nerves are necessary to confer on the tongue one of these sensations.

This question, which has often occupied the attention of physiologists, the case published by me tends, I conceive, to determine, since it was found that the destruction of the lingual branch of the fifth pair involved at the same time that of the senses of taste and touch. It appears, therefore, quite unnecessary to "look for any other nervous supply." The view here taken agrees with the theory of one of our greatest authorities, Sir Charles Bell.

The nerves of the fifth pair being compound and ganglionated, giving both sensation and motion, doubtless contain filaments specially modified for the purpose of taste also; and as compound nerves derive their origin from separate portions of the central mass, it is not at all surprising that one portion of the nerve may be paralyzed, without implicating the other, a circumstance often illustrated by paralyzed parts possessing either motion without sensation, or sensation without motion. A distinction must likewise be made between the effects of partial paralysis, and the total destruction of a nerve. In my patient's case the post-mortem examination, carefully performed in the presence of Dr. Roget and Mr. Robert Wade, ascertained the total destruction of the fifth pair, which had completely extinguished the senses both of touch and of taste. Mr. Noble's second hypothesis must consequently be erroneous. Much credit is, however, due to him for shewing the distinctness of the functions of taste and touch, which his interesting experiments tend to demonstrate.

A case in illustration of this theory has been brought to my notice by the kindness of Mr. Robarts, in a woman aged about 45 years, who has nearly lost the common sensibility of the whole right side of the face, including the eye, nostril, and tongue, which latter part, however, retains a slight perception of touch; but the taste on that side is entirely abolished, so that neither vinegar, salt, nor ordinary aliments, produce the slightest impression,—a circumstance exactly agreeing with the experiments of Mr. Noble.

In conclusion, it appears necessary only to remark, that although the experiments of Mr. Noble tend to demonstrate the distinction of the senses of touch and taste, in which view I fully concur, and have briefly illustrated it by an appropriate case, yet the deductions which he has drawn from them respecting the modification of the senses, and the nervous supply to the tongue, are at variance both with the facts detailed in my paper, and with the opinions of almost all the celebrated physiologists of the present day, whether of the French or English school.

I am, sir,
Your obedient servant,
JOHN BISHOP.

38, Bernard-Street,
Jan. 15, 1836.

LETTER ON THE BLIND,

By MONSIEUR A. RODENBECK,
Member of the Chamber of Deputies, Brussels.

[The following letter was recently sent by the author to a surgeon in London, by whom it has been forwarded for insertion in the Medical Gazette. It adds greatly to its interest to be aware that the writer has been blind from early life.]

SIR,—In our last conversation, you asked me for some information on the blind. I believe I cannot answer you better than by communicating my reflections on this subject, which are unfortunately the fruit of a very long personal experience.

The greater part of the writings which have been published on the blind are full of exaggerations and errors. It belongs to one of their fellow-sufferers to judge of them without prejudice, and to appreciate them properly. Without partaking of the prejudices consecrated by time and habit, I will endeavour to found my opinion on facts, and to weigh with equal frankness my praises and criticisms.

Education, the first benefit of civilization, and an abundant source of the purest consolations, becomes for the blind an absolute necessity, and the more

attractive, because, knowing the advantages to be gathered from it, they feel more keen regret at being deprived of it. Sight is, of all the senses, that which procures to man the most delicious sensations; it is always at the expense of happiness that we cease to enjoy it, though the development of other senses, and especially that of the touch, makes up to us in some manner for the privation of light. I do not speak here of blindness in a figurative sense,—that species of blindness imparts felicity, and our drawing-rooms are crowded with happy blind men of this kind.

The idea of rendering the blind useful to society and to themselves, belongs more particularly to M. Valentine Hauy, who erected, in 1784, the first blind school which ever existed in Europe. Doubtless you will learn with pleasure the accident which induced him to devote himself to their instruction; he relates it as follows:—"A novelty of a singular kind," says M. Hauy, "attracted, some years ago, a concourse of company to the entrance of one of those places for refreshment in the public promenades, where our honest citizens go to refresh themselves for a moment at the close of day. Eight or ten poor blind men, with spectacles on their nose, seated in a row at a desk which held music, executed a discordant symphony, which seemed to excite the mirth of the bystanders. A very different sentiment affected me, and I conceived at the moment the possibility of realizing, for the advantage of those unfortunate men, the power and the means of enjoying the art of which they had only a limited and ridiculous idea. The blind (I said) know objects by the diversity of their forms; why should they not distinguish letters —an *A* from an *I*, if these characters were rendered palpable?"

Soon after, M. Hauy submitted to the judgment of the Royal Academy of Sciences a memorial, in which he explained the methods which he proposed to employ for the instruction of the blind; consequently, an asylum was opened to them. They were for the most part wandering on the highways, a prey to ignorance and misery. They were now collected in a residence by the cares of benevolent persons, and withdrawn from that moment from the contagion of vice. The times were favourable to the foundation of such an establishment: philanthropy had become at that epoch a sort of fashion, which all the world followed, some from humanity, others from vanity. There was a crowd of subscriptions for the new school, and by the magic of fashion the blind became the subject of every conversation. M. Hauy gave up his whole fortune for their benefit. The whole life of this worthy man was one long act of beneficence, and it is to this illustrious scholar that the blind are indebted for being no longer considered as a misfortune to their family. Never—no, never—shall I forget this good man; and I acknowledge here, in the name of all the blind, an eternal gratitude to him who was their protector and their guide. I shall not quote to you here the brilliant success which has crowned the efforts of this learned professor, but I delight to recal to you, that by his ingenious contrivances he has in a manner given a new existence to the blind. To withdraw these interesting beings from the painful and dangerous burden of idleness—to make them find in labour a resource against want—to console them for their privation—such is the happy result of the method invented by M. Hauy.

The plan consists principally in enabling blind persons to execute different labours relative to arts and trades —such as making cordage, spinning wool and flax, making rush and straw bottoms to chairs, baskets and mats, knitting stockings purses, fishermen's nets, &c. In short, he has contrived to enable them to perform almost every kind of handicraft. These last advantages are suitable to the blind who belong to poor families; as for those who form part of the upper classes, they are enabled to learn reading, writing, arithmetic, geography, music, different games at cards, draughts, chess, dominos, and all the little manual arts which are used as amusements in society. In one word, they are inspired with a taste for employment; and ornament their memories (generally so excellent and methodical) with the finest literary extracts. By the help of this happy system, they forget their misfortune, and thus are restored to society men who frequently form some of its greatest ornaments; for experience has proved that they are infinitely more cheerful than

could be supposed possible from the nature of their calamity. I have above enumerated the several kinds of knowledge that the blind can acquire: this information is immense; but as mankind are always fond of the marvellous, ancient authors have exaggerated the talents and the defects of the blind; and the moderns, without examination, and without reflection, have repeated their errors. It is thus that there has been spread and propagated in society a variety of mistakes, either from the unworthy desire of astonishing men (and many like to be astonished), or to captivate the vulgar, who admire exaggeration and hyperbole.

An idea which has been generally received is, that the loss of one sense adds to the perfection of the other senses. This assertion is bold; I will say more—it is totally absurd. The perfectibility of the touch amongst the blind, called so ingeniously the geometrical sense, is only acquired by the continual exercise of this sense; necessity, the powerful mover of all our actions, is the sole cause of the superiority that we have in this respect over those who are. If these last practised more the sense of tact, the pre-eminence would be no longer in favour of the blind. If the prejudice which I have just observed was correct, it would follow that we might establish as a principle, that the loss of two or three senses would be attended by a moral compensation. It is not so. M. Hauy mentioned to me the case of a young girl, deaf, dumb, and blind, whom he tried to instruct without being able to succeed. Instead of having a double degree of intelligence, this unhappy creature was in some measure deprived both of intellect and feeling. All that has been said of the prodigious memory of the blind is equally erroneous: this superiority is only caused by exercise and necessity, and not by a *mnemonica*, as some authors pretend. Another cause of their great memory is that blindness prevents all distractions caused by sight. I must add, that it is of the greatest importance that the memory of the blind should be strengthened by exercise. This faculty is so useful to all men, that Bonaparte, impressed by its truth, declared that a man without a memory was like a city without a garrison.

A delicacy of touch and a good memory must form the basis of the education of the blind; and it is upon these two qualities that M. Hauy has founded his system of instruction. Reading by means of prominent characters and mutual teaching, have been equally employed by him from the origin of his Institution. All these methods are ancient, but have been modified in modern times.

Many persons believe that the blind can distinguish colours by the touch, and some well-informed men (at least on other subjects) have often asked me questions on this point. I can assure them that it is absolutely impossible; but the blind have some other means of distinguishing colours, although imperfectly. I knew a blind man whose sense of smelling was so exquisite, that by rubbing a piece of coarse blue cloth with his hand, he smelt indigo, and thence drew the conclusion that the cloth was of a blue colour. Another blind man had a habit of chewing any article of stuff that was presented to him, and was enabled by this means to guess the colour, particularly when gall-nuts had formed a part of the dye.

I met, one day, a blind Frenchman wandering about the boulevards of Paris; he was surrounded by a troop of citizens, who were all in ecstacies at perceiving that a man deprived of sight could ascertain colours. This man held in his hand a wooden octagon, whose eight sides were each painted of a different colour. When the curious bystanders placed his finger upon the surfaces, he cried out this side is red, or green, or blue, &c.; but when he was asked for some money, the crowd disappeared, and I asked permission to examine his octagon. I remarked distinctly with my finger, that one of the surfaces was highly polished, whilst others were rough and sharp to the touch. These irregularities were produced either by the particles of the paint, of which some are infinitely coarser than others, or by the labour of the carpenter, who, I am inclined to believe, had contrived with great art different marks or signs easily recognized by the touch. Such are the secrets of those blind persons who pretend to find out colours by the touch or smell. To discern the truth, it is necessary to place in their hands articles which they have not previously studied in the company of a person who sees; you will soon be convinced of their

ignorance. For those who have never been blessed with sight, this ignorance is no privation: — they have no regrets, for they cannot regret what they have never known. It is for this reason that persons blind from birth are more happy than those who become so by accident.

Metaphysicians have endeavoured to give an idea of colours to the blind by artificial means, which consist chiefly in comparing the sounds of different instruments with colours. Imagine, they say, that the brilliant sound of the trumpet has the same effect on the ear that the colour of scarlet has on the eyes. This method is ingenious enough, but is incomplete, and devoid of exactness. This ignorance of the blind, in all relating to colours, renders the history of the celebrated mathematician, Saunderson, who was blind from his birth, still more extraordinary. He gave a course of lectures on optics with the greatest success; but who, notwithstanding his great talents in demonstrating the laws of catoptics and dioptics, could never comprehend or conceive the colours of the prism, or the verdure of the foliage*. His comparison of the resemblance of the visual ray to a fine elastic thread, is more exact than that of the resemblance of the sound of the trumpet to scarlet. The blind have no positive ideas of ugliness and beauty, but as their eyes are at their fingers' ends, the sense of touch is employed by them with more or less success.

The blind easily discern the softness of the skin, leanness or plumpness, firmness of muscle, pleasant or unpleasant breath, the form of the face, &c. He distinguishes if the nose, the mouth, the chin, &c. have proportions suitable to the received rules. But neither the blind Du Puiseaux, nor the learned Saunderson, have been able to judge of the whole appearance of a face or figure; they have never known the eloquence of the eye; they have never felt the power of an expressive and tender glance to delight and intoxicate the senses. And Saunderson, although a learned professor of optics, has never conceived why lovers of both sexes, on whom nature has bestowed fine eyes, are almost certain of pleasing.

From what I have just proved, it appears that the charm of a good pronunciation, and a sweet and sonorous voice, must be the ideal of beauty to the blind. It is so, when they have heard from a clear-sighted person that the lady in question is pretty; but if, on the contrary, they are told that she is plain, the sentiment of self-love quickly dissipates their illusion; consequently their notions of the physical beauty of individuals are only borrowed, and they ought to refer to a third person. In this they resemble those gifted with sight, who hear of some one whom they have never seen, and who are contented with what they are told, till an opportunity arises of judging for themselves. It sometimes happens that the blind do not ask for information on the beauty or the charms of the exterior of an individual whose voice, from its agreeable or repulsive quality, leads them to create an imaginary face which corresponds with it. The ardour of their imagination, frequently even extravagant and romantic, leads them to suppose the individual in question either a Venus or hideously ugly. The quickness with which the blind perceive the connexion between the sound of voice and the character of the individual, is really remarkable: those who are well informed, read the scale (if we may be allowed the expression) with a quickness of perception and a penetration rarely possessed by persons gifted with sight. The blind recognize deformed persons by the sound of the voice, which will not surprise those who have some knowledge of physiology; but what is still more extraordinary is, that very lately, in an evening party at L'Hôtel de Suède, at Brussels, a blind man was able to tell the age of every one in the room from the sound of their voices; and this with a precision which astonished all present. Amongst the party there were some pretty women, but the gallant blind man willingly deceived himself, and made them at least five years younger when he told their age. He knew that the fair sex approve of such errors. He recollected, without doubt, the beautiful woman of high rank, who preferred remaining a widow, as a second marriage would compel her to exhibit the date of her birth; and the famous coquette who lost her law-suit rather than shew the judge a paper which exactly proved her age.

* See Diderot's Letters.

Some blind persons have the four senses in such exquisite perfection, that if they hear any one speak they directly approximate his height; will find out the size of an apartment—if it contains much furniture; and at their own houses will discover if any article has been displaced. They know, by the remembrance of sounds, persons whom they have heard speak formerly. They can tell if they are in a street or a blind alley, in a large or a small room. They calculate the proximity of the fire from the degree of heat; the fulness of vessels by the noise made by the liquor when pouring into another cup; and the neighbourhood of bodies by the action of air on their face. Their hands are scales for weighing; they judge correctly of the duration of bad weather; recognize places where they have formerly been introduced. They feel correctly the vicissitudes of the atmosphere, and remark, by the different action of the rays of the sun on their face, when a cloud obscures its disk. All this is doubtless astonishing; but to walk alone in Paris is still more difficult. I know several who possess this audacious skill; amongst others, the intelligent Leré and the bold Manne. I recollect meeting them passing over the most dangerous crossings with only a stick to guide them *. I do not believe that the blind Du Puiseaux, or Saunderson himself, have done as much as these adventurous Frenchmen.

After having spoken of the talents of the blind, it now remains to examine the defects with which they are reproached: I have said that they have been much exaggerated, and I hope to prove the truth of what I advance.

First, they are accused of irreligion: this reproach appears to me unfounded. It is perhaps true that at church they have less fervour and less devotion than those who see; but may we not believe that the reason that they have less apparent piety, is that their imagination is not struck by the pomp and majesty of religious ceremonies? All the magnificence of the worship of the Divinity, this language, which speaks only to the eyes, loses its influence on the blind; and the pious exterior observed with so much art by the many, is a pantomime of which they must necessarily remain in ignorance. Three-fourths of the blind whom I have known (and the number is very great) were so far from being irreligious, that they felt more than others the necessity of the love of God, and their hearts sought with avidity for religious sentiments, which could alone console them under their misfortunes: but strongly as they are devoted to an enlightened religion, even more strongly do they abhor hypocrisy, for sincerity is one of their peculiar qualities; and they equally abhor cant and irreligion.

At the beginning of the Restoration, these calumnies were propagated in France, by some active impostors who had an interest in changing the disposal of the property of the Institution for the Blind. It is possible, also, that during the course of the revolution, this part of their education was not attended to sufficiently; and as they had been neglected in not teaching them some dogmas of their faith, some persons consequently concluded that the blind had no faith in religion. All must be learnt in this world, even virtue itself; and since such is the law of nature, of course the blind are not excepted from the general rule, as their minds, their hearts, and all their sensations, require more culture and more assiduous care than those of other human beings. There is, however, a singular quality which is innate amongst the blind—the horror they have of theft. This sentiment has been attributed to the difficulty they would experience in thieving, and to the natural anger they feel at being unceasingly exposed to the deceptions of others; but even supposing there is a little egotism in their manner of acting on this head, let us observe, however, that in examining the registers of the criminal courts, it is astonishing (considering the amazing number of blind now existing) not to find a single instance of one condemned for theft.

I have not yet mentioned all the faults assigned to the blind; amongst the accusations laid to their charge, I ought to place in the first rank the want of modesty with which they are accused, as well as the ingratitude, the want of sensibility, and the suspicion which they are supposed to possess: to the first charge I answer, that it is from ex-

* At Paris, in a very thick fog, some blind men of the Quinze-Vingts served as guides to those who saw, and even conducted them to their dwellings.

treme licentiousness that so much stress is laid on the appearance of modesty; that the Old Testament does not display that reserve in words which has been carried so far in our times; and, in short, that in Italy and at Rome, even the angels are unattired in the churches. Besides, the number of the blind who have been educated, is certainly not more than one in five hundred; therefore I confess that the great majority cannot have that sentiment of propriety which makes the charm of society; but are the lower classes generally distinguished for modesty? Is not ingratitude (the vice of little minds) common to this class of men? In regard to sensibility, if this native faculty appear less lively amongst the blind, it is because they are not moved by exterior demonstrations—because cries and complaints are necessary to make them acquainted with suffering, and to make them compassionate the sufferer; but they certainly are ignorant of those grimaces of etiquette, that parade of sensibility so happily called *sensitiveness;* they certainly do not go into convulsions on every trifling accident. It must be owned also that the word sensibility is very elastic; for one often sees a woman shed floods of tears at a theatre over an imaginary sorrow, and refuse on going out the smallest alms to a poor infirm old man. I knew a man of great sensibility, at least in his own opinion, who beat his groom in the most horrible manner. A moment afterwards he was miserable, and gave him a piece of gold by way of consolation. Other individuals go to criminal executions to sob in public; but the blind never display such scenes, because the sorrows addressed to the eyes are not felt by them. In regard to suspicion, with which the blind are reproached, this quality is in fact a necessary consequence of their position in society; they can never rely entirely on any individual; and as their moral vision is remarkably fine and subtle, they easily discern falsehood, and he who has once deceived them loses their confidence for ever.

Without dwelling longer on this subject, it appears to me that instead of exciting these clamours against the blind, all those who have the blessing of sight ought especially to defend them, and to display the greatest indulgence towards them; for be assured there does not exist, at least in this world, a greater misfortune than that of blindness. If I am not to be believed, ask those Belgian soldiers, 2000 in number, who are condemned to eternal darkness; they would have faced death without changing colour, but their courage could not support them under so great a misfortune without giving way to tears and despair.

If (as we are told) some precautions with regard to health, added to the suppression of neckcloths, would be sufficient to extirpate this ophthalmia, let us implore our king, who is a philosopher as well as a citizen, to put a stop to this devastating plague, and to assuage the terrible anguish of parents whose children are enrolled under his banners. If these lines should contribute to so great a blessing, how happy shall I feel to have snatched some victims from the calamity in which I am myself a fellow-sufferer.

Before terminating my letter I shall endeavour to give you an account of the manner in which the blind read, write, calculate, and understand geography. I shall in a few lines acquaint you with the methods invented by M. Hauy; those who wish to make use of them may examine these methods at leisure in the establishments for the blind at Paris, at St. Petersburgh, at Berlin, at Amsterdam, and even at my residence*, where I shall have great pleasure in communicating them, more particularly to my fellow-sufferers.

The blind write by means of a pen or stylet of iron. For this purpose they must have a table or board, upon which is fastened with hinges a frame of copper or iron, furnished with fiddle-strings, or thin slips of metal, placed from side to side at equal distances, so that they can write between the regulator, which is formed by two rods of metal, or two fiddle-strings. To write thus, you place on a sheet of leather a sheet of letter-paper: upon this you place some paper blackened with soot and grease: over all this a sheet of blank paper is laid. This operation finished, the frame is lowered, and they write with the iron stylet between the lines of the regulator. It is necessary to press very hard, to

* At Roneres, in Western Flanders.

form at once two writings, of which one is black, for the use of those who see, and the other white, and in relievo, serving as a copy, which the blind can read with their fingers.

To learn to read and to calculate, we make use of letters and figures of lead in succession, which are placed in holes regularly cut out in a board, either for reading or for calculation. With these leaden characters they print in relievo, and form books, which the blind read with the finger. The same method is used for music.

As for geographical maps, they are also in relievo. There are many modes of preparing them; the simplest is to sew a little circumference on the silk or limits of the different countries. All the maps which I *touched* at Paris, and in many other blind institutions, are imperfect; but thanks to the talents and the politeness of Mademoiselle Langlart and Madame Lefebvre-Muxaini, at Lille, I have at last perfected mine, so that now I recognize by the touch, towns, rivers, seas, the degrees of latitude and longitude, and even the scale to measure distances. As for playing cards, the signs are pricked on the card with a needle, and are so distinct that the blind man who is accustomed to them recognizes them as quickly as those who have eyesight.

As there exists a sort of brotherhood of misfortune between the dumb and the blind, I am often asked if I had rather be deaf and dumb than be deprived of sight. All blind men answer this question by declaring that they prefer the loss of sight. When the deaf and dumb are asked, they declare that they prefer their own infirmity. But observe, I beg, that the blind are much more cheerful than the deaf and dumb; these last are incontestibly much more out of society; their means of communication are a most fatiguing labour for those who have the use of their faculties, though the pantomimic action required is both curious and interesting. This is not the case with the blind. Conversation, that great charm of society, is open to them: to converse with them is not more difficult than to converse with the rest of mankind; besides, the intelligent blind man knows very well, that as he has one sense less, he ought to endeavour to possess higher moral qualities as a compensation.

I conclude, then, that the blind who are in circumstances sufficiently easy to subsist without any labour, are more happy than the deaf and dumb, especially as their children do not inherit their affliction, as is generally the case with those who are deprived of hearing and speech. But I must add, that for poverty it is better to be deaf and dumb, as those who are deprived of these powers very easily become mechanics: no handicraft is impossible to them, and under this head the comparison is all in their favour; but it is remarkable that the blind are longer-lived than the deaf and dumb. It will hardly be credited that persons thus differently afflicted can converse with each other. The blind man must learn their alphabet, and the signs that represent their ideas. By the help of these methods he writes in the air, or makes signs, which are comprehended by the deaf and dumb, who, in their turns, write upon the back, or in the hand, of the blind man. I use this method myself in conversing with a most intelligent man who is deaf and dumb, who was educated by the Abbé Sicard, and who has had private lessons from the celebrated Massieu. This interesting man is M. Lauwers, of Ostend.

I do not know if you are aware of an establishment for the blind in Holland, founded by Louis Bonaparte. Here we do not enjoy this blessing; but I venture to hope that our present august monarch, who pays such devoted attention to the instruction of his subjects, will permit the lights of science to illuminate the minds of the blind in Belgium, and to enable them to participate in the benefits of civilization.

I do not know if in this letter I have had the good fortune to convince you, and to inspire those who read it with some benevolent feelings for my fellow-sufferers. I have endeavoured to compensate by accuracy as to facts for the errors I may be justly reproached with in regard to style and purity of expression; but to support my opinions, and to shew that I have advanced nothing without proof, I will give you an account of some celebrated blind persons, and of some whose names, hitherto unknown, are nevertheless quite as worthy of our respect and admiration.

[To be continued.]

ON THE CAUSES AND TREATMENT

OF

PROLAPSE OF THE FUNIS IN LABOUR.

By R. T. Hunt,

One of the Surgeons to the Manchester Lying-in Hospital.

The frequent termination of those labours which are complicated with prolapse of the funis in the death of the child, not only when the case has been left to the unaided natural efforts, but also when every assistance that art could afford has been rendered, sufficiently evinces the importance of investigating the various circumstances under which this untoward accident occurs.

The present time appears favourable for such a communication, the publication of my colleague, Mr. Roberton's, remarks "On the Causes of Prolapse of the Funis in Labour*," having recently directed the attention of the profession to this subject. I should not, however, have ventured to obtrude any remarks of mine on the public, had I not, in consequence of my office as surgeon to the Lying-in Hospital, during nearly eight years, possessed considerable opportunities of investigating subjects of this description.

Many circumstances have been noticed as conducing to prolapse of the funis in labour; the chief of which may be arranged under the following heads:—

1. Unusual mobility of the fœtus in utero, dependent either upon its small size, the too great quantity of liquor amnii, or the too great length of the funis.

2. Presentation of a part of the fœtus which has not entered the brim of the pelvis in such a manner as to entirely occupy it at the time the rupture of the membranes and escape of liquor amnii occur.

3. Premature rupture of the membranes, and their rupture when the mother is in an unfavourable position.

4. Distortion of the pelvis.

5. The pelvis being considerably above the standard size.

6. Presentation of the abdomen.

7. Insertion of the placenta in the neighbourhood of the cervix uteri.

8. The funis becoming twined round some part of the child.

1. Unusual mobility of the fœtus in utero.

If a number of females at the full period of pregnancy, but anterior to the commencement of labour, are carefully examined by pressure with the hands on the abdomen, it will be found that the extent of motion of which the fœtus is capable varies very considerably. In some the head of the child can be distinctly felt in succession at the epigastric, the right and left lumbar regions, within a very short space of time; whilst in others the head seldom changes its position in utero, and with much less rapidity. This extent of motion in the former instances depends, according to Madame Lachapelle*, upon the small size of the child in relation to the uterus, the great quantity of liquor amnii, and the great length of the funis. Unusual facility of motion in the fœtus may be considered as predisposing to all those presentations which are termed preternatural, whilst the cause which in such cases determines the descent of the funis, viz. the untimely rupture of the membranes, may occur even in instances where the funis has not been previously found presenting between the head and os uteri, but is situated, in consequence of this mobility of the fœtus, within reach of the current of fluid passing into the vagina. I feel surprised that Mr. Roberton has omitted to notice this mobility of the fœtus as a cause of prolapse; for in one of his cases he says, "I found the funis pulsating in the vagina in a great number of short turns; these passed down, in contact with the promontory of the sacrum, and consequently *threw the head forwards on the pelvis*†." Much as I attribute to the motion of the fœtus, I certainly am at a loss to conceive how the soft funis can by its descent throw the head to either one part of the pelvis or another.

2. Presentation of a part of the fœtus which does not fully occupy the brim of the pelvis.

In presentations of the upper or lower extremities, the presenting part is so small in proportion to the size of the brim of the pelvis, that the funis may

* Med. Gaz. Jan. 9th, 1836, p. 558.

* Lachapelle; Pratique des Accouchemens, tome iii. p. 217. 1825.

† Med. Gaz. *ut supra*, p. 560.

easily present at the os uteri with it, and as the pains increase in force, the hand, foot, or other part of the presenting extremity, being frequently propelled against the membranes, which are always tense during uterine contraction, may thus cause the untimely rupture of the membranes, independent of any interference on the part of the attendant, or any unfavourable position of the mother, although either of these circumstances will accelerate the giving way of the membranes. These remarks are less applicable to breech presentations, in consequence of the size of this part more nearly approaching that of the head. All these presentations are referrible, in a greater or less degree, to unusual mobility of the fœtus in utero. Should the head enter the pelvis in an unfavourable direction—with its long diameter, for instance, corresponding with the short diameter of the brim—the length of time which must elapse before the head is, by means of the powerful action of the uterus, compressed into the brim, so as to occupy it fully, not only places the life of the child in imminent danger, but the imperfect adaptation of the head to the inlet in the meantime also materially conduces to the prolapse of the funis. Such a case is related by my colleague, Mr. Radford, in the Medical and Surgical Journal for January, 1834. In this instance the midwife had ruptured the membranes, in order to ascertain the presentation. The funis shortly prolapsed, and after the death of the child, the delivery was completed with difficulty by means of perforation. Mr. Radford attributes the remote situation of the presenting part of the child, in such cases, to the undeveloped condition of the cervix uteri, and deprecates the rupture of the membranes with the view of discovering the presentation. "If the membranes," he continues, "are ruptured before the uterus is prepared for action, the head will rarely enter the pelvis favourably; for previous to the sinking of the uterus and its contents into the superior aperture of the pelvis, the child is easily moveable in the waters. This floating of the child may be readily ascertained, when it becomes necessary to make an examination per vaginam, before the completion of pregnancy. But when the uterus has undergone the preliminary changes preparatory to active labour, the head of the child may be felt through the substance of the uterus, as a globular body, presenting considerable resistance to the finger. At this period it assumes its final position in relation to the pelvis*."

Mr. Roberton also refers, in the commencement of his paper, to these preparatory changes of the uterus, and their influence on the position of the head in the pelvis. He remarks, "If in this stage," (viz. the head having been located in its natural situation in the pelvis), "during a pain, the waters escape, then the head of the fœtus instantly descends into contact with the lips of the womb, and closes the opening." And further on he adds, "Such is the mode by which nature shuts the mouth of the womb during labour, whereby its floating contents, viz. the funis and remnant of the waters, are kept from escaping into the vagina †."

Now it appears to me, that if the head is properly located in the inlet, so as to occupy it fully, there is no necessity for this closure of the os uteri to prevent prolapse of the funis from taking place; and in those who have borne many children, the lips of the os uteri are frequently so loose that two fingers can easily be interposed between any part of them and the fœtal head, and yet no prolapse of the funis occurs. It is, then, this adaptation of the head to the inlet of the pelvis which prevents the funis from prolapsing, unless it has fallen down before the head has occupied the brim,—a circumstance which has already been noticed as depending upon unusual mobility of the fœtus.

3. Premature rupture of the membranes has been so long admitted as one of the causes of prolapse of the funis, that it appears unnecessary to notice this subject at length on the present occasion; and the same observation will apply to the influence of the position of the mother at the time of the rupture.

4. Distortion of the pelvis.

Several authors have noticed the connexion of prolapse of the funis with pelvic deformity. In such cases at least as long a time must elapse previously to the fœtal head properly occupying the brim of the pelvis, as in the instances of malposition of the head already noticed as conducing to prolapse.

Baudelocque* observes, "Le risque

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* Medical and Surgical Journal, p. 785.
† Medical Gazette, p. 558.
* L'Art des Accouchemens, édition 6me., tome i. p. 497. 1822.

que court l'enfant dont le cordon est sorti, n'est jamais plus grand que quand le bassin de la mère est un peu reserré, puisque la pression que doit y éprouver ce cordon est alors plus forte."

And Lachapelle * remarks, "Ainsi une obliquité quelconque de la tête et de la totalité du fœtus, de même qu'un obstacle dépendant de l'etroitesse du bassin, retenant cette tête au-dessus du detroit (surtout si les contractions de l'utérus sont assez faible pour ne point presser fortement la tête sur l'ouverture de ce detroit), permettent au cordon ou à une main de glisser aupres et jusqu'au dessous d'elle."

5. The pelvis above the standard size.

I am not aware that this cause of prolapse of the funis has been noticed by any author except Madame Boivin †. It is probable that in such a pelvis the funis and head will descend together; but the same circumstances which cause this descent, also favour the speedy delivery of the child; and consequently such cases of prolapse are less dangerous than any other.

6. Presentation of the abdomen.

Deneux ‡, in his excellent memoir on prolapse of the funis, adduces this cause for its occurrence, and cites a case of Delamotte's in support of his opinion. The rationale is too evident to require observation.

7. Insertion of the placenta near the cervix.

This cause also is mentioned by Deneux, together with cases from Smellie and Mauriceau.

8. The funis being twined round some part of the fœtus.

This cause is adduced on the same authority as the two last, Deneux §. He is of opinion that presentations of that part of the child round which the cord is twisted, favour its descent by carrying it near the os uteri. It appears probable that when the funis surrounds the neck of the child it is, generally speaking, in little danger of prolapsing. But, even when it is round the neck, the risk of prolapse will depend upon the position of the head in relation to the brim of the pelvis at the time of the escape of the liquor amnii, the length of that portion of the funis which is not twined round the neck, and the situation of the placenta. Deneux * relates three cases in which the funis prolapsed, although twisted round the child's neck. In one of these, which occurred to Mauriceau, the funis and one hand presented, together with the head, before the rupture of the membranes. In the two others the cord was folded several times round the neck, and yet prolapsed. After a careful consideration of the foregoing circumstances, I think Mr. Roberton is not warranted in remarking †, "It is true, there is another cause which often helps to keep the funis from escaping before the end of labour: I allude to its becoming twined round the neck, body, and limbs of the child." Although I have not met with any recorded case in which the funis was found surrounding any other part except the neck, yet the frequent occurrence of its prolapse, conjointly with presentations of the extremities, probably depends upon its having been twined round the presenting part previously to the descent of this part into the pelvis. The funis becoming subsequently disengaged anterior to or at the time the membranes rupture, and its consequent descent by the side of the presenting part, may be easily accounted for by the manner in which the cord floats in the liquor amnii, and the extent of space which, in such presentations, is left unoccupied in the brim of the pelvis.

Treatment.—The object of the treatment, in cases of prolapse of the funis, being to save the life of the child, the first question to be decided is—How is the death of the child caused? It was formerly supposed that the escape of the cord proved fatal in consequence of its being placed in a lower temperature than that to which it had been accustomed. We constantly meet with cases in which the pulsation of the funis continues for 15 or 20 minutes after the birth of the child, and where the funis is more exposed to the influence of cold than in any case of prolapse. The truth of this view cannot, therefore, be admitted. Deneux ‡ is the only author who seems to have given a correct ac-

* Lachapelle; tome iii. p. 217.

† Memorial de l'Art des Accouchemens, edition 3me., p. 262. 1824.

‡ Mémoire sur la sortie du Cordon Ombilical pendant le Travail de l'Enfantement. Journal Général de Medecine Française et Etrangere. Mai, 1820, p. 6.

§ Mémoire, p. 6.

* Mémoire, p. 7.

† Medical Gazette, p. 558.

‡ Memoire, p. 19.

count of this subject. He states, that the death of the fœtus happens in two ways—

1st. In consequence of its blood not being able to pass to the placenta, where the change necessary to the existence of the fœtus is performed, it then dies of a kind of asphyxia. This appears to be produced by that state of the fœtal circulation, which is caused by such pressure on the cord as entirely interrupts the flow of blood both in the umbilical vein and arteries.

2d. The circulation in the umbilical vein being stopped by a degree of pressure inadequate to cause its cessation in the umbilical arteries, this continuation of the arterial circulation has the same effect upon the child which hæmorrhage would produce, its system being drained of blood, whilst the umbilical vein no longer supplies the loss thus occasioned.

This appears to me the best method of accounting for the pale and exsanguineous appearance of those children who are still-born in consequence of prolapse of the funis. The apoplectic condition of the placenta in these cases is also noticed by Deneux.

The treatment at present adopted consists in—

1. Leaving the case to nature.
2. Returning the funis, and keeping it within the uterus and above the presenting part, either by means of the hand or instruments, until its re-descent is prevented by the progress of the labour.
3. Turning the child.
4. The employment of the forceps.

Of these methods turning is the one most frequently in use at the present day; and the mortality is so much greater than in the cases reported by Mauriceau and Baudelocque *, as to lead to the conclusion, either that this operation has been adopted in cases to which it was inapplicable, or that it has been performed in an improper manner. For information on this part of the subject, Denman (Waller's edition), Mauriceau, Lachapelle, Deneux, and particularly Dr. F. Ramsbotham, may be consulted.

The remarks of the last, contained in his practical lecture on this subject †, fully coincide with the view I have been induced to take.

The length of time the cord has been down, even after the rupture of the membranes, is no guide for our practice, which should be entirely determined by the effect produced by the pressure suffered, as indicated by the state of its pulsations with reference to their regularity, force, and fulness. I think it will appear, on examination of those instances in which the child has been born alive after turning, that the cord had not begun to suffer materially from pressure previously to this plan having been adopted. In Mr. Roberton's successful case * the funis had been down from ten in the evening until five the following morning, and yet he found it pulsating in the vagina. I fully agree with Dr. F. Ramsbotham, that turning is not only uncalled for, but also injurious in all cases in which we should not have had recourse to it, in consequence of the other presenting part requiring its adoption, independent of the prolapsed funis. The cord will undergo more pressure, even in head presentations, during delivery by turning, than if the labour is left to the unaided natural efforts. In both instances compression of the funis must take place during the passage of the head. Where the head descends naturally, can this pressure be more injurious than when it descends, as must be the case after turning, in an unfavourable direction? But, even if it is admitted that the descent of the limbs and body of the child relax the soft parts, which will, therefore, cause less pressure than if the head passed first, yet against this must be balanced the compression of the funis, caused by the introduction of the hand in utero, which, even in the most favourable cases, and where the greatest caution is used, must be considerable, if the liquor amnii has already escaped. To this must be added the pressure the cord sustains from the time the abdomen of the child descends to the outlet of the pelvis until delivery is completed. How many cases of hand or arm presentation prove fatal to the child where no prolapse occurs, in consequence of this compression of the funis I have just described?

Where the funis can be returned by the hand, and its relapse prevented, this

* In 26 cases, in which Mauriceau brought down the feet, 20 of the children were born alive. In Baudelocque's practice, out of 36 cases 25 terminated safely, in all of which either turning or the forceps was the means employed.

† Medical Gazette, Sept. 20, 1834, p. 867.

* Medical Gazette, present vol. p. 560.

method is always to be adopted. But the employment of instruments for this purpose carries with it the objection, that scarcely any instrument will be adapted for all the cases that occur, and prolapse of the funis being a rare case, we have not the opportunity of using these instruments so often as to adapt them properly for their intended purposes.

When the funis cannot be returned with the hand, and when it is not suffering from compression in the manner already noticed, by far the safest practice to the child appears to me the leaving the case entirely to the natural efforts. It is impossible for the funis, under such circumstances, to suffer an amount of pressure so great as that which is caused by turning; and it appears probable that in all the successful cases in which turning was adopted, the child would have lived if the case had been left to nature.

When the head presents in such a manner as to prevent the artificial return of the cord, and the pulsation is feeble, intermittent, or irregular, particularly if it ceases during the pains, and when there is every probability of the labour being speedily terminated by the use of the forceps, either long or short, no time should be lost in having recourse to them.

From the foregoing remarks on the cause of the child's death, it may, I think, be collected that the compression, which entirely stops both the arterial and venous circulation of the funis, is much less injurious than that which affects the umbilical vein previously to the arteries. From the state of asphyxia produced by the former description of pressure, the child more frequently recovers than from the exhaustion occasioned by the latter. After extracting the head, and cautiously disengaging the funis, if twined round the neck, the delivery of the child should not be completed by traction. And if, when the child is entirely expelled, no pulsation can be felt in the region of the heart, inflation of the lungs and the other means for resuscitation must be had recourse to. If, however, the slightest pulsation of the heart can be felt, the child will revive much better by deferring for 15 or 20 minutes the ligature and division of the funis.

Before concluding the subject of treatment, I must refer to a method entirely different from any here noticed, but which, I believe, has never yet been adopted. It is recommended by A. C. Baudelocque, nephew of the celebrated author of that name, and noticed by Deneux. This plan consists in applying a ligature to the prolapsed funis so as to entirely stop its circulation and place the child in a state of asphyxia. The view which Deneux gives of the cause of the child's death in these cases appears to have suggested this treatment.

Such are the observations which a very cursory investigation of the subject of prolapse of the funis has led me to make. It would have been difficult to account for such an omission in the standard obstetric authors as is implied by Mr. Roberton's introduction to his paper on this important subject—viz., "The manner in which this distressing accident takes place has never, so far as I know, been adequately accounted for." Before we complain of the inadequacy of explanations it is at least our duty to become acquainted with those already given; and when we adopt methods of treatment recommended by others, it is equally imperative upon us to acknowledge the sources from which our information has been derived.

Manchester, Jan. 29, 1836.

STATISTICS AND MORTALITY OF HOSPITALS.

To the Editor of the Medical Gazette.

Sir,

At the conclusion of the Report of Cases treated by Dr. Macleod at St. George's Hospital, a few remarks are offered on the anomalous fact, published in the British Medical Almanack for 1836, that at that hospital the proportion the deaths bear to the patients treated is higher than in any other hospital in England. "In the British Medical Almanack for 1836, a table is given," says Dr. Macleod, "which professes to illustrate the comparative rates of mortality in the different hospitals of the metropolis. By the table in question it would appear that at St. Bartholomew's the rate is only 7·6 per cent., while at St. George's it is 10·6."

Dr. Macleod has here fallen into the error of former writers on the statistics of hospitals, in comparing the mortality of different institutions without taking into account the mean *time each patient remained in the house.* Now it is manifest, that although 10 persons in 1000 may die in *one year*, and 20 in *two years*, the rate of mortality would remain absolutely the same; *double* the proportion would die in double the time. It is the column of the table in the British Medical Almanack next to the one cited by Dr. Macleod, which shews the real comparative rate of mortality in the London hospitals. Only 7·6 per cent. of the patients admitted into St. Bartholomew's Hospital died; but this was in 34 *days;* and 10·6 per cent. of the patients admitted into St. George's died in 43 *days;* so that in the *same time*—36·5 days, assumed as unity—the rate of mortality in both hospitals was the same, 8·1 per cent.* This is explained in the Almanack, page 116.

Only the proportion of deaths to the admissions is generally given; but as the time of residence varies from 28 to 102 days, and as the difference is still greater in some European hospitals, a comparison of the mortality in such circumstances is absurd: unless the *time* is reduced to some unity, the relative intensity of mortality cannot possibly be compared. For the ordinary tables of mortality the u ity of time is one year; 36·5 days is here preferred, because it is near the average term of residence; and being the tenth of the year, can be readily compared with the ordinary tables by changing the decimal place: thus the average deaths for 36·5 days in the twenty-one (county) hospitals are 4·12 per cent.; for 365 days, 41·2 in 100 constantly living.

Besides this, in comparing the mortality of different hospitals, it is necessary to take into consideration all the data referred to by Dr. Macleod, and others mentioned in the Supplement to the Almanack; but it should be recollected that the mortality of an hospital is a very constant quantity, fluctuating but little from year to year, and must therefore depend on invariable causes.

In order to determine the mortality of hospitals it is absolutely necessary to know their mean annual population. According to the British Medical Almanack, this essential information, published by several county infirmaries, has hitherto been withheld by the governors of the London hospitals. In the article alluded to, it is stated that "*the average number of patients under treatment in the London hospitals is never published;* there is, consequently, no check on the expenditure—no valuable statistical information whatever officially furnished by these great and wealthy establishments."

As the Governors of St. George's Hospital have no motive for secrecy, the medical men have no doubt the power, and I hope the inclination, to have one page of such useful facts added to the next Annual Report. It will well supply the poetical exaggerations which disfigure these documents, generally written by clergymen, who, of course, know little of the matter. The following are some of the points which I think they may publish with advantage:—

1. The average number of patients in the house, and the number admitted each month. 2. The ages of the patients, living and dying, in decennial periods. 3. The number of cases that remain 10, 20, &c. days under treatment, and the average term of residence. 4. The causes of death, at least, if not the cases of disease, classified.

Dr. Macleod would enhance the value of his Report by stating the *ages* of the patients, which can never be overlooked; and the duration of each case in days, weeks, or months.

I remain, sir,
Your obedient servant,
William Farr.

8, Grafton-Street, Fitzroy Square,
Jan. 26, 1836.

The healing art is peculiarly the physics of living nature; and the process of cure an eternal experimenting in the regions of life.—Hufeland.

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* The rate of mortality for the French hospitals is, I believe, correct; and for the hospitals of this metropolis the table in the Almanack appears to come very near the truth. Dr. Macleod states that the deaths at St. George's last year were 10 per cent. of the cases treated; for the previous years 10·6 per cent. is given in the Almanack. The mortality at St. Bartholomew's in 36·5 days is stated at 8·1 per cent.; and from an official report just published, I have calculated that the mortality for 36·5 days was during the last five years 8·0 per cent.

A CONCISE SUMMARY

OF THE

FACTS RELATING TO THE NATURE OF THE NERVOUS INFLUENCE.

By A. P. W. Philip, M.D. F.R.S. &c.

To the Editor of the Medical Gazette.

Sir,

It is necessary to preface the subject of the following paper by a few observations on electric tests; for writers, deceived by the name, speak of them as if they possessed a power equal to that of chemical tests.

There is, and from the laws of electricity we have reason to believe ever will be, an essential difference in the power of these tests. A correct chemical test will separate what we are in quest of from all substances with which it may be combined, and is therefore capable in all instances of detecting its presence, and consequently its absence also. This arises from there being but one counteracting power, that of affinity. If the affinity be stronger in the test than in any other substance, the effect of all other affinities is destroyed. We possess no such electric test, because here there may be other counteracting causes besides the power of affinity—opposing currents, for example.

Although electric tests, therefore, give evidence of the presence of electricity, we cannot by their means prove its absence, a fact with which we should not have been acquainted, were it not, under certain circumstances, possible to prove the presence of electricity without their aid; that is, the presence of electricity may, under certain circumstances, be proved, where it is not indicated by any of the properties generally admitted to be peculiar to it.

Suppose it were said, for example, that we cannot admit that electricity is the agent in the combination of oxygen and carbon, because there is no test by which its presence can be detected. The reply of Dr. Faraday, I conceive, would be, we cannot at present, whatever we may do hereafter, make the electricity employed in effecting this combination evident to any of our tests; but I consider its presence as a necessary inference, because I have adduced facts which prove either that electricity is the agent in such combinations, or that nature here deviates from the simplicity observed in all her other works. Either electricity is the agent in the combination in question, or there are two kinds of chemical affinity.

Under such circumstances can any other reply avail, except either disproving the facts, or pointing out the fallacy of the inference? What should we say of the reasoner who should tell us that the position is opposed to what he considers the best authorities, and give us, in refutation of it, dissertations on electricity, and on the properties of oxygen and carbon?

Of a similar nature with the foregoing is the position I have long maintained respecting the nature of the nervous functions, properly so called.

It appears from experiments repeated, as all my experiments were, in the presence of those capable of judging of the results, till no doubt remained in the mind of any of them, that the following are the nervous functions, properly so called:—

1. The excitement of the muscles of voluntary motion, in all their functions.
2. Of the muscles of involuntary motion, in some of their functions.
3. The maintenance of the processes on which animal temperature depends.
4. The formation of the various secreted fluids; and,
5. The processes of assimilation, by which the structure of our various organs is both effected and maintained*.

Of these functions the excitement of the muscles alone is the only one which may be supposed to be the effect of either a chemical or mechanical agent.

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* Much confusion has arisen from confounding, in consequence of the organs of both classes of functions belonging to the brain and spinal marrow, the functions above enumerated, with the sensorial functions, to which, neither in their nature nor their properties, they bear the slightest analogy. (See my Inquiry into the Laws of the Vital Functions, part ii. chap. xi.) I have been asked if I suppose that electricity can feel or think.

Similar difficulties have arisen from another inaccuracy, if possible still more at variance with all just views of the nature of the animal economy, to which I had occasion to refer in a late discussion, namely, confounding the nervous influence with the principle of vitality, implying a confusion of ideas to which it would be difficult to adduce a parallel.

In all the healthy functions of life, however, in which the muscular power is employed, the stimulus which excites it, if we except the mere power of distention, appears to be of the former description. Even that which maintains the peristaltic motion of the alimentary canal, which, remotely depending on the stimulus of the food, may at first view be supposed to be the effect of a mechanical agent, appears to be wholly of a chemical nature. The muscular coat of the stomach is not duly excited unless the food has been converted into a healthy chyme, the formation of which it appears, from direct experiment, depends on the healthy state of the influence supplied by the brain and spinal marrow. In like manner the healthy action of the intestines can only be maintained when their healthy stimulant has been duly prepared by the chemical processes which take place in the duodenum, which also depend on the influence supplied by the brain and spinal marrow.

Many circumstances may prevent the electric nature of the functions above enumerated becoming evident to our tests. A more powerful affinity, counteracting currents, or other less evident causes, may for ever prevent electricity leaving the nerves to affect them; but the facts being admitted—and the experiments on which they rest have been too frequently repeated to admit of any doubt respecting their results, namely, that after the nervous influence is withdrawn, all its functions, even the most complicated not excepted, are as perfectly performed by voltaic electricity, as by that influence itself—I say, these results being admitted, it is a necessary inference that either the nervous functions, properly so called, are maintained by voltaic electricity, conveyed by the nerves; or there are two distinct principles of action, the one of which is capable of all the functions of the other; an inference not merely at variance with all we know of the operations of nature in other instances, but inconsistent with the principles of our knowledge, because the only knowledge we possess of any power is that of its properties, which are consequently the only means by which it can be distinguished.

Such are the facts and the very simple train of reasoning by which I was led more than twenty years ago to the inference that electricity is the agent in all the chemical processes of the living animal; a position strikingly illustrated by the late investigations of Dr. Faraday, from which it appears that to the agency of the same power we must refer *all* chemical processes.

In the various functions of the living animal we find many of its powers identical with those of inanimate nature. All admit that its mechanical powers are of the same nature with those of the world which surrounds it. And why? because we at once perceive in them the same properties. Now, on what principle can we doubt the same identity in its chemical powers, which rests on precisely the same basis? That in the one instance, from the simplicity of the phenomena, the truth is self-evident; and in the other, in consequence of their more complicated nature, requires the aid of experiment to make it apparent, constitutes no difference in the nature of the proof.

To those whose minds have been exercised on such subjects, the foregoing positions will, I believe, appear conclusive. My late discussion with Dr. Williams, however, has not been without its use, because it shows all that can be brought in opposition to them, by a man both of talents and information*.

I might have enumerated, among the facts tending to illustrate the nature of nervous influence, one in an essential respect the most important;—I mean the success of the practice founded on the view I was led to take of it.

In certain diseases of the brain and spinal marrow, in which the nervous influence fails, voltaic electricity has been found to afford important relief, and that of a kind which cannot in any degree be effected by other means†.

I am, sir,

Your obedient servant,

A. P. W. PHILIP.

Cavendish-square, Feb. 1836.

* Why Dr. Williams requested that certain extracts from Dr. Davy's papers, very important, doubtless, as relating to the peculiar property of the animals called electric, should be published as a post-script to our discussion, does not appear, since as far as they relate to it, they only confirm my statement respecting the results of Dr. Davy's experiments.

† My Inquiry into the Laws of the Vital Functions, Part III.

ANOMALOUS CASE OF LITHOTOMY.

POUCH CONNECTED WITH THE PROSTATE.

To the Editor of the Medical Gazette.

SIR,

IF you deem the accompanying case worthy of admission into your valuable periodical, its insertion will much oblige, sir,

Your most obedient servant,

JOHN LIZARS.

38, York Place, Edinburgh,
Jan. 22, 1836.

A surgeon attached to a large hospital has peculiar opportunities for improvement, and he fails of his duty if he either neglect them or conceal the useful rssults of his observation and experience. Success, though it flatters vanity, is at best an equivocal proof of merit, for it may happen to the rash and unskilful. Successful cases, in ordinary circumstances. when published, afford but little information; cowardice may be unwilling to divulge the unsuccessful, but these are for the most part our proper instructors; from these we learn whether nature or art is the more to be blamed for any untoward event; but whether successful or unsuccessful, those cases are invaluable which lead to the detection of such morbid deviations as would certainly occasion the death of a patient in the hands of a timid or irresolute operator. Of this last class is the following case:—

James Brown, a healthy-looking man, fifty-nine years of age, entered the hospital on the 29th December last, and presented the usual symptoms of stone in the urinary bladder, under which he had laboured during the last eight months. He had been in the hospital seven months ago, under the late Professor Turner, who sounded him, but detected no calculus. The day after his admission, he was carefully sounded, but no stone was felt; the bladder was rough and fasciculated. He was ordered warm baths, leeches to the region of the pubes, the mistura aquæ potassæ, the uva ursi, and a seton over the pubes. By these means all irritation having been subdued, he was again sounded, and a stone distinctly perceived. A dose of castor oil was administered, and on the following day the lateral operation was performed.

All the preliminary steps having been taken, and the existence of a calculus again ascertained, a large staff was inserted, which could not be made to pass the prostatic portion of the canal. A similar staff was next employed, which apparently entered the bladder, as its handle was loose and moveable. One of my colleagues held it over the pubes, whilst I commenced, and cut down to the membranous portion of the urethra. I then proceeded seemingly through the left lobe of the prostate gland, which was hard, cartilaginous, and studded with calcareous deposition. The left forefinger, which guided the lithotomy knife, seemed to enter the urinary bladder, and a little fluid, considered to be urine, flowed out; when I begged the staff to be withdrawn. I next inserted a pair of forceps; but instead of a calculus such as the sounding had led me to expect, I discovered nothing but calculi, varying in size from that of a millet seed to that of a pea. I now used the searcher, but was not more fortunate. My finger felt a pouch equal in magnitude to a urinary bladder, which contained numerous small calculi. One of my colleagues, at my request, introduced his finger, and the sensation communicated so nearly resembled that of a mucous membrane, that he suspected I had wounded the rectum, but convinced himself of the contrary by examining that viscus with the forefinger of his other hand. Another of my colleagues was also requested to examine, and he, with a scoop, removed some of the small calculi already mentioned.

I now inserted a catheter, which passed the entrance of this pouch, got into the bladder, and urine flowed out. The catheter was replaced by a staff, along which the knife was carried through the neck of the bladder, as there was no substance like prostate gland, and a stone of the size of a flattened plum was instantly extracted.

The first incisions into the pouch occupied about one minute: the second incision and extraction of the calculus about another minute. From fifteen to twenty minutes were spent in examining this pouch.

The patient has had no bad symptom; no case of lithotomy ever went on more favourably; and this is now the tenth day from the operation.

The anomalous pouch, which rendered this case so complex, seems to me to have been nothing more than the external fibrous capsule of the left lobe of the prostate gland gradually dilated until it became as large as the bladder itself.

Crosse, in his work on Urinary Calculus, p. 34, says, "Concretions in the prostate gland, commencing in its ducts, often at a distance from their urethral orifice, even at the very bottom of a duct, go on increasing until each duct is enlarged into a pouch, rendering an escape of the concretion into the urethra impossible; the narrow orifice by which the pouch commu-

nicates with the urethra becomes often closed in consequence of inflammation and effusion of lymph; the pouch is a secreting cavity which furnishes additional deposit; and as the secretions enlarge or multiply, the pouch enlarges in the direction where there is least resistance towards the lateral or posterior surface of the prostate gland."—See Plates ix. fig. 1; and xi. figs. 2 and 3.

Wilson, on the Urinary Organs, at p. 353, also observes, " I have met with a urinary calculus, larger than a common-sized olive, in a cavity of the prostate gland, where, from the orifice which first admitted it having contracted, or the size of the calculus having enlarged, the stone could not be pressed back into the urethra, and the whole of the prostate gland had been changed into a capsule surrounding it."

I possess a preparation in my museum with cysts exterior to the urinary bladder, one of which may hold from four to five ounces: these communicate with the bladder. Another preparation, where the right lobe of the prostate gland forms one capsule.

I confess freely that I was not prepared for the complication just described, nor am I ashamed to confess it, since no mention is made of such an anomaly in the writings of the most eminent surgeons, if we except Crosse and Wilson, from whose works I have quoted above, but which I had not seen.

ANALYSES AND NOTICES OF BOOKS.

" L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

A Practical Treatise on Midwifery, containing the Result of Sixteen Thousand Six Hundred and Fifty-four Births, occurring in the Dublin Lying-in Hospital, during a period of Seven years, commencing November 1826. By ROBERT COLLINS, M.D., late Master of the Institution.

THERE is not, we believe, any medical appointment in the united kingdom, which offers such advantages in respect to experience in midwifery, as the mastership of the Dublin Lying-in Hospital. Had the predecessors of Dr. Collins exercised a tenth part of his diligence, we should now be passing rich in facts of great value to practitioners. As it is, we hope the present volume will excite the industry and emulation of the author's successors.

The number of births mentioned in the title-page seems prodigious; but it appears to fall considerably short of the actual number of deliveries, the results of which Dr. Collins witnessed during a residence of ten years in the hospital both as assistant and master. We are told in the preface that these amounted to 24,119.

The main feature of the work is statistical: the tabular views with which it abounds—conveying a vast quantity of information in a space comparatively circumscribed—must render it to practical men highly important and necessary for the purposes of reference. But at the same time we should add that its interest is well kept up, the dryness of statistical detail being judiciously relieved by the illustrative cases interspersed. A few of these we are tempted to extract. The first two are examples of fatal hæmorrhage occurring previously to the expulsion of the placenta.

Uterine Hæmorrhage terminating fatally.

CASE 1.—Ann Douglas was admitted into hospital, March 19, in labour of her 13th child. She was not delivered for 20 hours, during the greater part of which time her pains were very severe; her child was born alive. Fifteen or twenty minutes after, suddenly, a dash of blood took place from the uterus, not however to any unusual extent; the afterbirth had not yet been thrown off. We were sent for, and in less than five minutes reached the ward; she was very much debilitated; her pulse only to be felt at intervals, her countenance ghastly, her body and extremities quite cold, accompanied with a state of great restlessness and anxiety. After having administered some stimulants, the hand was passed into the uterus, which was found considerably distended and filled with clotted blood; part of the placenta was adherent to the fundus; it was easily separated; the uterus acted well, expelling both hand and placenta into the vagina, from which they were slowly withdrawn. The patient from this time lost *no blood;* the uterus remained firmly contracted; the pulse continued weak and fluttering, often imperceptible, particularly after vomiting; which is not usually the case with patients reduced by hæmorrhage after delivery; as this occurrence seems rather to rouse the patient and improve the pulse. She gradually became more exhausted, her respiration difficult, the power of swallowing almost

lost, and frequently so restless, as to be with difficulty kept more than a minute or two in the same position; which in all cases of uterine hæmorrhage is one of the very worst symptoms.

From the time of the removal of the afterbirth, which was at 1 o'clock P.M., till 11 P.M. she was watched by myself and assistants, Drs. Nicholson and Darley, with the closest attention, and was liberally supplied with cordials; having in the course of those ten hours taken at intervals not less than two-thirds of a bottle of spirit, burned, and mixed with a little water and sugar, besides more than a pint of port wine. All possible means were used to restore heat to the body and extremities, as warm flannels, jars filled with boiling water, hot bricks, hot stupes, &c., but in vain; at length, finding our efforts to produce any rally ineffectual, we determined on trying the effect of transfusion.

Having heated Read's apparatus, by injecting through it water at the temperature of 90°, it was filled with blood which was made to flow through the pipe previous to its being inserted into the patient's vein, in order, as much as possible, to exclude all air from the instrument. The blood flowed copiously from a healthy young woman, whom we selected for the operation, and was easily thrown into the median vein of the patient's right arm. It did not seem to have any more marked effect than that of causing the woman to mutter indistinctly; the circulation was not improved, though we injected about ten ounces of blood. She expired in a few minutes after the operation.

This woman's death seemed to have been principally owing to her state of constitution previous to the coming on of labour; being in a very debilitated condition, both from the number of children, viz. 13, she had given birth to, as also her continued exposure to hardship; as the quantity of blood lost would not have materially affected any young person, but in her, the *sudden* loss gave the constitution a shock from which it had no power to rally.

We rather think the transfusion hastened her death, though we all dreaded a fatal termination before it was resorted to.

CASE 2.—Mary A. Courtney, aged 19, was delivered of her 1st child (living) on the 3d of December, after a labour of seven hours. In half an hour after hæmorrhage came on, which continuing, and the uterus not contracting satisfactorily, the hand was introduced, when the placenta was found morbidly adherent, and requiring much exertion for its removal. When this was accomplished, the uterus contracted pretty well, the hæmorrhage ceased, and her strength improved. An opiate was then given. In 30 minutes a draining of blood was observed, attended with sinking, jactitation, &c., and she died in half an hour, in spite of every possible exertion.

On examining the body, the uterus was found relaxed and exsanguineous, but in other respects healthy; there was nothing else observed worthy of notice.

Among the cases of ruptured uterus or vagina is the following very curious one, complicated with

Vagitus Uterinus.

Was 36 hours in labour with her second child, (her first born alive.) The pains were at no time severe, nor did the head descend so low into the pelvis as to cause the face to turn into the hollow of the sacrum. Her pulse was quick; she seemed anxious and restless; her strength began to fail, so much so, that her pulse faltered; her countenance became ghastly; extremities cold and livid, with vomiting. One hour previously she had no alarming symptom.

She was immediately delivered with the crotchet. The hand was then passed to ascertain the extent of the laceration, (as there could hardly be a doubt of its having occurred;) but none could be satisfactorily detected. She never rallied. She died in 17 hours.

On dissection, a laceration was found in extent about two inches, anteriorly and to the left side, running from the junction of the uterus and vagina, upwards in a longitudinal direction, and confined to the muscular substance of the uterus, the peritoneum remaining uninjured. There was a considerable effusion of blood between this and the muscular substance, near the lacerated part. There was also a quantity of bloody fluid in the cavity of the abdomen. The intestines were exceedingly vascular. The most extraordinary occurrence in this case was the *respiration* and *crying* of the *child in utero*: both of which were heard, as distinctly as possible, four hours before delivery, the latter at a distance of some yards from the couch on which the patient was lying. These facts were witnessed by myself and assistants, besides several of the pupils, both by stethoscopic examination, and otherwise. The head was, at this time, high in the pelvis; the soft parts partially dilated, and the waters but a short time discharged.

The cry was so distinct, that I imagined the child was merely placed under the bed-clothes. When called to witness this truly singular phenomenon, I little cre-

dited the truth of what I was told, and confess, had I not been present, I should have remained sceptical.

How forcibly should this fact prove the uncertainty of some of the tests most confided in, as indicative of the murder of new-born infants! It also affords the medical witness in such cases, a salutary caution, in addition to those so ably advanced by the learned Dr. William Hunter on this subject.

These must serve as specimens (we are sorry we have not room for more) of the kind of information which is copiously given in Dr. Collins's work.

Some of our readers may wish to be made acquainted with the heads of the principal subjects contained in the volume. These are, the different kinds of labours, beginning with the natural: under the head of complex, we have hæmorrhage cases, convulsions, rupture of the uterus or vagina, prolapse of the funis, twins, and triplets; observations on the general mortality of the Dublin Lying-in Hospital, (which, by the way, is under 1 in 100 annually); puerperal fever; still-born children; and remarks on the deaths of children dying in the hospital.

We are satisfied that this work only requires to be examined in order to be as highly esteemed as it deserves to be. The scientific practitioner will find in it a treasure of facts.

Leçons de Chimie appliquée à la Médecine pratique, et à la Médecine légale. Par M. Orfila. 1836. Dulau.

The quantity of useful information which this little volume contains, we are satisfied cannot be procured in any English work of twice or thrice its size. The student, therefore, of chemistry and legal medicine in particular, who cannot read French, must labour under a disadvantage: for here is a manual peculiarly adapted to his use, if he only possess the means of being familiarly acquainted with it. We are sorry to entertain a supposition of the inability of *any* of our students to avail themselves of the valuable elementary works which issue from the presses of France and Germany: but we fear, notwithstanding the oft-repeated theme of our lecturers and writers on medical education, that the study of foreign languages is greatly neglected by the mass of English students. The loss we hold to be a serious one, and one which, we doubt not, many of our young friends will in after-life have to regret.

It is of course only a condensed abstract of Orfila's lectures which we have here; but the work of condensation and abridgment is well performed; and the result is what we consider to be an excellent little production, most useful to have at hand for immediate reference. It notices all the recent discoveries in chemistry, with their applications to practical medicine and toxicology.

MEDICAL GAZETTE.

Saturday, February 6, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

A WORD TO STUDENTS ON THE RECENT MEETING.

The remarks on the Court of Examiners at Apothecaries' Hall which we made in our last number but one, as well as our open, reiterated, and continued disapprobation of the latest and most harassing edition of their "Regulations," must have convinced every impartial person that we are as ready to express censure where we think it due, as we are steady in refusing to join the hue and cry of the demagogue, who finds fault alike where there is reason to do so and where there is not. Agitate, agitate, is his motto; and to convert this agitation to his own proper gain, is the secret but powerful spring, which regulates every thought, and word, and action. To those not blinded by passion the selfish and interested motives which led to the instigation of the late meeting of medical students appear naked and revolting. A candidate is rejected at Apothecaries' Hall, and without any inquiry—without the possibility of knowing any thing about his examination,

except from the young gentleman himself—the case is at once assumed as an instance of injustice and oppression; the parties are taxed not only with "vulgarity, rudeness, cruelty, insolence, heartlessness, grossness, and brutality," (and the manner in which those epithets are used shows how foreign to the writer's nature is the coarseness he denounces), but the Examiner, is charged with "ignorance,"—an ignorance the extent of which, we are informed, was proved by the "errors he committed" in the course of the examination. Now, setting altogether aside the *ex parte* statement on which this assertion is grounded, we put it to the common sense of any one (except the writer of the paragraph in question) if it be probable that a gentleman accustomed for years to the routine business of examination — one who spends every Thursday evening of his life in asking certain sets of questions, should not know whether the answers given to them be right or wrong? Such an Examiner might be rude, or petulant, or overbearing—and we fear there has too often been grounds for such charge—but when he is accused of *ignorance*, every unprejudiced person feels at once that the imputation results purely from the spirit of party. We have not appealed to the individual by whom this allegation was made; and simply for this reason, that he knows its falsehood as well as we do: but then he is aware that bold assertions, when in accordance with the feelings, are not always very scrupulously weighed. Young men are very naturally anxious and excitable on the subject of their examinations; they listen with eagerness to any imputation on the fairness or competency of their Examiners; and naturally indignant at the idea of injustice, easily become the tools of one who has long studied their sentiments and wishes, and even their prejudices, in order to turn them to his individual ends.

From this dishonest play upon their excited feelings, this unworthy tampering with their hopes and fears, we turn with indignation, and make our appeal to their judgment, confident in the strong reaction which has already been manifested, and strong in the consciousness of having no personal antipathies—no private interests—no political considerations—to mislead us. We tell the students fairly and boldly, but without disrespect or unkindness, that they have fallen into a great error—not in seeking the redress of any grievances of which they have to complain—but in their mode of doing so. From the moment that the medico-political adventurer of Finsbury was allowed to contaminate the proceedings by his interference, all moral weight which their resolutions might otherwise have had, was at once annihilated.

It is not fit that men devoted to scientific pursuits, and to a learned profession—a profession of gentlemen—should so underrate themselves as not to feel, and instinctively to shrink from, the moral contamination of such unwholesome presence. Neither ought the meeting to have been weakened by the attendance, much less ought it to have been presided over by one so palpably interested (even interested in a pecuniary point of view) as the private teacher of the rejected candidate. Such a petition as that which has been prepared—one so got up, and bearing such a signature, does not, and need not, give the Society of Apothecaries one moment's uneasiness. To neutralize it, no more is necessary than to put any member of the House of Commons in possession of the facts, and to get him to state in his place in parliament, that the meeting was convened under the patronage of

Mr. Wakley, and that the petition is signed by one person only — the "grinder" of the rejected candidate. This part of the affair was, indeed, a great mistake on the part of the students, and showed a plentiful lack of wit, or marvellous vanity, on the part of the Chairman.

Another point on which the students erred grievously, was assuming, on such imperfect evidence, that the rejected candidate was not deficient. The Editor of the Lancet, indeed, says, it was known "to the lecturer" that he was fully qualified; but we have heard a very different account, and dare him to the proof. It is easy to bluster about another examination, *because it is known that by the Act of Parliament this cannot be complied with*, and that six months must necessarily elapse before the party *can* receive a second trial. But we ask this—will the rejected candidate demand that the examination he has already undergone be published? We strongly suspect that he will not; but we are, at the same time, confident that after his open challenge, which, however, as we have said, the law renders but a mere bravado, the Court of Examiners are bound in self-defence to make known the grounds of his rejection.

It will thus be seen that, judging from all the information we have been able to collect, we entirely repudiate the idea of the Examiners being "ignorant," or the gentleman whom they rejected "fully qualified;" and that we utterly disapprove of the students meeting under the auspices they did. But this does not alter in any degree—no, not one single tittle—our yet unanswered allegation, that pupils, in their intercourse with the officers of the Worshipful Society, have sometimes been treated as if they were not gentlemen,—have met with rudeness at the very onset, ere yet their studies in London could be said to have commenced,—and that when they have gone up for examination, if found unprepared, they have sometimes had the bitterness of rejection increased by its being accompanied by censure—censure which, at such a moment, is unfeeling, even if deserved. This is not to be borne, and must be amended; it is a serious evil to the students in medicine, and it constitutes the triumph of the agitator, to whom it affords a foundation to build on; and a goodly superstructure of misrepresentation he has contrived to rear upon it!

But "coming events cast a shadow before," and, even as we write, the rumour of approaching change is whispered around us. What powers will be conferred on the medical department of the New University?—will the privilege be limited merely to conferring degrees in physic, or will it extend to the granting of licenses to practise as apothecaries?—will grades heretofore unknown in this country be established?—are we to have a lower qualification for Licentiates in medicine and in surgery, and a higher standard for Doctors in both these departments? Greater wonders than these have come to pass; but as yet they are in the womb of time, and the hour of delivery is not come.

PUFF PETITION

IN BEHALF OF THE PROFESSION.

A Mr. Baker, of 1, Dorchester Place, Hoxton, is taking a prodigious deal of pains to advertise himself in connexion with a petition to Parliament for procuring remuneration for medical witnesses attending coroners' inquests. We know nothing of this gentleman save what we gather from his productions. One of these, in the shape of a handbill, we thought it right to introduce to the notice of our readers, with remarks, some months since; and a richer specimen of the mode of customer-catching adopted in certain parts of the town, it was generally admitted, could not easily

be displayed. The petition now before us is so much a counterpart to the bill, that we are tempted to make an extract or two.

"*To the Honourable the House of Commons, &c.—The Petition of James Baker, &c. on behalf of himself and the Profession of which he is a Member,*

"Sheweth,

"That your petitioner having been called, on the 22d day of July, 1834, *to attend* Elizabeth Headhouse, whose body was lying in a long lonely road, *bounded the whole length* by a field of considerable extent on the one side, and the Regent's Canal on the other, at some distance from your petitioner's residence, at the hour of two o'clock in the morning, and in consequence of what your petitioner then saw and *ascertained of a suspicious character*, and *a man* who was taken near the place, having been detained in custody under suspicion of being criminally concerned in her death, your petitioner performed a post-mortem examination, and attended to give evidence at the inquest, on the 24th, when the investigation was adjourned, as the jury requested the contents of the stomach to be analyzed before they returned their verdict; and an application was made to your petitioner for that purpose, but your petitioner declined *the tedious and unpleasant duty* without a special order," &c.

So much for the opening of the case, which, we confess, we had to read more than once in order to form some conjecture of what Mr. Baker might probably mean. The length and complexity of the sentence (for the whole of which we have not room), we rather fear, will puzzle parliament not a little. But we pass on to more beauties:—

"That your petitioner, in compliance with such judicial injunction, and with a sincere desire to promote justice, in the presence of the coroner and jury *took* the contents of the stomach *for analyzation*, and the inquest was adjourned till the 28th, to give your petitioner time for the necessary processes and tests *in severance of the abstract specific matter connected with such a case* (!), which your petitioner with assiduity and scrupulous minuteness performed, and *attended thereon* at the adjournment."

And what was the modest demand made by our lover of justice "for the *severance* of the *abstract specific matter connected with such a case?*" Hear him once more:—

"That your petitioner, after the inquest had terminated, applied for the moderate sum of *three guineas* for his services, *although ten guineas is the customary fee.*"

We wish Mr. Baker would favour us, or "the profession of which he is a member," with his notes of this famous *analyzation*, which he considers cheap at *three* guineas, and for the like of which, he says, the customary fee is *ten*. We have no doubt that his publishing it would give him additional claims to attention.

But our petitioner is not selfish; he draws up a short prayer separately, for the profession in general, an echo of his own, and humbly asking Parliament—for what? for "*a clear and certain remedy!*"—an odd request, as it strikes us, for a medical petition to conclude with: if the business-members only cast their eyes to the foot of the document for the prayer, they must think we want reform in good earnest, when we ask of them for a panacea—some universal specific—"a clear and certain remedy."

To crown the matter, we are informed by Mr. Baker that the said "petition LAYS for signature" in several places which he mentions. Sir Boyle Roach once asked how he could be in two places at the same time—barring he was a little bird? But Mr. Baker's petition far surpasses Sir Boyle's bird; it can not only be in half a dozen places at once, but *lay* also in the said localities.

And this Mr. J. Baker is the person who takes up the cause of the profession single-handed, and attempts to thrust himself into publicity at their expense! We have not for a long time met with a better example of the adage, that "Fools rush in," &c. The cause, we are most ready to admit, is a good one; it is just and expedient that medical men should be remunerated when called upon to give their professional assistance in forwarding the administration of the laws; but many a good cause is lost by the interference of bungling and vain blockheads. In the present instance, besides, the meddling of such an officious party is wholly unnecessary: the County Coroner's Bill was lost in the last two sessions of Parliament only

in consequence of some trifling impediment; *the clause for fixing the remuneration of medical witnesses at 2l. or 3l. was actually carried*, and it only remains to bring it forward again this session, with some slight technical alterations, in order that it may become the law of the land.

We must therefore particularly request Mr. James Baker, of 1, Dorchester Place, Hoxton, to keep himself quiet; or if he needs occupation, to go to some respectable evening school in his neighbourhood, where he may acquire the rudiments of English grammar.

WORKING OF THE CONCOURS.

It is nothing new to find that what is plausible in theory, sometimes halts lamentably in practice. This appears to be remarkably the case with the concours, which some wiseacres are anxious to introduce among us. In France, where it has been long adopted, the experience of many years has been insufficient to contrive any plan by which the elections by *concours* may be freed from the charges of intrigue and favoritism; thus each vacancy which takes place affords occasion for a renewal of the most violent tirades against all concerned, coupled with the most unhesitating accusations of corruption and dishonesty. Nothing can exceed the virulence with which M. Orfila, as Dean of the Faculty, is at this moment assailed; and whether with justice or not, in either case the insufficiency of the present system to satisfy the public mind, is equally and lamentably proved. For heaven's sake let our contemporaries who are affected with this Gallo-mania look at what is going on in the French capital, and say if the *concours* be calculated to allay party spirit.

EGYPTIAN STUDENTS OF MEDICINE.

THEIR RETURN HOME.

The young men who were brought from Egypt by Clot-Bey, and placed at the medical school in Paris, for the purpose of pursuing their medical studies, have recently been sent back to their native country. They remained in France three years and a half, and are said to have made excellent progress in their studies. It was intended originally that they should remain a year longer, so as to have taken their degrees in medicine, but the circumstance of many medical men in Cairo and Alexandria having fallen victims to the plague during its irruption, has induced the government to require the immediate return of those who had been sent to Paris; and we cannot but hope that the enlightened views which they have there acquired will lead to important improvements in Egypt, now so rapidly emerging from the darkness which has hung over it for ages.

ORIGINAL PAPERS IN DIFFERENT JOURNALS.

The British and Foreign Medical Review, recently published, contains a "List of original papers published in the British journals during the last quarter." By this list, which is exclusive of lectures and hospital reports, the number of papers contained in each journal respectively is as follows:

Edinburgh Medical and Surgical Journal—*eight*.

Dublin Medical Journal—*fourteen*. (This, however, embraces two numbers of the journal).

The Lancet—*fourteen*.

The Medical Gazette—Forty-two.

Reckoning the number of persons by whom those communications have been made to the two latter, the account stands thus:

Contributors to the Lancet during the last quarter—*eleven*.

Contributors to the Medical Gazette during the last quarter—*thirty-seven*.

THE LATE DR. WM. TURTON.

In the death of Dr. Turton, the profession has to lament the loss of one of its most learned and estimable members. In the various departments of natural science (particularly in botany and conchology) we believe he had few or no rivals. The latter years of his life were spent at Bideford, Devon, where he died rather suddenly, on the 28th of December, 1835, in the 73d year of his age.

Dr. Turton was born at Tockington, in Gloucestershire, near Bristol: he had two brothers, both in the law (to which pro-

fession his father also belonged), and three sisters, the youngest of whom became the wife of the celebrated Lackington, the bookseller. Where the Doctor received his medical education we have not learned, but he graduated at Oxford, being a member of Oriel College. When Dr. Jenner's discovery of vaccination began to attract the notice of the profession, Dr. Turton was among the first to appreciate its advantages, and to proclaim the benefits to be derived from its general adoption. He was certainly, as we are informed, the first who introduced the practice of vaccination into Wales. This was in the year 1799; he lived at the time in Swansea; and it is worth adding, that the first subject on whom he tried the vaccine lymph was his own daughter, an infant, who was afterwards baptized by the name of Vaccina, in memory of the circumstance.

The disease of which the Doctor died, is said to have been a spasmodic affection of the heart. He was out, and perfectly well, on Saturday: on Sunday he complained a little, and died in the course of the night.

He was a man of mild and gentle manners, and of very retiring habits. He has left four children. Mrs. Turton, who also survives him, was the daughter of the Rev. Denny Cole, of Pettistree Hall, in Suffolk.

Among the works by which the name of Dr. Turton is best known to the public, are 1. A Medical Glossary, in which the words used in the various branches of medicine are deduced from their original languages, 4to. 2. Observations on Consumption, Scrofula, and some other forms of disease. 3. A General System of Nature; comprehending Animals, Vegetables, and Minerals, 7 vols. 4. An Edition of Goldsmith's Animated Nature, with Notes and Additions, 6 vols. 5. The British Fauna. 6. The Bivalve Shells of the British Islands; with 20 coloured plates. 7. A Manual of the Fresh and Salt Water Shells of the British Islands, arranged according to the modern systems of classification, and described from perfect specimens in the author's cabinet, with coloured plates of each species.

No fruit thrives without the dew of heaven; and this is not only the case with earthly fruits, but with those of a spiritual nature.—Hufeland.

The bitter principle is only a product of organic chemistry — that is, of life: thus without life there is no bitterness, morally or physically.—Hufeland.

LECTURES ON THE DISEASES OF THE NERVOUS SYSTEM.

By M. Andral.

As published in the *Gazette des Hôpitaux*, with the approval of the learned Professor.

HÆMORRHAGE OF THE NERVOUS CENTRES.

(*Affections of the Senses and Intellect, concluded from page* 700.)

When the sight of one eye is abolished, on which side does this take place? In one set of cases of this affection, the vision was lost on the same side as that on which the paralysis and loss of sensibility existed; while in another set of cases, the loss of vision was on the side opposite to the above; and the phenomena may be explained by a knowledge of the situation in which the effusion takes place, and of the influence which this is capable of exciting on the roots of the optic nerves, which, as is well known, are very numerous; some of them mingling without crossing, others actually crossing, and yet others running direct to their destination; so that according as the hæmorrhage has implicated this or that set of roots, the loss of vision will be direct or crossing.

Is it possible, when the sight is abolished, to determine *à priori* whether the extravasation be in the optic thalami, or tubercula quadrigemina? Theory replies in the affirmative. There are cases, certainly, in which vision is abolished, and yet the tubercula quadrigemina are intact. According to M. Serres, in order to produce blindness, it is necessary that the lesion be found in the commissure of the optic thalami; while the upper part of these ganglia may be implicated without this effect being produced. But blindness has been met with in cases where the hæmorrhage had occurred far from the optic thalami—in the cerebellum, for example, as, indeed, I have already stated.

In cases of extensive hæmorrhage, the pupil is dilated in proportion to the affection of the sight. The same considerations apply to the other senses: hearing may be abolished if the extravasation be very great; but it is only modified where the cerebral lesion is less considerable. The senses, too, may be affected in different ways, in certain lesions of the fifth pair of nerves, as I formerly mentioned in the *resumé* given in my first lecture.

The changes of sensibility, with their different forms, observed in hæmorrhage of the hemispheres, are also found in hæmorrhage of the mesocephalon, the symptoms of which are very speedily fatal. We have coma, and much depravation of sensibility. When the spinal cord is affected with hæmorrhage at its central part, there is loss of sensibility to a greater or less extent.

INTELLECT.

Let us adopt, with respect to the disturbances of intellect connected with cerebral hæmorrhage, the same divisions we have laid down in regard to the other functions. In the majority of cases, before the supervention of the apoplexy, it only affords negative symptoms. Nevertheless, in a certain number of cases, there is a perceptible sluggishness of the intellect for a longer or shorter time before the attack; labour becomes irksome, and there is a dulness which always leads to a preference of intellectual repose; this is attended by a tendency to sleep. In others, again, we remark a singular excitability, a constant desire of motion and change of place. Instead of having the intellect dull, as in the preceding cases, it is here morbidly active. In yet others, there are hallucinations resulting from an excited state of various senses—hallucinations, by the way, which are occasionally very remarkable, and to which I shall take an opportunity of again returning in detail.

Disturbances of the Intellect occurring at the moment of the Hæmorrhage.

1. It has been too often said that hæmorrhage of the nervous centres is always accompanied by loss of intellect. This is very far from being constant; and, indeed, the intellect may remain quite undisturbed.

2. In certain cases the intellect is diminished, without being abolished.

3. It may be entirely lost.

The cause of these differences is to be sought, I think, in the intensity of the disease rather than in its seat. Wherever the lesion is of small extent, the intellect remains entire; but if the lesion be considerable, there is some modification of the manner in which the brain elaborates thought. It has been a question whether this also holds good when the lesion exists in other parts than the brain itself? The answer is afforded by a certain number of cases of hæmorrhage of the cerebellum, in which the intellect was abolished. The thirty-two cases analysed in a previous lecture go far to show this, whether it be that this organ participates in some manner in the intellectual functions, or that its disease reacts sympathetically on the brain.

With respect to the mesocephalon, as all the cases of hæmorrhage of this part which have been observed have been very severe, and always accompanied by coma, so has intellect always been abolished.

In hæmorrhage of the spinal marrow the intellect is retained, and it is chiefly by lesions of sense and motion that the sympathetic symptoms of this malady are manifested. The exceptions to this, which are sometimes met with, are to be explained by the reaction of the spinal disease upon the brain; for every thing is connected in the animal economy, and not a molecule can be deranged in one part without influencing the others more or less. M. Fabre, in his thesis, has quoted the case of a patient in whom the anterior pyramids alone were the seat of hæmorrhage, without the slightest trace of extravasation elsewhere; and this very limited lesion was coincident with a complete abolition of intellect: in fact, the apoplexy had been as profound as it is seen to be in the most extensive hæmorrhage of the hemispheres of the brain.

Let us now resume the general history of hæmorrhage of the nervous centres. The hæmorrhage once completed, the blood no longer continuing to flow, either death must take place, or reabsorption be commenced.

If the hæmorrhage be great, the intellect is abolished, and this abolition remains permanent; coma is present, and this becomes more and more deep. If the hæmorrhage has been less active and more limited, the individual becomes roused, and resumes that communication with the external world which the hæmorrhagic attack had, as it were, cut off: he recovers a portion of his intelligence. In some cases, indeed, the intelligence regains all its power;—for example, men of letters have been known, after an apoplectic seizure, to return to their accustomed and cherished pursuits, without their works affording any evidence of their having suffered so terrible a malady as cerebral hæmorrhage: more frequently, however, there remain certain disorders, although they may not perhaps manifest themselves in ordinary intercourse; but if the patient attempt any thing requiring great or continued mental exertion, his inability to accomplish it becomes manifest; his brain is no longer what it formerly was, and can no longer act as before the attack; and this loss of mental power subsequent to the attack is sometimes conspicuous in those who did not shew it at the onset; it steals on progressively, by a gradually increasing weakness of the mind.

Others are still more unfortunate than the preceding, not being capable of even the business of ordinary life, and thus a state of childishness and imbecility succeeds to the attack, and always goes on increasing. Under such circumstances, if the patient be plagued, or employed about any thing which he dislikes, he very readily weeps, without the least appeal having been made to his sensibility. In others the diminution of the intellectual powers is not progressive, but takes place suddenly, the individual falling into a state of acute mental alienation. Cases of this kind are rare, but I am acquainted with some such. Febrile delirium comes on in others; and this state, which is distinct from insanity, shows that there exists a complication of the case with inflammation of the brain or its membranes. But the intellect may be affected not merely in the mass; the numerous faculties of which it is composed may be separately diseased. Thus it is not uncommon to meet with hemiplegic patients who exhibit their usual manner in conversation, and yet, if you ask them what they were about the day before, they cannot tell you: their imagination is entire, but their memory is gone. Sometimes it is not the *whole* memory which gives way, but only the recollection of particular things: thus sometimes they are only puzzled in finding substantives to express their ideas, the memory of which may be totally abolished. This, however, is a subject to which I at present allude only *en passant*, as I shall have occasion to allude to it hereafter.

We have seen that many persons lose the power of articulation, and this may depend upon modifications of the organs of speech themselves—as of the tongue, for example; but language is not formed by the tongue alone—it results from a special faculty of the brain; and man alone can speak, because he alone has such cerebral organization as is compatible with speech. Accordingly we may have, and actually do frequently meet with, cases in which the individual becomes dumb without the movements of the tongue having undergone the slightest modification, but depending upon a lesion in that part of the brain which regulates the co-ordination of ideas on which articulate language depends. We see, in proportion as we advance, that the functional lesions are insulated; a circumstance which proves the existence of a special seat for each in the brain: but I do not mean, by this, to say that the seat in question is known to us. On this point there is nothing but confusion and contradiction in science, and, as an example, let us take speech, and examine the facts which have served as the grounds of this attempt at localization.

M. Bouillaud, who has paid much attention to this subject, has inferred from his cases that the power of speech becomes modified when a lesion takes place in the anterior lobes of the brain. Now, on examining this doctrine, I propose to appeal to facts, just as I did in tracing the connexion between lesions of particular parts and paralysis either of the upper or lower extremities.

It appears, then, that in thirty-seven cases in which there was lesion of the anterior lobes, there was complete abolition of speech in twenty-one, while the faculty was retained in sixteen. It thus appears that lesion of the anterior lobes does not necessarily cause loss of speech; neither does it prove that they are not the seat of the function, because, in the sixteen where speech was retained, the lesion may have been slight. It would be requisite to ascertain how the speech is influenced where, the anterior lobes being intact, the posterior and middle lobes are diseased. In seven cases there was lesion of the posterior lobes, the anterior being unaffected; the power of speech was abolished. In seven other cases also speech was lost, although the anterior lobes were healthy, the lesion being in the middle and posterior lobes. An old woman was admitted into La Pitié, who had entirely lost the power of speech during two years; the movements of the tongue were quite free. She died, and I examined her with the utmost care. There was a small portion of softening in the centre of each hemisphere, on a level with, but exterior to, the interval which separates the optic thalami from the corpora striata, consequently it was in the middle lobe. There was absolutely nothing else. This case is of great importance in reference to the present question, for when the disease suddenly proves fatal, it may be said that if the anterior lobe was not actually implicated, there was, at least, a reaction upon it; but here this supposed reaction will not apply, because the case was entirely chronic. M. Recamier was led to suppose that the portion of the brain which presided over speech was the middle part of the centrum ovale of Vieussens; and other authors have assigned to it other seats: M. Serres, for example, has placed it in the corpora striata, M. Foville in the cornu ammonis, &c. These are evidently conclusions drawn prematurely from too limited a number of cases; for not only may speech be abolished with cerebral lesions the most different as to their seat, but the loss of speech may be coincident with complete integrity of the whole brain. M. Lal-

lemand has quoted a case where the lesion did not exist in the brain at all, but only in the cerebellum; the hemisphere of the brain did not present the slightest alteration, and yet the faculty of speech was abolished.

From all I have said, it follows, that we can only establish one solitary fact—namely, that the loss of speech is a frequent consequence of cerebral hæmorrhage; and although there must be some difference between the cases where the faculty is lost, and where it is retained, yet in what that something consists is yet to be discovered.

M. Cruveilhier, some time ago, saw a case in which the loss of speech coincided with a lesion of the tuber annulare, near the medulla oblongata; but this part of the question I shall have occasion to examine more particularly in the sequel.

Having now pointed out the modifications which the life of relation undergoes from hæmorrhage of the nervous centres, we shall next proceed to examine the influence which the same cause produces upon the actions of nutritive life.

COURT OF EXAMINERS OF THE APOTHECARIES' SOCIETY.

To the Editor of the Medical Gazette.

Sir,

The last paragraph of the very commendable and temperate resolutions passed at a meeting of the students at St. Thomas's Hospital (p. 704), requires some explanation. The words employed are—"That this meeting also strongly disapproves the principle of self-election and irresponsibility."

If by this it be only intended to express an opinion upon the principle, no objection can be made to the resolution: but it seems to imply something more—namely, that the Court of Examiners is a self-elected and irresponsible body. If the students at St. Thomas's have been led to believe this, they are misinformed.

When James I., in the year 1617, separated the Apothecaries from the Company of Grocers, with whom they had been incongruously associated, and incorporated these practitioners of medicine by the title of the *Society of Apothecaries*, he placed the executive power of the Society in the Master, Wardens, and Court of Assistants, amounting together to twenty-four members: and when, in 1815, the first Medical Reform Act was passed, commonly called the Apothecaries' Act, the power of electing twelve persons, duly qualified to constitute the Court of Examiners, was intrusted to this executive body (the Master, Wardens, and Court of Assistants), who are authorized and required to elect annually twelve members to compose the Court of Examiners: it is clear, therefore, that this is not a self elected court.

Neither is the Court of Examiners an irresponsible body; on the contrary, they are answerable for their conduct to the Court of Assistants: and if any well-grounded complaint was made to the Master, Wardens, and Court of Assistants, of any misconduct on the part of a member of the Court of Examiners, an investigation of such alleged misconduct would be assiduously entered into; and if good and sufficient proof was given of such misconduct, or if it could be shown that any member had been guilty of not "faithfully, impartially, and honestly, according to the best of his skill and knowledge, executing the trust reposed in him as an Examiner, without favour, affection, prejudice, or malice," agreeably to the oath which he solemnly swears before he is admitted to the office, he would be immediately suspended from his seat in the Court, and assuredly would not be re-elected.

Since the election of the first twelve Examiners, when the Court was established in 1815, twenty-one other persons have at various times been appointed members by the Court of Assistants, and in no one instance has an attempt been made to show that any person has been elected on the Court who was not a man of honour, respectability, and professional science.

A Friend to Medical Students.

Jan. 31, 1836.

MEDICAL ATTENDANCE AT CORONERS' INQUESTS.

PETITION FROM THE PRACTITIONERS OF BERKSHIRE.

To the Editor of the Medical Gazette.

Sir,

I take the earliest opportunity to inform you that at a Committee Meeting of the Berkshire Medical Association, holden *this day*, at the Reading Dispensary, the following petition was prepared for presentation to the House of Commons. I am directed to request that Robert Palmer, Esq. will do us the honour to present it, and also to furnish a copy to all the members in the county, with an earnest solicitation for their support.

I have the honour to be, sir,
Your obedient servant,
George May.

Reading, Feb. 2, 1836.

To the Honourable the Commons, &c.

The Petition of the undersigned practitioners, in Reading and its vicinity, and in other towns and villages in the county of Berks, humbly sheweth,

That your petitioners have been at all times willing to further the due administration of justice in their attendance on coroners' inquests.

That the duties which thus devolve on your petitioners are highly important and responsible; requiring the possession of extensive knowledge, and the devotion of much time and anxious labour.

That your petitioners are advised and believe that the present state of the law has not provided a remuneration for their services.

That your petitioners respectfully approach your honourable house, in the temperate but earnest language of complaint and remonstrance, humbly beseeching you to remedy this injustice, and to enact such equitable remuneration as to your wisdom may seem meet.

And your petitioners will ever pray, &c.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN JANUARY.

James Dixon, London.
James Carter, Witham, Essex.
John Pyle, Nether Wallop, Hants.
Horatio Weston, London.
Thomas Norris, Wigan.
Charles Spencer, Chippenham, Wilts.
George F. Marshall, Wallingford.
Alfred Baker Cutfield, Deal.
Philip O. E. Baines, E. I.
William Jenkin, Penzance.
Stephen D. Crawford, E. I.
Abraham C. Gall, Woodbridge, Suffolk.
Giles R. Burt, Bridport, Dorset.
George Toynbee, Eckington, Lincoln.
Alfred H. Vallack, Kingsand, Devon.
Thomas T. Campbell, Burton Crescent.
Richard W. Norris, Liverpool,
Charles H. M. Mules, Ilminster, Somerset.
William T. Dalby, London.
George A. Cope, Melbourne, Derby.
Robert C. Edwards, Lyme-Regis.
W. Sheldon Sweeting, Bridport.
Charles Clark, Saintfield, Devon.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

January 28, 1836.

Edward Smiles, Newcastle-upon-Tyne.
Thomas Buck, Holwell, Leicestershire.
Charles William Coverton.
Philip Burnard Ayres, High Wycombe.
William Thomas Hudson, Cambridge.
Woodland Wyatt Wardrofer, Arundell.
William Marten Cooke, London.
William Wogan Baynes, Adstock, Bucks.
William Nickols.
Robert Hunter Semple.
John Potts, Sunderland.
Edward Welchman, Birmingham.
Thomas Land, Tiverton.
Samuel Marshall, Marnham, Notts.
John Frederick Abram, London.

February 4.

Hugh Davies, Maesgamedd Corwen.
Joseph Aldridge Bond, Polesworth.
Robert John Bell, Reednett, Yorkshire.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Feb.* 2, 1836.

Age and Debility	51	Inflammation	24
Apoplexy	6	Bowels & Stomach	3
Asthma	18	Lungs and Pleura	7
Cancer	1	Insanity	3
Childbirth	4	Jaundice	1
Consumption	58	Liver, diseased	3
Convulsions	30	Measles	2
Croup	2	Mortification	4
Dentition or Teething	1	Paralysis	3
Diarrhœa	1	Small-pox	15
Dropsy	15	Sore Throat and Quinsey	1
Dropsy on the Brain	10	Spasms	2
Dropsy on the Chest	3	Thrush	1
Fever, Scarlet	2	Tumor	1
Gout	4		
Heart, diseased	3		
Hernia	1	Stillborn	5
Hooping Cough	2		

Decrease of Burials, as compared with the preceding week . . . 97

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Jan. 1836.	THERMOMETER.		BAROMETER.	
Thursday . 21	from 29	to 41	29·95	to 29 68
Friday . . 22	29	47	29 52	29 54
Saturday . 23	43	54	29·38	29 73
Sunday . . 24	38	49	29 92	30 01
Monday . . 25	30	43	30·22	30 28
Tuesday . . 26	29	46	30·17	30·05
Wednesday 27	35	46	30·01	29·84

Prevailing wind, S.W.

Except the 24th, generally cloudy, with a little rain at times.

Rain fallen, 1 of an inch.

Thursday . 28	from 38	to 49	29 73	to 29·42
Friday. . . 29	31	42	29·45	29 05
Saturday . 30	28	41	28 98	29 43
Sunday . . 31	28	47	29 49	29 23
Feb.				
Monday . . 1	35	44	29·21	29.25
Tuesday . 2	38	40	28 93	28·65
Wednesday 3	32	39	28·83	29.32

Prevailing winds, S.W. and N.E.

Except the 30th ult., and the 1st instant, generally cloudy, with frequent showers of rain.

Rain fallen, 1 inch, and ·025 of an inch.

CHARLES HENRY ADAMS.

NOTICE.

MR. MEADE.—We have received a coarse and vulgarly abusive letter from this gentleman, apparently designed for publication: any temperate reclamation we should insert, but not an impertinent tirade like that before us. We commend Mr. M. to his friend's journal—the Lancet—for the pages of which his composition is exactly suited.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 13, 1836.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

By Jon. Pereira, Esq., F.L.S.

Lecture XX.

ON AMMONIA AND ITS PHARMACEUTICAL COMPOUNDS.

The next substance obtained by the decomposition of animal matters to which I propose directing your attention is *Ammonia.*

History.—The term *sal ammoniacus* is met with in the writings of Dioscorides and Pliny, but some of our most learned authorities are of opinion that it refers to common salt. The erudite Beckmann says—"I am convinced that the *sal ammoniacus* of the ancients was rock salt, and not our *sal ammoniac.*" I think it is not unlikely that the two substances may have been confounded under the same name. The first distinct traces of the salt which now bears this appellation, are to be met with in the writings of Geber, an Arabian, who lived during the eighth century, but it seems to have been quite common in his time. Ammonia, in its gaseous and uncombined state, was first obtained by Dr. Priestley in 1773.

Synonymes and etymology.—The substance (whatever it may be) which the ancients termed sal ammoniacus, is, by some, stated to have derived its name from *Ammonia*, a district of Libya, where the oracle of Jupiter Ammon was situated; or rather, according to Pliny, "quia sub arenis inveniatur,"—αμμος, signifying sand. Few substances have had a greater number of synonymes than ammonia, some referring to the sources whence it may be derived—for example, the terms *spirit of urine, spirit of sal ammoniac, spirit of hartshorn*, and *bone spirit*: some founded on its properties, as *volatile alkali* or *alkaline air*: others, lastly, referring to its composition, of which *azoturetted hydrogen* is an instance.

Native state.—It is found in both kingdoms of nature—the inorganic and organic. Muriate and sulphate of ammonia (the latter called by mineralogists *mascagnine*) are met with native in the neighbourhood of volcanoes. Ammonia-alum, found in Bohemia, is likewise another native ammoniacal salt. Chevallier has recognized ammonia in many native oxides of iron, and in the chalybeate waters of Passy. It is one of the bases found in urine, where it exists in combination with phosphoric and muriatic acids, and, according to Prout, with uric acid also. It is developed during the putrefaction of organic substances containing nitrogen. Free ammonia has been detected by Chevallier in some plants—for example, *chenopodium vulvaria, sorbus aucuparia*, &c. Raspail considers that the varieties of the glutinous and albuminous tissues are composed of *organic atoms* (each consisting of an atom of carbon united with an atom of water) combined with *ammonia* as a base; while in woody fibre the organic atoms are combined with a fixed base: so that the nitrogen, described by chemists as forming an elementary constituent of gluten and albumen, is, according to this notion, in combination with hydrogen.

Production.—Ammonia is primarily obtained either from the decomposition of animal matters, or in the manufacture of coal gas. I shall very briefly allude to the first, though I believe nearly all the ammonia of commerce is now obtained from the latter source.

(*a*) *Production of muriate of ammonia by the destructive distillation of animal matters.*—Bones, or refuse animal matters of any kind, are distilled in a cast-iron cylindrical retort or still. The products of the operation are a *brown liquor* containing carbonate of ammonia, an *empyreumatic tar-like oil*, and a quantity of fœtid inflammable *gas*. By mechanical means the greater portion of the oil is separated, and the residual liquor constitutes what is denominated *rough bone spirit;* by a second distillation at a more gentle heat, nearly all the residual oil is removed, and the distilled liquid constitutes the *spirit of hartshorn*—that is, an aqueous solution of carbonate of ammonia, with some empyreumatic oil. The rough bone spirit, however, is usually digested on ground plaster of Paris (sulphate of lime), and by double decomposition there are produced sulphate of ammonia in solution, and insoluble carbonate of lime. The liquid, containing the ammoniacal sulphate, is concentrated, and common salt added; by evaporation, double decomposition takes place, sulphate of soda and muriate of ammonia being the results: the first crystallizes, while the second remains in solution in the mother water. This is evaporated until the muriate of ammonia crystallizes, when it is purified by sublimation in earthen jars. In Aikin's *Dictionary of Chemistry* will be found a detailed account of this operation.

The *theory* of the process is not difficult to comprehend. Animal matters (as the gelatine of bones) are composed of carbon, hydrogen, oxygen, and nitrogen. By heat in closed vessels these elements enter into new combinations. Thus, some of the oxygen and hydrogen unite to form *water;* carbon and oxygen, combining in different proportions, furnish *carbonic oxide* and *acid:* carbon with hydrogen forms *carburetted hydrogen;* while the nitrogen, uniting with hydrogen, produces *ammonia*, which, with the carbonic acid, forms carbonate of ammonia: carbon and hydrogen, with a little oxygen, constitute the *empyreumatic oil.* By the action of the sulphate of lime on the carbonate of ammonia, we obtain sulphate of ammonia and carbonate of lime.

Re-agents.	Results.
$Am + \ddot{C}$	$Am + \dddot{S}$
$\dot{Ca} + \dddot{S}$	$\dot{Ca} + \ddot{C}$

The sulphate of ammonia is then decomposed by the common salt, and by the aid of water, we get sulphate of soda and muriate of ammonia.

Re-agents.	Results.
$Am + \dddot{S}$	$Am + H\ Chl.$
$\dot{H}$	$\dot{Na} + \dddot{S}$
$Na\ Chl.$	

(*b*) *Production of muriate of ammonia in the manufacture of coal gas.*—In the distillation of coal, as practised at the gas-works, the volatile matters are conveyed to a condensing vessel, or refrigeratory, in which tar and an ammoniacal liquid are deposited. This liquor contains several ammoniacal salts, as the sulphate, carbonate, hydrocyanate, and hydrosulphate. By the addition of sulphuric acid these are converted into the sulphate, which is obtained by evaporation, in the form of brown crystals: these are sublimed with common salt in order to furnish the muriate of ammonia. The *theory* of the process is as follows:—Coals consist essentially of carbon, hydrogen, nitrogen, and oxygen, and there is also usually present more or less of sulphuret of iron; so that, you observe, there are all the elements in coal to furnish the ammoniacal salts just mentioned. The sulphate of ammonia obtained by saturating the ammoniacal liquid with sulphuric acid, is decomposed by the common salt, and by the aid of a little water the products obtained are muriate of ammonia and sulphate of soda, as in the last mentioned process.

The sublimation is carried on in an iron pot lined with clay: the head of the pot being perforated in the middle. After the operation, there are found imbedded in the clay beautiful and perfect crystals of the bisulphuret of iron.

Sometimes sulphuric is substituted for muriatic acid in saturating the ammoniacal liquid, and, by evaporating, crystals of rough muriate are obtained, which are sublimed in the iron pot, as in the other process, but without the addition of common salt.

1. *Of Ammoniacal Gas.*

Preparation.—This gas is procured by mixing equal parts, by weight, of muriate of ammonia and quick lime, introducing the mixture into a retort, and collecting the gas over mercury; or, in the absence of a mercurial apparatus, we may generate the gas in a Florence flask, and, by means of a tube curved twice at right angles, collect the evolved gas in bottles by displacing the atmospheric air.

The *theory* of the process is as follows:—One proportion of the muriate is acted on by one proportion of lime, giving rise to

one of ammonia, one of water, and one of chloride of calcium.

Re-agents.	Results.
Am + H Chl.	Am.
$\dot{\text{Ca}}$	$\dot{\text{H}}$
	Ca Chl.

Physical and chemical properties.—It is a colourless, invisible gas, having a strong and well-known odour; it reddens turmeric paper, but the application of heat restores the original yellow colour. It is decomposed by heat into its constituents, hydrogen and nitrogen, every two volumes of ammonia yielding three volumes hydrogen and one nitrogen: mixed with oxygen it forms an explosive mixture.

Before explosion.		Results.	
Amm. =17	Ox. =16	Nitr. =14	and Water =27
	Ox. =8		

You will observe from this diagram that two volumes of this gas require one volume and a half of oxygen for its complete combustion. Chlorine acts powerfully on ammonia, producing muriate of ammonia and nitrogen. With the vapour of muriatic acid, it forms copious white fumes of the muriate of ammonia. The specific gravity of this gas is about 0.59.

Characteristic tests.—Its odour, its action on turmeric, and the white fumes with muriatic acid, readily distinguish it. When dissolved in water, the solution may be recognized by other characters to be noticed presently.

Physiological effects. — Ammonia is a powerful local irritant. This is proved by its pungent odour, and its acrid and hot taste — by its irritating the eyes, and, when applied for a sufficient length of time to the skin, by its causing vesication. If an attempt be made to inhale it in the pure form, spasm of the glottis comes on; when diluted with atmospheric air, it irritates the bronchial tubes and larynx, and, unless the quantity be very small, brings on inflammation of the lining membrane. Nysten injected some of this gas into the veins of a dog: the animal cried out, the respiration became difficult, and death soon took place. Neither gas nor visible lesion was observed in the heart, the two ventricles of which contained liquid blood. In another experiment he threw ammoniacal gas into the pleura of a dog: cries, evacuation of urine, and vomiting, immediately followed; soon afterwards convulsions came on, and continued for several hours; ultimately they ceased, and recovery took place.

In almost all cases of poisoning in animals, by ammonia, or its carbonate, convulsions are observed, apparently shewing that these substances act on the spinal marrow. The proper medicinal or therapeutical effect of these preparations on the system in general will be examined under the head of the solution of ammonia.

Uses.—Ammoniacal gas is rarely employed in medicine. Bourguet de Béziers employed it in the case of a child affected with croup, to provoke the expulsion of the false membrane.

Antidotes.—Inhale the vapour of hot vinegar, muriatic acid, &c.

2. *Solution of Caustic Ammonia.*

General remarks, synonymes, &c.—Ammoniacal gas and water have a strong affinity for each other. This may be readily proved by opening a phial filled with the gas under water: the liquid rushes up with such violence as sometimes to fracture the bottle. The quantity of the gas which water can dissolve varies with the pressure and temperature. Sir H. Davy says, that at a temperature of 56°, and under a pressure equal to 29·8 inches of mercury, water absorbs 670 times its volume, and becomes of specific gravity 0·875. Such a solution would contain 32·5 per cent. of its weight of gas.

In the Pharmacopœia we have an officinal solution much weaker than the one here alluded to; it is denominated *liquor ammoniæ*.

Preparation.—It is prepared by distilling a mixture of quick-lime, muriate of ammonia, and water. The sulphate might perhaps be manufactured at a cheaper rate, and would answer equally well. The *theory* of the process is simple: the lime abstracts the muriatic acid, and the ammonia distils over with some water. By the mutual action of the muriatic acid and lime, we obtain water and chloride of calcium. These symbols will explain the decomposition:—

Substances employed.	Results.
Am + H Chl	Am
$\dot{\text{Ca}}$	Ca Chl
	$\dot{\text{H}}$

Physical and chemical properties.—It is a colourless liquid, having the smell and alkaline properties of the gas already described. Its specific gravity is 0·960. Here is an extract from Sir Humphry Davy's table of the quantities of ammoniacal gas in solutions of different specific gravities:—

Specific Gravities.	Per centage quantity of Ammonia.
0·9572	10·82
0·9597	10·17
0·9619	9·60
0·9692	9·50

You will observe from this table, that a solution having a specific gravity of 0·960 must contain very nearly 10 per cent. of ammonia; so that the officinal solution will consist of—

Ammonia	10	or 1 atom	17
Water	90	or 17 atoms	153
	100		170

Characteristic tests.—Solution of ammonia is characterized by its odour, its temporarily reddening turmeric, its fuming when a rod moistened with strong liquid muriatic acid is brought near it; by its producing a deep blue solution when added in excess to a solution of the salts of copper; and, lastly, when chloride of platinum is added to it, a yellow precipitate (*chloro-platinate of muriate of ammonia*) is produced.

Impurities.—Liquor ammoniæ frequently contains traces of the carbonate, which may be detected by lime-water, or by a solution of the chloride of calcium, either of which occasions a white precipitate of the carbonate of lime. When a portion of the liquid has been neutralized by pure nitric acid, it ought not to cause a precipitate by the addition of the nitrate of silver, or oxalic acid; for the first would indicate the presence of muriatic acid, or chlorine; the second, of lime.

Incompatibles.—It is hardly necessary to say that all acids are incompatible with ammonia. With the exception of the salts of potash, soda, lithia, lime, baryta, and strontia, ammonia decomposes most salts.

Physiological effects. — Let us examine these under the two heads of local and remote effects:—

(*a*) *Local effects.*—In the concentrated form the local action of liquor ammoniæ is exceedingly energetic. Applied to the skin it causes pain, redness, vesication, and destruction of the part; thus acting first as a rubefacient, then as a vesicant, and lastly as a caustic or corrosive. Its emanations are also irritant: when they come in contact with the conjunctival membrane, a flow of tears is the result; when inhaled, their powerful action on the air-passages is well known. You observe persons in syncope, almost immediately raised from a death-like state, merely by causing them to inhale the vapour of this solution. Whenever you employ it in cases of insensibility, bear in mind that you are using a powerful local irritant; and unless administered with caution, you may give rise to most serious, or even fatal consequences. Nysten tells us that a physician, for some years subject to epilepsy, was found by his servant in a fit. In order to rouse his master, the latter applied a handkerchief moistened with this solution to the nose, so assiduously, that he brought on bronchitis, of which the physician died on the third day. In the Edinburgh Medical and Surgical Journal there is the report of the case of a lad whose death was produced, or at least hastened, by an attendant applying "with such unwearied but destructive benevolence" ammonia to the nose, that suffocation had almost resulted. Dyspnœa, with severe pain in the throat, immediately succeeded, and death took place forty-eight hours afterwards. A French physician also suffered ulceration of the mouth, and violent pulmonary catarrh, in consequence of the excessive use of ammonia, given as an antidote for hydrocyanic acid.

I have referred to these cases with the view of strongly impressing on you the necessity of exercising great caution in the application of this agent.

When the liquor ammoniæ is swallowed in large doses, it acts as a powerful corrosive poison. Fortunately we do not meet with frequent examples, so that the symptoms and appearances are rather assumed from analogy than from any modern well-marked cases in the human subject. However, it is very evident that violent inflammation of all that part of the alimentary canal with which the poison may be in contact, would be the result, and that if much be taken, decomposition of the part might be expected. When swallowed in a very dilute form, and in small quantity, the local phenomena are not very marked, and the effect of the substance is then seen in the affection of the general system.

(*b*) *Remote effects.* — Let us investigate these under two heads, according as they are produced by small or large doses:—

(*α*) *Effects of small, or therapeutic doses.*—When administered in those quantities we are accustomed to employ in the treatment of diseases, ammonia acts as a stimulant, excitant, or calefacient. It produces a sensation of warmth in the mouth, throat, and epigastrium, frequently attended with eructations. The pulse is soon rendered quicker and fuller; the heat of the skin is sometimes increased, and there is a tendency to sweating, which, if promoted by the use of warm diluents and clothing, frequently terminates in copious perspiration. But the skin is not the only secreting organ stimulated to increased exertion; we observe the kidneys produce

more urine, and frequently the quantity of bronchial mucus is increased. The nervous system is also affected. Wibmer has made several experiments on himself, and from them it appears that ammonia affects the head, sometimes causing oppression, or a sense of fulness, but no pain.

The increased capability of muscular exertion, the excitement of the mental functions, and the greater facility with which all the functions are executed, are further indications of the action of ammonia on the nervous system.

(β) *Effects of large, or poisonous doses.*—I cannot better introduce this subject, than by reference to some experiments made by Orfila. He injected 60 grains of liquor ammoniæ into the jugular vein of a strong dog; tetanic stiffness immediately came on, the urine passed involuntarily, and the animal became agitated by convulsions: death took place in ten minutes. The body was immediately opened, when the contractile power of the muscles was found extinct.

In another experiment 36 grains of concentrated solution of ammonia were introduced into the stomach, and the œsophagus tied: in five minutes the animal appeared insensible, but in a few moments after was able to walk when placed on his feet; the inspirations were deep, and his posterior extremities trembled. In twenty hours he was insensible, and in twenty-three hours died. On dissection the mucous membrane of the stomach was found red in some places.

I have referred to these cases in order to show the effects of large doses of this solution on the nervous system. The first experiment, you will observe, agrees in its results (that is, in causing tetanic convulsions) with that made by Nysten, of throwing ammoniacal gas into the cavity of the pleura. From the convulsions it may be inferred that in these instances the spinal marrow was specifically affected.

On the human subject we hardly know its action in very large doses; but that the nervous system is affected seems probable from a case mentioned by Plenck, which terminated fatally in four minutes; though the symptoms are not related.

The *modus operandi* of ammonia deserves to be noticed. Does ammonia become absorbed? I am not acquainted with any facts which can enable us to decide this question. Even admitting that absorption does take place, it is more likely that the remote effects are of a sympathetic kind, than the result of the absorption. In calculous complaints ammonia and its subcarbonate may be used with considerable advantage in those cases where potash and soda are serviceable; from which I think it probable that, like the fixed alkalies, ammonia taken into the stomach would be evolved by the kidneys.

Uses.—We resort to a solution of ammonia, properly diluted, on a variety of occasions, some only of which can be here noticed.

1. *To neutralize acid.*—In dyspeptic complaints, accompanied with preternatural acidity of stomach, and flatulence, but without inflammation, a properly diluted solution of ammonia may be employed with a two-fold object—that of neutralizing the free acid, and of stimulating the stomach. And here I would remind you, that the healthy secretions of the stomach are of an acid nature, and doubtless, therefore, this condition has its proper uses; so that the continued use of ammonia, or any other alkali, must ultimately be attended with injurious results, more especially to the digestive functions. While, therefore, the occasional employment of alkalies may be serviceable, their constant or long continued use must ultimately be deleterious. But I shall more particularly examine this point, when speaking of potash and soda.

Ammonia may, under some circumstances, be employed to neutralize acids introduced into the stomach from without, as in poisoning by the mineral acids, though chalk and magnesia would be more appropriate, being less irritant. I mentioned in my last lecture that ammonia was a valuable antidote in poisoning by hydrocyanic acid. This was at one time referred to its uniting with the acid, thereby forming the hydrocyanate of ammonia; but since it has been found that this salt is highly poisonous, it is evident that this explanation is not satisfactory. Some have ascribed the activity of the hydrocyanate to its decomposition by the free acids of the stomach, and the consequent evolution of free hydrocyanic acid, but to me the explanation is unsatisfactory. I therefore believe the efficiency of ammonia as an antidote to poisoning by hydrocyanic acid, arises from its exerting an influence of an opposite nature to that of the poison. I need hardly say that in poisoning by the oil of bitter almonds, or other agents supposed to contain this acid, ammonia is equally serviceable. Let the remedy be given by the stomach, if the patient can swallow, and also let the vapour be inhaled, constantly keeping in mind the caution already given.

2. *To produce local irritation, rubefaction, vesication, or destruction of the part.*—As a *local agent*, ammonia has been employed in a variety of diseases, sometimes as a rubefacient or irritant, sometimes as a vesicant, and occasionally as a caustic. Thus it is employed as a rubefacient in

rheumatic and neuralgic pains, and as a counter-irritant to relieve internal inflammations. As a local irritant, a weak solution has been injected into the vagina and uterus, to excite the catamenial discharge; but there are some objections to its use. Thus, it is a most unpleasant kind of remedy, especially to young females; moreover, the stoppage of this discharge is in many cases dependent on constitutional or remote causes, and, therefore, it is unlikely a topical remedy can be beneficial. However, some may feel inclined to try it, and on that account I may state that Lavagna employed 10 or 15 drops of the solution diluted with milk. The following is Nisato's formula:—

℞ Ammon. liquid. gtt. xl.; Decoct. Hordei, unc. viii.; Mucilag. arab. unc. dimid. Misce, et fiant quatuor intra diem injectiones.

Sometimes ammonia has been employed as a vesticatory; and it has two advantages over cantharides—a more speedy operation, and not affecting the urinary organs. I shall presently give you a formula for making a vesicating ointment; or you may apply the solution of ammonia itself. As a caustic, the strong solution of ammonia may be sometimes employed with advantage in the bites of rabid animals.

3. *The vapour of the solution of ammonia may be inhaled* when we wish to make a powerful impression on the nervous system, as in syncope, or to prevent an attack of epilepsy. To guard against, or relieve fainting, ammoniacal inhalations are very powerful and useful; their instantaneous operation is frequently astonishing. Pinel says he once saw an attack of epilepsy prevented by this means. The patient (a watch-maker) had intimations of the approaching paroxysm from certain feelings, but he found by inhaling the vapour of ammonia, it was frequently prevented. In the case of a confirmed epilepsy which I was in the habit of watching for some years, I think I have also seen analogous beneficial effects. I speak doubtfully, because it is so difficult to determine, in most cases, the actual approach of the fit.

In asphyxia, ammoniacal inhalations have been strongly recommended by Sage, who says that he produced the apparent death of rabbits by immersion in water, and recovered them subsequently by the use of ammonia. A case is told us of a man who had been submerged in the Seine for 20 minutes, and who, when taken out of the water, appeared lifeless, yet by the use of ammonia recovered; and a M. Routier, a surgeon of Amiens, is said to have restored a patient in the same way. That it may sometimes be of service I can readily believe, but it must be employed with great caution.

4. *Ammonia is given internally as a stimulant and sudorific* in a variety of cases, with manifest advantage. In recapitulating these, I must be very brief: (*a*) in continued fevers which have existed for some time, and where all violent action has subsided, and the brain does not appear much disordered, ammonia is occasionally of great service. You should promote its diaphoretic action by diluents and warm clothing. It has an advantage over opium, that if it do no good, it is less likely to do harm. (*b*) In intermittent fevers, ammonia is sometimes of advantage during the cold stage, to hasten its subsidence. Some persons, however, employ blood-letting in these cases, and I can bear testimony to the practice being unattended with any ill effects, though I have not seen benefit result therefrom. (*c*) In the exanthemata, when the eruption has receded from the skin, and the extremities are cold, a stimulant and diaphoretic, like ammonia, is sometimes of great benefit. But you must not lose sight of the fact, that in many of these cases the recession of the eruption arises from, or is connected with, an inflammatory condition of the mucous membrane, for which the usual treatment is to be adopted. (*d*) In some inflammatory diseases, where the violence of vascular action has been reduced by proper evacuations, and where the habit of the patient is unfavourable to the loss of blood, ammonia has been serviceable, more especially in pneumonia and rheumatism.

5. *In certain affections of the nervous system* ammonia is frequently employed with the greatest benefit. Thus it has been used to relieve the cerebral disorder of intoxication; Chevallier says, benefit is often, but not invariably, obtained from it. In poisoning by those cerebro-spinants commonly denominated *sedatives*, such as foxglove, tobacco, and hydrocyanic acid, ammonia is a most valuable agent. This remedy has been supposed to possess a specific influence in relieving those disorders of the nervous system accompanied with spasmodic or convulsive symptoms; and hence it is classed among the remedies denominated *antispasmodic*: but whenever it gives relief it is by its stimulant operation. Velsen, of Cleves, has used it with advantage in delirium tremens. It was a remedy frequently tried in the malignant or Indian cholera, and occasionally procured relief, but this effect was not sufficiently common to lead practitioners to place much reliance on it.

6. *Against the bites of poisonous animals*, as serpents and insects, ammonia is frequently employed with the best effects,

There does not appear, however, any ground for the assertion of Sage, that it is a specific: in fact, Fontana declares it to be sometimes hurtful in viper bites.

Antidotes.—The dilute acids, as vinegar, lemon or orange juice, &c.

3. *Carbonates of ammonia.*

Ammonia combines with carbonic acid in three different proportions, giving rise to three salts, termed, from their composition, *monocarbonate*, *sesquicarbonate*, and *bicarbonate*; and they are thus constituted:—

Monocarbonate.

1 Carbonic acid	22
1 Ammonia	17
1 Water	9
	48

Sesquicarbonate.

$1\frac{1}{2}$ Carbonic acid	33
1 Ammonia	17
1 Water	9
	59

Bicarbonate.

2 Carbonic acid	44
1 Ammonia	17
2 Water	18
	79

Of these three carbonates, two are contained in the Pharmacopœia—namely, the *monocarbonate*, found in the spiritus ammoniæ, and the spiritus ammoniæ aromaticus; and the *sesquicarbonate*, usually called the subcarbonate. The bicarbonate is not officinal, but by long exposure of the sesquicarbonate to the air, free ammonia is evolved, and the bicarbonate formed. How soon the carbonates of ammonia were known is not easy to say, since chemists were for a long time unacquainted with the difference between caustic and carbonated ammonia. Raymond Lully, in the thirteenth century, had obtained the carbonate from urine, but it is probable that it was known to the Arabian chemists long before.

(*a.*) Spiritus ammoniæ.—This is prepared by distilling proof spirit with muriate of ammonia and the subcarbonate of potash of the Pharmacopœia. By the mutual action of the muriate and the subcarbonate on each other, water, chloride of potassium, and monocarbonate of ammonia, result: the latter distils over with the spirit.

Reagents.	**Results.**
Am + H Chl.	Am + $\ddot{C}$
$\dot{K} + \ddot{C}$	$\dot{H}$
	K Chl.

This solution is more pungent than the solution of the sesquicarbonate which is kept in the shops: its effects and uses are similar.

(*b.*) Spiritus ammoniæ aromaticus.—This is a very analogous preparation to the last, except that the proportions of the ingredients are somewhat different, and, in addition, some aromatics are employed, —namely, cinnamon, cloves, and lemon-peel; so that the resulting spirit is weaker, but more agreeable.

(*c.*) Besides these spirits of ammonia, there are two others contained in the Pharmacopœia: the *spiritus ammoniæ fœtidus* (which I shall notice hereafter, in speaking of asafœtida), and the *spiritus ammoniæ succinatus*, a preparation containing a solution of caustic ammonia, oil of lavender, oil of amber, mastic, and rectified spirit, and which is commonly termed the *eau de luce*. It is a powerful stimulant, and has been employed against the bites of poisonous animals.

(*c.*) Sesquicarbonate of ammonia: *History and synonymes.*—This salt has been long known under a variety of names: in the Pharmacopœia it is denominated *subcarbonas ammoniæ*, probably on account of its alkaline properties; in the shops it is denominated *volatile* or *smelling* salts. It is employed by bakers in the manufacture of some of the finer kinds of bread, cakes, biscuits, &c. in which yeast is not used.

Preparation.—The salt is prepared by subliming muriate or sulphate of ammonia (in the Pharmacopœia the former is directed) with chalk. When we examine the process *theoretically*, we are at first astonished that although the salts employed are neutral, the resulting compound is not so. One atom of muriate of amnia, by theory, ought to yield, with one of chalk, an atom of monocarbonate of ammonia, one of water, and one of chloride of calcium. But the resulting ammoniacal salt is a sesquicarbonate, showing that part of the ammonia must have escaped in the process. The following symbols show the changes which take place:—

Reagents.	**Results.**
3 (Am + H Chl.)	Am + $\dot{H}$ $\triangle$
3 ($\dot{Ca} + \ddot{C}$)	2 ($Am^2 + \ddot{C}^3 + \dot{H}$)
	3 (Ca + Chl.)

When three atoms of muriate of ammonia react on three of carbonate of lime, one of ammonia and one of water escape; and two atoms of the hydrated sesquicarbonate of ammonia and three atoms of chloride of calcium are formed.

Properties.—It is met with in cakes,

which are fibrous, white, translucent, and about two inches thick. When exposed to the air, it evolves ammonia, and is converted into the bicarbonate; so that its vapour has a pungent odour, and strongly reddens turmeric paper. Its composition has already been stated. It is met with in the shops in various degrees of purity; the more impure kinds containing some empyreumatic oil.

The *liquor ammoniæ subcarbonatis* of the Pharmacopœia is really a solution of this sesqui-salt, in the proportion of four ounces of the salt to a pint of distilled water. It has the ammoniacal odour, and gives the results already mentioned as indicative of its containing ammonia. By lime-water we obtain a white precipitate of the carbonate of lime, soluble in acetic acid. The solution effervesces with any of the strong acids.

Physiological effects.—The *local* action is that of an irritant and corrosive, but it is much less powerful than the liquid caustic ammonia, in consequence of the presence of the carbonic acid, which in some degree neutralizes the alkaline properties of the ammonia present. Swallowed in large doses it excites pain, inflammation, and all the consequences of a violent irritant poison; while the immoderate inhalation of its vapour is capable of giving rise to violent bronchial inflammation.

The *remote* action of this salt is similar to that of caustic ammonia. Thus, in small doses, it has the same exciting action on the heart, brain, &c. and the same diaphoretic effect. The principal experimenters with this salt are Seybert, Orfila, and Gaspard, on dogs, and Wibmer on man. Seybert injected in one experiment fifteen grains, in a second twenty-five grains, and in a third experiment forty-five grains of this salt, dissolved in a little water, into the crural vein of a dog: the animal appeared to suffer great pain; the frequency of the heart's action was increased, the respiration became difficult, and violent convulsions came on; but in all these cases perfect recovery took place. The blood drawn after the injection had the natural colour, odour, and consistence. Orfila found that two drachms and a half of the salt, given to a dog, caused gastric inflammation, with tetanic convulsions; the body ultimately becoming curved, with the head forcibly bent backwards.

Wibmer's experiments were made on himself. A grain and a half of this salt produced no remarkable effect; three grains increased the frequency of the pulse from 68 to 72 beats per minute, with throbbing headache. In other experiments, in which he took from six to twelve grains (in some repeating the dose at short intervals), the effects were usually, but not constantly, increased frequency of pulse, with disorder of brain, manifested by the pain, heaviness, throbbing, &c. In one case, he says disposition to cough, and increased secretion of bronchial mucus, were remarkable.

Uses.—This salt is used in the same cases, and under the same circumstances, as the liquor ammoniæ; it would, therefore, be both tedious and useless to recapitulate these.

In some cases we employ this salt in the production of effervescing draughts. The following are the relative proportions of acid and base to be employed:—

20 grains of subcarbonate of ammonia (Ph. L.) will saturate about	6 fluid drachms of lemon juice. 26 grains of crystallized citric acid. 26 grains of crystallized tartaric acid.

The citrate and tartrate of ammonia, obtained as above, are useful in allaying nausea and vomiting; they are also feebly diaphoretic, and in some cases diuretic.

4. *Solution of Acetate of Ammonia.*

History and Synonymes. — This solution appears to have been first described by Boerhaave, in 1732, and was introduced by him among the articles of the Materia Medica. Subsequently it was employed by Mindererus; and hence it obtained one of its names, *spiritus seu liquor Mindereri.*

Preparation.—The *liquor ammoniæ acetatis* of the Pharmacopœia is prepared by dissolving the so called subcarbonate in acetic acid; by which the carbonic acid is expelled, and an acetate of ammonia formed in solution. Of course the quantity of ammonia dissolved depends on the strength of the acetic acid. If the latter be prepared according to the directions in the Pharmacopœia, its sp. gr. is about 1·009, and a pint of it will require very nearly seven drachms of the subcarbonate.

Properties. — When the acetic acid and the ammoniacal salt are both pure, the solution should be perfectly colourless. Any tint, therefore, which the solution of the shops may have, is referrible to impurities in the substances employed. I would remark, it is always better to employ a little excess of acetic acid; for if the ammonia preponderate, the solution is highly irritant, and if used as a collyrium, might produce inconvenient results. In regard to *incompatibles*, I need hardly tell you that acids, alkalies, and most metallic salts, produce chemical changes when mixed with it. It is a very

common practice to combine sulphate of magnesia with the acetate of ammonia; and although double decomposition takes place, no precipitate is produced. Pure acetate of ammonia gives no precipitate with a solution of acetate of lead, but owing to the presence of a little carbonic acid, the liquor ammoniæ acetatis of the shops usually does. You ought, therefore, not to combine them, more especially as Dr. A. T. Thomson says the carbonate of lead which is formed is much more likely to occasion colic than the acetate of lead.

Physiological effects.—Its action is exceedingly slight. "I have known," says Dr. Cullen, "four ounces of it taken at once, and soon after four ounces more, without any sensible effect." It is reputed, however, to be diaphoretic, and sometimes diuretic; but to produce these results, it must be given in very much larger doses than is usually done. Wibmer tried on himself the effects of repeated doses of it, but did not observe any diaphoretic, diuretic, or purgative operation. It is a slight local irritant.

Uses.—It is employed both externally and internally. In cold lotions it is frequently used as a refrigerant; and in febrile affections it is one of the most common constituents of the saline draught, generally combined with a solution of tartar emetic, and sometimes with the spiritus ætheris nitrosi.

5. *Muriate of Ammonia.*

History.—I have already mentioned that the date of the discovery of this salt is not well ascertained. It is commonly known by the name of *sal ammoniac;* Berzelius terms it *chloride of ammonium.* In Thomson's History of Chemistry, as also in Parr's Medical Dictionary, will be found many other synonymes.

Preparation.—The methods now adopted for procuring this substance for commercial purposes, I have already mentioned. Some years since, a M. Leblanc, chemical manufacturer at St. Denis, near Paris, procured it by synthesis — that is, by bringing ammoniacal and muriatic acid gases together in a leaden chamber. The ammonia was generated by burning various animal matters in iron cylinders; the muriatic acid was obtained by the action of sulphuric acid on common salt. In Parkes's *Chemical Essays,* you will find an account of the process, with a ground-plan of the laboratory. The natives of Egypt did (and perhaps now do) obtain it by sublining the soot produced by the combustion of the dung of camels, oxen, and other animals. By reference to Parkes's work, just alluded to, you will find an account of other processes.

Properties.—In commerce it is usually met with in large cakes; they are translucent, and become slightly moist in the air. It may be obtained in regular octahedrons; and occasionally these are met with in the subliming pots. It produces cold in the act of dissolving in water.

Composition and nature.—There are two modes of viewing the composition of this salt—either as consisting of muriatic acid and ammonia, or as being a chloride of ammonium. Ammonium, according to the views of Berzelius, is a suppositious metal, composed of 4 proportions or atoms of hydrogen, and 1 of nitrogen. Here are the symbols for both views.

$$H^3 N + H\ Chl. \qquad H^4 N + Chl.$$

For the advantages and inconveniences attending Berzelius's theory, I must refer you to the fifth volume of Dumas' valuable *Traité de Chimie;* pharmacological lectures being, I conceive, unsuited to these details.

Characteristic tests.—It is a white volatile solid. By placing a small portion of it on the point of a knife, and introducing it into the flame of a candle, we may readily demonstrate its volatility. If a little be rubbed with caustic potash or quick lime, ammoniacal gas is evolved, which may be recognized by its odour and other properties, already pointed out. That it is a muriatic salt, is shown by the action of the nitrate of silver causing a white precipitate of the chloride of silver, soluble in ammonia, and insoluble in nitric acid. If a solution of the muriate be added to a solution of the chloride of platinum, a yellow precipitate takes place, which is the *chloro-platinate of the muriate of ammonia:* a similar precipitate is produced by chloride of platinum with the salts of potash, but not with those of soda.

Physiological effects.—The *local* action of muriate of ammonia is that of an irritant, and, therefore, when swallowed in large quantities, nausea, vomiting, purging, with inflammation of the alimentary canal, may be expected. I am not aware, however, of any cases of this kind having occurred in the human subject; but the experiments of Orfila and Arnold have shown these to be the effects on other animals. As to the *remote* action of this substance on man, Wibmer is the only one—as far, at least, as I know—who has experimented with it. He tried it on himself in doses of from 10 to 20 grains, repeating the quantity at the end of an hour: the effects were—sensation of warmth in the stomach, pulse slightly increased, head oppressed, and increased desire to pass the water. Courten, Sprögel, Viborg, and Gaspard, have tried the effects of injecting

solutions of it into the veins of animals (dogs and horses): convulsions were generally observed. According to the experiments of Orfila, Smith, and Arnold, it would appear, that to whatever part of the body this salt be applied, inflammation of the stomach is produced.

Uses.—Muriate of ammonia is principally employed, dissolved in water, as an irritant application to the skin. I have seen it serviceable in chronic affections of the joints. It has also been used for a variety of other local purposes, namely, as a constituent of a dentrifice, as an application to ulcers, skin diseases, &c.; as an ingredient in stimulating gargles and collyria, &c.

Internally, it has been recommended in gout and rheumatism, and in catarrhal complaints.

6. *Ammoniacal Liniments.*

I have already, on more than one occasion, alluded to the employment of ammonia, when combined with fatty bodies.

(*a*) The *linimentum ammoniæ fortius* of the Pharmacopœia is prepared by mixing one part of the liquor ammoniæ with two of olive oil.

(*b*) The *linimentum ammoniæ subcarbonatis* is made with a solution of subcarbonate of ammoniæ, and three parts of olive oil.

Both these liniments may be employed with advantage to relieve rheumatic and neuralgic pains, sore throat, and various other complaints where stimulants to the skin are likely to be useful.

7. *Ammoniacal Ointment.*

In France an ointment is employed called *liparolé d'ammoniaque*, or *pommade ammoniacale de Gondret*, composed of 8 parts of solution of ammonia, mixed with 8 parts of fatty matter (7 lard and 1 mutton suet). The mixture thus obtained, if rubbed on the skin, and covered by a compress, produces very speedily vesication; but rubbed on the skin without the compress, it occasions only rubefaction, owing to the escape of the ammonia. This preparation might, I think, be usefully employed where counter-irritants or vesicants are required.

OF THE EMPYREUMATIC OIL OBTAINED BY THE DISTILLATION OF ANIMAL MATTERS.

General remarks.—I have previously alluded to *empyreumatic oil* as a product of the decomposition of animal substances by heat. Thus, in the distillation of bones, an oil is obtained by the decomposition of the gelatine, which has been termed *Dippel's* oil, being, in fact, the same as the *oleum cornu cervi*, or *oil of hartshorn*, and which, until very lately, was an officinal article.

Effects.—It is a very powerful agent, and in large doses has produced death. Its local action is that of an irritant; while its remote effects are principally observed in the nervous and vascular system, on which it acts as a stimulant.

Uses.— It has been employed as a local remedy in bruises, gangrene, porrigo, &c. Internally, it has been used to prevent an attack of epilepsy or ague, as a stimulant in low fevers, and those cerebro-spinal disorders accompanied with convulsive movements, as hysteria; lastly, it has been given as an anthelmintic. The favourite remedy of Bremser against tape-worm, is *Chabert's oil*, which is prepared by mixing three parts oil of turpentine with one part Dippel's oil, and distilling three parts.

Dose.—A few drops, cautiously increased.

LETTER ON THE BLIND*.

By Monsieur A. Rodenbach,

Member of the Chamber of Deputies, Brussels.

[Concluded from p. 731.]

Miss Wilson, the daughter of the celebrated General Sir Robert Wilson, lost her eyesight in her infancy in a most distressing manner. During the campaign in Egypt, General Wilson was attacked by that dreadful ophthalmia which has been fatal to so many thousand Europeans. His wife, the tenderest of wives and mothers, devoted herself to the care of her husband, and she soon felt the effects of this pestilence. Their little child, this darling girl, never quitted them a moment, and consoled them by her caresses; but this terrible affliction extended to her also; and when her parents recovered their sight, they had to weep over the object of their tenderest affections consigned to everlasting darkness. When peace was concluded, and the Bourbons restored to France, General Wilson came to Paris; and it was in that capital that I obtained some information about his daughter, whose fate interested every one who saw her. They all praised her

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* In the former part of the letter, *Rodenbeck* was substituted for *Rodenbach*. Also at page 731, *for* "sew a little circumference on the silk of the different countries," *read* "sew a little silk on the circumference," &c. It may be proper farther to add, that we have slightly condensed the letter, particularly by the omission of some verses.—Ed. Gaz.

mildness, her sweet manners, and that modesty which is the most precious dowry of her sex. She added to these qualities a superior mind, much information, and a most extraordinary memory. She was well acquainted with English and French literature, and could recite the most beautiful passages in both languages from memory. Early accustomed to the highest class of society, it is not surprising that she was distinguished by the excellent tone of her manners; but she would have been remarkable in every class of society for her love to her parents, her tender feeling for all unfortunate persons, and her generosity to the poor, which caused her to be revered by all around her, and afforded a continual source of consolation in her own affliction.

Miss Sophia Osmont, a blind young lady of 18, is the daughter of Madame Mirette, one of the most talented actresses in France, and the best comic performer of the Vaudeville. This lady is constantly employed in the education of her daughter, who may be compared to Miss Wilson in information and elegance of manner. This young lady plays admirably on the piano, and on the harp. She executes the finest passages of music with a precision and steadiness which are most rare. She is very partial to foreign literature. The best French and Italian authors are familiar to her. She understands geography, and writes and casts accounts remarkably well. But the most singular and surprising of all her talents is, that she teaches her mother all her parts : this requires explanation. The mother reads her part aloud ; her daughter, gifted with a prodigious memory, knows it directly by heart, and then she in her turn repeats continually all the phrases of the part, to alleviate this painful task to her mother. I have been present at this exercise, and I own it interested me greatly. Sophia is gifted with the rarest qualities ; and this is still more remarkable, as she may be said to have been blind from her birth, having been seized almost at the first moment of her existence with convulsions, which deprived her of sight. The inimitable Mars gave her many proofs of interest and friendship, and went often to see her. Thus when you saw the lovely Mars play "Valeria," it was Sophia that she copied, and all her peculiar blind manners ; for this celebrated actress always studied nature in order to represent it on the theatre with that truth which was her peculiar attribute. As a proof of her attachment to Sophia Osmont, she made her a present of a bracelet of great value, and of the most exquisite work, upon which I read in characters in relievo, "Valeria to Sophia." Madame Minette collects a charming society around her.

May I be permitted to consecrate some lines to my remembrances, and to deplore the death of a female, the honour of her sex, who has been lately torn from her friends. Mad. Defosseux-Virnot, of Lille, lost her sight in early youth, in consequence of a paralysis of the optic nerve. Far from being depressed by so great a misfortune, she occupied her mind in the acquisition of knowledge, and turned every thought to the study of philosophy and literature. She became one of the most enlightened and best-informed women who have ever ornamented society. Good, mild, always amiable, she lived adored by her family, and by all who surrounded her. Severe suffering had not altered her angelic temper, and she possessed in a most eminent degree that evenness of temper which, according to a great man, constitutes true education, passionately devoted to the study of philosophy, but of that philosophy which sprung from a strong and liberal mind, superior to prejudice. From the natural strength of her mind, and her extreme good sense, this lady, so justly regretted, was formed for society, but she quitted it without regret, for study, solitude, and friendship. She repeated often to the two female friends who were constantly with her this proverb :—"friends ought to agree to die the same day." She died in their arms, preserving the perfect use of her faculties to the last moment.

M. Heilmann is a scholar of Pfeffel, and of Valentine Hauy ; his memory is prodigious ; he possesses the most extensive knowledge of history ; not a single detail escapes him ; he has employed himself with philosophy, has studied all the German authors, and is profoundly conversant with their literature : he has written a most excellent work on education, which has gained him the friendship of M. Maron, the head of the

protestant church in France. Under the empire, M. Heilmann was at the head of a very considerable cotton manufactory. Many invalid blind men wove calicoes in his manufactory, and some disabled soldiers with wooden legs mended the threads and overlooked the frames at which the blind men worked.

You will, perhaps, have thought it strange that the blind should marry their fellow-sufferers. There are many examples of such marriages, and M. Hauy encouraged them very much; his experience led him to discern that such weddings made happy families. Children from these unions generally see well. This is not always the case when the deaf and dumb marry their companions in affliction: it has been remarked that their descendants generally inherit their misfortune, whilst since the reign of St. Louis, history mentions only one family in which blindness was hereditary, and then the children lost their sight gradually. At 20 they saw well enough to walk by themselves, but at 40 were totally blind.

M. Alexander Fournier lost his sight in his infancy from the small-pox. His father, who was a merchant at Paris, put him under the care of M. Hauy, and he was one of his best scholars; he became in a few years one of the most intelligent of the blind. Few amongst them have excelled so much in the beauty of their hand-writing; he is a good geographer; and when he places his fingers on a map in relievo, he distinguishes the capital cities, rivers, &c. more quickly than a person whose sight is perfect: he is equally gifted in arithmetic, and by the help of his plank and leaden cyphers, he performs with the utmost celerity the most difficult sums and calculations; he reads very well in relievo, and decyphers music quickly in the same manner. I remember once, that at the musée of the institution for the blind, at Paris, M. Fournier and myself contrived to read with our toes the leaden characters. Another time, to perfect the sense of touch, we procured some pumice-stone, by the help of which we took off the epidermis of the fore-finger; when this operation was finished, we took great care to wear a leather finger-stool. M. Fournier has taken lessons from M. Paingeon, the most astonishing blind mathematician of his age. When M. Hauy was summoned to Russia by his Imperial Majesty to found an establishment for the blind, M. Fournier accompanied this celebrated professor to St. Petersburgh, and in this city he married a very amiable woman, whom he had the misfortune of losing some years ago. The only consolation he finds in this irreparable loss is, the attention he pays the education of his children, which he directs himself.

Lesueur was born at Lyons, and came to Paris: he was begging at the door of a church when M. Hauy saw him there, took pity on him, carried him home, and completed his education. In three years, or rather less, he taught him to read, write, and cast accounts; he understood geography and music; he was also successively a private tutor to his companions, and the head of the printing office. His spirit of order in business and accounts caused him to be employed as steward of the institution. He died some years ago at Paris. His wife was a blind woman. Avarice rendered this beggar ungrateful to his benefactor. M. Hauy said to me, whilst speaking of Lesueur, "Ingratitude is the daughter of selfishness, and the vice of little minds; we will not regret him,—I have had the pleasure of doing good." But this reproach addressed to Lesueur, proves nothing against the blind; for the numerous scholars of M. Hauy always cherish his memory, and look upon him as a father. This excellent man died some years ago, at his brother's, M. L'Abbé Hauy, professor of mineralogy in the Jardin des Plantes at Paris.

Madlle. Petronille Moens was born in Holland, in 1765, at Cubart, in the province of Friezeland. She lost her sight at three years old. Her father, a Protestant minister at Aardenbourg, discovering that she had a great taste for poetry and elegant literature, encouraged, by every means in his power, her taste for poetry. We have by her "Esther;" "Hugo Grotius;" "Jean d'Oldenbarneveld;" and "Les Frères de Witt." Her most celebrated works are, "The Spring," a poem, in three cantos, published in 1788; and afterwards, the "History of Humanity," and "Reflections on the Eighteenth Century." We discover in her writings some bold and brilliant sallies, which characterize the great poetical genius

of the author. On many different occasions she has carried off the academic prizes. She obtained the first prize at the Hague, for her poem entitled "Le Vrai Chrétien." She was also crowned at Ghent, for a poem on the Battle of Waterloo. Madlle. Moens has also published "Le Patriote Victorieux," and many other little works on the affairs of the day, now forgotten; but our Sappho of the Netherlands has completely succeeded in a romance, in prose, called "Caroline d'Eldemberg, ou la Fidélité conjugale éprouvée." This work appeared in 1819. Notwithstanding her advanced age, this lady does not suffer her lyre to repose, and her melodious strains are still heard. She published lately, at Amsterdam, her "Bouquet pour la Jeunesse." This kind of writing does her the highest honour. This Nestor of blind poets is universally esteemed for her virtues and her talents. She now resides at Utrecht.

M. Rocques, a poet, born at Montauban, is the more remarkable amongst the blind, because he has educated himself. Before coming to the Musée of the Blind at Paris, he had invented letters in relievo, in order to learn to read. He employed the same method for music. This literary man has translated, in verse, with much taste, the odes of Metastasio. He has often inserted in the journals some articles on literature, which are expressed with all the warmth of a southern imagination. He displays infinite sagacity in discovering the defects and the characters of persons with whom he is acquainted. When he is at Paris, and wants a servant as a reader, he advertises for one in Les Petites Affiches, which brings to his house a crowd of knavish valets; he asks for their certificates of character: but he knows how easily masters give a character for honesty to worthless servants, and whilst he pretends to examine their papers, the lively Gascon questions them with as much skill and sharpness as could be practised by an old justice of peace. He is rarely deceived in the choice he makes, selecting them entirely from their answers.

I need not eulogize M. Paingeon; it is sufficient to say that this extraordinary blind man is gifted with a spirit of order peculiar to himself. He possesses transcendant knowledge in mathematics, and may be justly called the Saunderson of his age. After having gained every prize in 1806, in the general assembly of the four Lyceums of Paris, he was named by the Grand Master of the University professor of mathematics to the Lyceum of Angers, where he teaches with the greatest success. As a reward for his talents, he has been recently created Chevalier of the Legion of Honour.

M. Delille, a blind man, belonging to the Quinze-Vingts, has carried to a high degree the metaphysics of the French language: he has the most admirable precision in his definitions. I do not hesitate in saying that he is one of the first grammarians in France. This name recals to my memory a still more celebrated Delille. You anticipate, without doubt, that I mean the French Virgil. Like Milton, whom he has translated so well, he lost his sight at an advanced age. During his emigration in Switzerland, he married Mdlle. Vandechamps, whom he often called his Antigone. The gossiping chronicles of Paris raised some doubts of the mildness and complaisance of this new Antigone. I give an anecdote on this subject, which, however, I will not vouch for:—Mad. Delille, unmindful of the softness of her sex, forgot herself so much as to throw an enormous folio volume at her husband's head. The tender poet answered, with the greatest calmness, "Madam, you really ought to bestow your caresses on a smaller scale." You see by this that the tender Antigone had no pity for the author of so many fine poems; but it appears to be ordained that great men are to endure feminine tempests occasionally: Socrates himself could not conquer Xantippe.

Happening to be some time ago at Mont-Morency, I wished to visit a literary blind man, the nephew of my countryman Gretry. I went to the hermitage of J. J. Rousseau. The Belgian composer, of glorious memory, had been the proprietor, but at his death his nephews became the possessors. M. F. Gretry gave me a most obliging reception, talked to me of Rousseau, and hastened to make me touch the table on which he wrote his immortal Heloise. I touched also the weeping willows planted by this most eloquent of writers. Almost opposite

are placed the heart and the bust of the memorable Gretry, with this simple and expressive inscription: — "His genius is every where, but his heart is only here." I displayed my astonishment that these precious remains were not at Liege, the country of this great composer; particularly as, in his will, he expresses his desire for it to take place. M. Flemand Gretry then told me that at the death of his uncle he had written to the authorities of Liege, to agree with them in what manner to send the precious remains; "but (added he) you will hardly believe that these gentlemen desired me to send the heart of my uncle by the Diligence, and to excuse its public reception. I was indignant, and still more angry, because at that time the Cossacks were pillaging France, and traversing the country in every direction."

I have spoken, in the beginning of this letter, of the lively disposition which is commonly possessed by the blind. I knew one at Paris, who, in the whole course of his life, had never felt sorrow: it was impossible to be more happy than he appeared. He was constantly jovial, making always puns and playing on words. His name is Barbier; he possesses an independent fortune; he goes every night to two or three theatres to amuse himself, and at the same time to learn by heart, without trouble, the new airs and couplets. This blind man is a musician, and his memory is so strong as to enable him to sing several pieces when he returns from the first representation of an opera. His motto is, "Liberty and independence."

I shall pass over in silence a crowd of blind persons who are well known in the arts and sciences; such as Aboulola, Aufidius, Cneus, Dydyme of Alexandria, Malaval, Buret, Lineman, Caralhi, Anastassi, Chauvet, Chatelain, Martin, Gambasias of Volterre, de Ries, Sir Henry Mayer, &c. as I believe I ought only to speak of those who particularly merited your attention.

As I thus occupy myself with the blind, I beg to submit a few questions to physiologists. Why are blind and deformed persons so anxious to marry? Why are there so many instances of longevity amongst the blind, whilst it seldom occurs amongst the deaf and dumb? Why are there hardly any cases of mental alienation amongst the blind? and why, in the Canton de Vaud, in Switzerland, are there 150 deaf and dumb in a population of 150,000 souls? This strange statistical anomaly appears also in the Canton of Berne.

I hope that you will forgive a blind man for his presumption in attempting to enlighten a person gifted with sight. On any other subject I should have begged instruction from you, but as learning removes all darkness, I have endeavoured to explain to you how in this respect even the blind may see.

I am, with profound respect,

Your obedient servant,

A. Rodenbach,

Member of the Chamber of Deputies, Brussels.

January 1836.

GERMAN MEDICAL INSTITUTIONS AND PRACTICE.

HEIDELBERG—AUGSBURG.

To the Editor of the Medical Gazette.

Sir,

In a recent number of the Gazette, where my work on Continental Medical Institutions is noticed, a wish is expressed for more information respecting the German schools and hospitals. I have, therefore, pleasure in forwarding an account of some of those which I had not previously visited.

The University of Heidelberg is one of the most ancient, having been founded in 1386; and although it suffered much during the wars of the seventeenth century, it subsequently recovered itself, and now ranks high among the German universities, especially as a school of medicine, to which the zeal and ability of its professors mainly contribute. Among these I may specially mention Tiedemann, professor of anatomy and physiology; Chelius, professor of surgery; and Nægele, professor of obstetricity. The students have the reputation of being attentive and zealous in the pursuit of knowledge; their number amounts to about six hundred, of which more than one-third attends the medical classes. The shortness of my visit, which occurred at vacation time,

prevented my obtaining information respecting the plan of study, the examinations, and the facility with which degrees are granted.

The building is a plain quadrangular structure, in the centre of the town: its interior is disposed in lecture-rooms and a hall for examinations. The library is in an adjoining building, and the museums of zoology, mineralogy, &c., are in the houses of the respective professors. The anatomical theatre and dissecting-rooms form part of an edifice, formerly an extensive convent. What was the chapel, is now the *locale* of Tiedemann's anatomical and pathological museum, which is well arranged, and contains several valuable preparations, especially those of the sympathetic, par vagum, and cervical nerves.

Augsburg has a general and a military hospital; the former, a large building, used some years ago as a work-house and house of correction, is situated in an open and salubrious part of the town, and is supported by funds subscribed at the time of its foundation, by contributions from the magistracy, and by a trifling quarterly tax on domestics and others of the inferior classes, who, when ill, thus acquire a right to admission. The wards are not large, but are well ventilated and clean, and contain about 130 beds, each of which is furnished with an enormous eider-down covering, which doubtless obviates, in many cases, the necessity for the exhibition of diaphoretics, and with several pillows, by which the patients are maintained in a half sitting posture. Individuals with acute, chronic, syphilitic diseases, and psora, are received; as also those labouring under mental aberration. There is likewise an obstetric ward. The professional duties are performed by a physician, a surgeon, an assistant, and a house-physician. The diseases most prevalent at Augsburg are bronchial, rheumatic, gastric, and hepatic affections; chlorosis, bronchocele, and nervous disorders. Typhoid and intermittent fevers are rare.

Rheumatism is extremely prevalent throughout Germany, to which circumstance, I have little doubt, the universal use of eider-down bed coverings contributes. The acute form is here treated by general and local bleeding and purgatives; diaphoretics and colchicum are only employed in the more chronic forms, the affected parts being enveloped in tow. Baths are not much used, and the locale is not in good order. In pulmonic inflammation, bleeding and refrigerant beverages are chiefly trusted to; blisters are used when the urgent symptoms are relieved. In hepatic and gastric derangement, the taraxacum is much used, in combination with small doses of calomel. Active purgation is had recourse to in many instances. Among the patients were — a case of epilepsy, of many years' standing, which had been materially benefited by the exhibition of indigo; a case of hemiplegia, in which great advantage had been derived from the rhus toxicodendron; another of paralysis of the hands and feet, without apparent cerebral affection, in which recovery was taking place from the use of strychnine. In the syphilitic wards, mercury appears to be the remedy almost exclusively employed; frictions are preferred to its exhibition by the mouth.

The department for the insane consists of nineteen cells, warmed by stoves, the windows being secured by iron bars and wire; each cell has an occupant, and contains no other furniture than a bed and a stool. Tranquil patients are allowed to walk in the exercise-ground, but the more intractable ones are confined to their cells; those who are furious are fixed to their beds by strong leather straps. Abstinence, the confining chair, and occasionally chains, are the other means of restraint employed. One of the most frequent causes of insanity in this part is religious enthusiasm: there appears to be neither moral nor medical treatment; in fact, these cells must be considered merely as places of detention. There is no good institution for the insane throughout the Bavarian territories.

On a future occasion, I will transmit some account of the medical institutions of Munich.—I am, sir,

Your obedient servant,

Edwin Lee.

Munich, Jan. 17, 1836.

Medicine is addition and subtraction: subtraction of what is in excess, and addition of what is deficient, in the system.—Hippocrates.

USE OF VERATRINE

IN THE

TREATMENT OF CHRONIC GLANDULAR SWELLINGS.

To the Editor of the Medical Gazette.

SIR,

SHOULD you deem the following short communication worthy of insertion in your pages, it may perhaps be the means of calling the attention of the profession to a few facts in regard to the use of veratrine which appear to me worthy of notice.

Whilst employing it in the form of friction in cases of chronic rheumatism attended by considerable swelling of the affected joints, I observed that the removal of the pain was followed by a marked diminution in the size of the articulation, and that in some instances, by steady perseverance in the treatment, the swelling disappeared; and from this circumstance I was led to try its effects in chronic glandular enlargements, and in these I have found it of much service, even in cases where iodine had ceased to be of any use. This last, though a valuable medicine, is found, like all others, to lose its efficacy when employed for a length of time, and it therefore becomes an object with the practitioner to be possessed of other remedies, to which he can have recourse if necessary, and which he may make use of alternately with it.

Veratrine is one of these; and after considerable experience of its effects in bronchocele, indolent tumors of the mammæ, bubo, and scrofulous tumors in different situations, I can recommend it as a useful external application. It has, besides, a few minor advantages: it may be rubbed over a painful tumor, and generally with the effect of giving immediate relief; it has no tendency to irritate the skin, which preparations of iodine often have, and the superfluous quantity of ointment may be removed with a little *warm* water and soap immediately after the friction is finished; and on these accounts it is preferable in tumors about the neck, as the part may be exposed immediately, if necessary.

The following is the formula I have generally employed:—

℞ Veratrinæ, gr. x.; Axung. ℥ss. M. ut fiat unguent.

A piece of this ointment, the size of a nut, should be rubbed for ten minutes twice a-day over the seat of the tumor; and at the end of every week an additional quantity of veratrine, in the proportion of five grains to the ounce, should be added to the prescription, so as to keep up the effect.

The other remedies which belong to the same class as veratria, namely, delphine and aconitine, have nearly the same effects; and it will often be found advantageous to substitute one of these for the veratrine, when its influence has become less by repeated application.

I am, sir,
Your obedient servant,
A. TURNBULL.

Russell-square,
Feb. 4, 1836.

TWO CASES

IN WHICH

THE TRICHINA SPIRALIS

WAS LATELY FOUND.

To the Editor of the Medical Gazette.

SIR,

As every circumstance connected with the recent discovery of myriads of parasites pervading human muscles cannot be otherwise than interesting to the profession, I beg to send you a notice of two cases in which they have been lately observed at the London Hospital. One is the case of a man, apparently in good health, who was brought into the hospital in a state of insensibility, with a fracture of the skull, and died two days afterwards.

On examination, the thoracic and abdominal viscera were found healthy; and the larynx having been taken away for the purpose of dissection, the sterno-thyroideus and other muscles connected with the larynx were subsequently observed to be thickly studded with minute white specks, which, upon microscopical examination, proved to be cysts containing the trichina spiralis.

The other is the case of a man who had been many months in the hospital with a large aneurism at the commencement of the descending aorta, which had made its way through the anterior parietes of the chest, the patient having died exhausted.

The parts having been removed for the sake of preservation, numerous cysts were found in the various muscles of the neck, and in the diaphragm; but on minute inspection none could be detected in the heart or axillary nerves. In both cases the insects were examined by Mr. Owen, and found to correspond with those already described.

It is worthy of remark, that in the first case the presence of these insects appears to have been unconnected with disease elsewhere; nor could I learn that the man had experienced any uneasiness, or loss of muscular vigour.

I am, sir,
Your obedient servant,
THOMAS BLIZARD CURLING.

1, St. Helen's Place,
Feb. 6, 1836.

MEDICAL GAZETTE.

Saturday, February 13, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

PUBLIC EXAMINATIONS.

THAT respectable document—the Petition—which is to receive the signature of Chairman Meade, *on the part of the medical students of the metropolis*, and to be presented to parliament by the *honourable* member for Finsbury, has not yet made its appearance; or, at least, it has been studiously kept out of our way. We are particularly anxious to see it, in order to ascertain one point —viz. what the students are made to say in it in respect to *public examinations*. We are curious upon the point, because, whatever the Meade-and-Wakley document may say to the contrary, we have reason to know that public examinations are *not* an object of desire with the great body of the students.

In several of the metropolitan schools, we believe, there may be found some few noisy and disorderly pupils—the idlest of their class, and of course the worst prepared for any examination—who join in the cry for a public trial, with the secret hope, no doubt, that it will afford them the best chance of passing muster; their nerve and impudence, they trust, will bear them safely through the ordeal. We could almost wish there *were* a public examination instituted for the benefit of these worthies, for we know well what would be the result—most rare and deserved *plucking* — disgrace indelibly branded on them, not to be obliterated for their lives' length, and serving as an excellent example *pour encourager les autres*.

But if we understand aright the genius and character of the great mass of well-disposed students who frequent the schools, there is nothing of which we are more certain, than that a public display—an exhibition before a gaping crowd of grinders and groundlings—when passing an examination for their diploma, would be wholly abhorrent from them. The large majority of medical students of the metropolis is far from being represented by the riotous few who figure occasionally before the police magistrates, obtain for themselves a scandalous notoriety in the newspapers, and unfortunately bring a certain portion of ill-repute on the general body of their fellows—nay, on the profession itself. These are the "choice spirits" with whose aid certain wily adventurers raise perpetual disturbances, and promise to establish a new order of things. But, we repeat, of such are *not* composed the ingenuous youth who fill the benches, and attend regularly to the business of the schools. We use no exaggeration when we say that it is our firm conviction that there are no students in any other part of Europe better disposed, generally speaking,

than those of London: they are gentlemanly, orderly, and unpresuming, entertaining a proper respect for their teachers, and not easily led away by the ill example of mischievous agitators. Their chief desire is to pass into the order of practitioners by a fair and well-conducted examination: they want no theatrical formality: they are satisfied to abide by the award of a tribunal from which they only demand justice, tempered of course with a proper degree of courtesy.

It is quite another thing where this fair-play and this courtesy are denied; where the student may have reason to complain that justice has not been duly administered to him. Here the scene changes. A public examination is desired—is demanded; but only as a *dernier ressort*—on an appeal. As matters stand at present in the constitution of the Society of Apothecaries, it is known that there can be no alternative of this sort; and therefore the call for it can only be accounted a bravado: but we confess ourselves rather disposed to think that such an alteration in the laws of the Society as would sanction a public examination, under the circumstances just alluded to, would be both politic and wholesome.

The possession of that presence of mind that intrepidity, necessary for a public trial, is a talent not very commonly met with among English students, nor is its absence to be imputed to any one as a crime or a fault. The love of display is not congenial to well-bred English gentlemen;—they shrink from the public stare whenever they can avoid it, though they can face it bravely when enjoined to do so by their duty to themselves.

It is, besides, very much a question with us, how far the preparation for, and the submitting to, such an ordeal, can be serviceable in the way of discipline for entering the profession. Our idea is, that such a course would materially tend to alter the character of young medical men in this country: it is to be feared we should have, instead of the steady respectable aspirant to practice, a race of young practitioners full of self-sufficiency, pertness, and conceit. We should at least have two distinct orders of junior men: those who, with a plentiful stock of assurance — seldom the concomitant of intelligence and ability—have passed their public ordeal with success, and are constituted thereby a very important set of personages, high in their own estimation, and no doubt making a favourable impression on the undiscriminating, undiscerning portion of the public; and secondly, those, forming by far the larger class, who, with modest merit alone for their portion, pass respectably, perhaps, their public trials, yet would enter on their professional duties with a feeling of self-abasement (owing to the embarrassment experienced from the public gaze), which probably would not wear off for many a year. Thus, by such an arrangement, a stumbling-block would be placed on the threshold of the profession, which the rattle-brained, probably, would be very well able to surmount, or at least lose nothing in attempting it; while their more sober and more competent companions would be much incommoded, or perhaps deterred altogether from further proceeding.

But this sort of public examination for diplomas is not only uncongenial with the habits of the English student, but not practised in any locality in this or any other country that we are acquainted with. The wild proposers of the scheme will probably point to their favourite *concours* (the excellent working of which we have ever and anon to bring to the no-

tice of our readers); but it is the height of ignorance and absurdity to adduce such a precedent. In France, it is true that certain students do sometimes compete by *concours*—but not for diplomas: for public situations, such as *internats* in the hospitals, and the like, public examination by *concours* must be submitted to. This, however, is wholly an optional thing; nobody is obliged to undergo such an ordeal, save those who are willing and have prepared themselves for it. The case is very different for the great mass of students, who are unambitious of public appointments, and seek only to be recognized as legitimate practitioners. They are examined as they are here, with due formality, but without the interference of a gaping and disorderly crowd.

Be it not supposed that we by any means object to a body of examiners, however large, such as may be deemed essential or requisite for the proper management of a well-conducted examination. Every aspirant about to enter into the profession of medicine ought at least to possess nerve enough to show an undaunted front before his legitimate examiners: he is a candidate for a privilege, the due exercise of which will often call upon him for the fullest display of all his energy and moral courage; and it were ill-becoming in him who seeks to be placed in a capacity which, he knows not how soon, may be attended with circumstances of a trying or dangerous nature, to shrink from the scrutiny of a board of seniors regularly appointed to test his merits. We would even have the observances and ceremonials of such a trial, in every case, complied with to the letter. But all this is very different from countenancing an idle display, at once embarrassing both to judges and candidates: the admission of the public, we are persuaded, on such occasions, would be attended with no good, whatever mischief might be traceable to it. Nine-tenths of the curious auditory would, no doubt, consist of not the most industrious students, but perhaps of the idlest, and those most distinguished as scandal-mongers; and no small portion of the space allotted to the public (as if the *public*, properly so called, were at all concerned in the matter) would be occupied by those grinders and their protegés, who would have so special an interest in the proceedings.

To the latter parties, we mean more particularly the grinders, these "public examinations" would doubtless afford a rich treat — indeed, a valuable source of emolument. Every question would be stored up for future use, and every answer given by a rejected candidate—especially if he had been previously *crammed*—would be defended like an article of faith on which salvation itself depended. Woe to the examiners who should venture to exercise a discretion in plucking the pupil of a grinder! The bullying, the worrying, to which the ill-fated functionaries of such an "open" tribunal would be exposed, may readily be conceived; the game would clearly be in the hands of the greatest disturbers.

But it is the condition to which the pupil would be reduced by this newfangled system, that is most worthy of note. The reign of the grinders would be firmly established; and no pupil could venture to shew himself at a public ordeal unless he had the passport of those potent authorities. Let who would be his examiners, the grinders would be sure virtually to preside; and who would condemn more loudly those not of their flock, or defend with greater bluster those they could call their own? We happen to know one instance of this sort of abuse, practised even where the

public, or more open, examination is granted only in case of appeal. In one of the Colleges of the United Kingdom, it is the rule to grant an *appeal* trial to the candidate who demands it after his first rejection. This appeal trial is the harvest of the grinders, and they reap it too with most diligent sickles.

To the students of the metropolis, whose character is acknowledged to be not a little compromised by the doings of certain parties at the late meeting, we earnestly recommend the consideration of these things; and we trust, that should such a petition as that fabricated by the managers of the Meade-and-Wakley meeting be presented to parliament, they will protest against it in an efficient manner. The strong resolution entered into by the students of Guy's (see p. 784), together with the decided and manly part already taken by other schools, shows that there is no disposition quietly to submit to the interference of selfish and turbulent agitators. We rejoice that it is so; and we sincerely congratulate our medical brethren that there are of those who aspire to come amongst them no inconsiderable number who are willing to enrol themselves as the friends of discipline and order, and whose conduct bids fair to add both grace and dignity to the profession.

MEDICAL STUDENTS' ASSOCIATION.

The "Crown and Anchor" meeting having failed to answer the purposes for which he intended it, Mr. Wakley is now straining every nerve to get up a kind of political union among the medical students, for the ostensible purpose of watching over *their* interests, but with the real design of promoting *his own*. We earnestly advise all young men who would avoid having to regret, when too late, time misspent and opportunities neglected, firmly to resist all solicitations to join this same "Association." No legitimate object can possibly be gained by it, while there are various ways in which it may bring them into trouble, both immediately and prospectively. Indeed, it is our firm opinion that the effect of such a combination would be to lead to the introduction into the new regulations which it is expected will soon be made regarding medical education, a much more rigid system of professional discipline than has hitherto existed.

Neither let the students be so simple as to imagine that Mr. Wakley can really do anything for them; he is too well known to have the slightest influence. He is very grandiloquent, it is true, on the hustings, or when addressing the pawnbrokers and old clothesmen of Finsbury at a tavern dinner, but in the House of Commons he is a cypher—a mere nonentity. Observe: he always votes with the Ministry, yet they consult him in nothing—trust him in nothing—countenance him in nothing—no, not even by appointing him on any parliamentary committee of the slightest importance. No reference is ever made to him even regarding the affairs of the medical profession; yet he endeavours to cajole the students by pretending that he can do great things for them. Again, they are aware that there is a plan in contemplation for establishing a University in London, and that this will probably lead to great changes in the existing regulations. Well, the Chancellor of the Exchequer, who is preparing the charter, has been most assiduously courted by Mr. Wakley, who has been flattering and fawning, and kissing the dust beneath his feet, to get a sight of the intended charter, *but in vain*. This we gather from

himself; for having been thrown off his guard by vexation, he says, in his last number, "We insisted on the production of a draft of the intended instrument," but "there is no person beyond the precincts of the Privy Council Chamber who can obtain a glimpse of such a document." This speaks volumes, and shows, that whoever is admitted to the "Council Chamber," he, at least, is excluded. Let not the students, then, be deceived. Whatever is done in regard to our profession will be done by Mr. Warburton or by the Ministers, *and Mr. Wakley is not in the councils of either*. He may get up a "lark" for the students; he may swagger at the "Crown and Anchor," and emulate ancient Pistol in his Lancet; but no one, short of a regular *Johnny Newcome*, is so simple as to imagine that by such means he can carry any measure for them in Parliament.

In conclusion, we repeat our caution to young gentlemen; and we earnestly advise the parents of those whose homes are distant to lose no time in interposing and warning their sons against that distraction from their studies, and from every purpose for which they were sent to London, which cannot but result from attending Wakley's tavern exhibitions, or joining his "Medical Students' Association."

A WORD TO THE LANCET.

For some time back the Editor of the Lancet has been occupied in the most rancorous abuse of all those whom he suspects of having been, in the remotest degree, connected with the Medical Gazette; and, to give him his due, he has contrived to exhibit such lowness of cunning, such daring in falsehood, and such ruffian-like ferocity of language, as we sincerely believe to be without parallel even among the most violent conversial writers of this country. Our readers are aware, however, that we have very rarely entered on the defence of individuals attacked by the Lancet, because it appears to us that those who think its calumnies worth noticing, are themselves the proper parties to answer them. But as one expedient recently adopted has been that of attacking the *principles of this journal*, we shall so far depart from our general custom as to take a brief notice of it. We are charged, then, with maintaining that "a knowledge of the science of medicine and surgery is confined to 'hospital' physicians and 'hospital' surgeons;" and also with "*perseveringly and systematically calumniating the general practitioners of medicine in this empire*."

To this charge we answer, that it is false—grossly, malignantly, designedly false; that it is a dishonest trick practised by a cozening knave to hoodwink his readers, and to sell his pamphlet. We have stated the accusation made against us, and to our readers we say—look to the pages of our journal, and judge for yourselves whether it be true. To the author of the accusation we say—*prove* your charge, and we shall acknowledge that for once you have been just. Lay aside for one short week the habit of blinding your readers with frothy declamation, and in its stead let us have a few facts. You say we systematically calumniate general practitioners; substantiate this—show when, and where, and how we have done so. But in this, do, pri'thee, if thou canst, be honest—we ask it but for this occasion. Do not deal in insinuations and generalities, but give us special instances; and pray, when you make quotations, set down the words as you find them in the Gazette; above all, do not forge passages as heretofore, nor even alter them in your own knavish way, to suit your purpose. You see we give you good advice, and treat you very civilly; much more so, you must be conscious,

than you deserve. We offer you a fair challenge, and if you decline it, or fail in making good your charges, depend upon it, people will be very much disposed to think, as we do,—that you are no better than a trading slanderer, and that your editorial character is a blot upon the medical literature of the country.

HOW TO HAVE THE "EXAMINATION" PUBLISHED.

We have ascertained that if the gentleman recently rejected at Apothecaries' Hall, really desires to appeal against the Examiners, the straightforward course is to address a complaint to the "Court of Assistants," at whose desire alone it is consistent with the laws of the Society to disclose the grounds of refusing a license to the complainant. We have reason to believe that the Court of Examiners are most anxious that this should be done; and, if the rejected candidate be equally sincere, there can be no difficulty in ascertaining where the justice of the case lies. At the same time we think it right to hint to the gentleman above alluded to, that after the public and prominent part he has taken in this affair, if he should neglect so simple a method of proving the truth of his allegations the inference will be, that all his seeming boldness is but blustering and bravado.

We may also take the opportunity of remarking, that when we wrote the observations contained in our leader of last week we were not aware that it had been publicly stated by whom the party in question was examined. This circumstance is of importance, because it gives an application to our strictures which we did not intend. When we said that an Examiner might be "rude, or petulant, or overbearing," we did not allude to the gentleman by whom we now learn that the examination which has excited so much discussion was conducted.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, Feb. 9, 1836.

Henry Earle, Esq., F.R.S., President, in the Chair.

The first paper read this evening was, *An Account of a Case of Fracture of the Atlas*,—by B. Phillips, Esq., F.R.S.

A man, aged 32, fell from a hay-rick to the ground head foremost, the occiput coming in contact with the soil. Although stunned by the fall he was in a few minutes enabled to walk a distance of half a mile to the parish surgeon, by whom he was bled and purged. In two days he had so far shaken off the effects of the accident as to be able to follow his usual avocations. The only lesion of function which remained was an inability to rotate the head: flexion and extension of the organ were performed up to the time of his death, though the extent of these motions was, perhaps, more limited than in their natural state. Being desirous to recover the power of rotating the head, Mr. Phillips was applied to on the subject. Upon examination a small tumor was evident at the back of the neck, over the second cervical vertebra. It was apprehended that the injury might have excited inflammation in the first or second vertebra, or possibly in both, and that a modification of the articulating surface between them was in progress, which might terminate in ankylosis. He was placed on a mattress, and a dozen leeches were applied to the tumefied region every third day for above three weeks. The power of rotation was not restored, and an issue was established at the points. This was in action for six weeks without any improvement in the function: it was healed up, and a second formed. In the period whilst the second was in action he became the subject of an attack of pleurisy. This was energetically treated and cured; but in a few weeks afterwards anasarca was manifested, during the existence of which, in the twelfth month from the occurrence of the accident, he died from hydrothorax. The only other symptoms which became apparent in the progress of the case were a thickness of the voice, much like that produced by enlarged tonsils, and a slight difficulty in swallowing, which were produced mechanically, in a way which will be immediately evident.

After death it was found that the violence of the fall had produced fracture of the atlas, immediately behind its occipito-atloideal articulation. The anterior portion was carried downwards and forwards

between the pharynx and the axis, until it arrived upon the same plane with the latter organs, to the body and transverse processes of which it became attached by true bony union.

In consequence of this forcible displacement one of two things must occur; either the odontoid process, or the transverse ligaments by which it is attached to the atlas, must give way. In this case the odontoid process was fractured, and to this the patient most probably owed his life.

Several questions were put to the author by Mr. Partridge, Mr. R. Quain, and other members, respecting the precise condition of the patient in performing the functions of rotating and supporting the head, &c., which Mr. Phillips answered to the extent of his personal observation. The patient appeared to have sufficient muscular power, at least in the early part of his disorder, to support his head without the assistance of his hands.

The President mentioned some interesting cases of injuries occurring to the cervical vertebræ of children, in some of which he had reason to believe that the odontoid process was fractured, or broken off. The little patients were generally scrofulous, and met with the accident in play. It was remarkable, in one instance in particular, how tenaciously the child persisted in holding his head fixed between his hands after the occurrence of the injury; nor did he venture to desist from the practice for some days. The cure was attended with complete ankylosis. While on his legs Mr. Earle wished to call the attention of the meeting to a specimen of injured spine which he exhibited. He had bought it among other bones at the late Mr. Heaviside's sale, and it was marked as an example of diseased vertebræ; but he (Mr. Earle) was convinced it was the result of a very complicated fracture. The vertebræ belonged to that part of the spinal column where the dorsal and lumbar portions unite; and the singularity was, that the vertebral canal was in one part reduced to the calibre of a common goose-quill. Here the patient must have lived for some time with his spinal marrow compressed in an extraordinary manner. The state of the callus, and union of the fracture, shewed clearly that a considerable time must have elapsed before the death of the patient.

Mr. R. Quain cited a very interesting case, in which the ring of one of the cervical vertebræ was suddenly broken off from the body of the bone, in an elderly man. The head had to be supported by various mechanical contrivances; but the symptoms were by no means alarming. The patient even took a long journey by coach, and, in short, survived for eleven months. Mr. Quain had had an opportunity of examining him after death, when the exact nature of the injury was ascertained. No doubt but for the age of the patient, and the little precaution taken in keeping the parts together, union might have been effected.

A second paper was read,—*On a Case of Recovery from Poisoning with Opium*,—by Charles Smith, Esq.

The peculiarity in this short history was, that the patient was sinking fast into death, under the influence of the narcotic poison, when artificial respiration was had recourse to. The effect was striking: the heart's action and the energy of the circulation were restored, and the person soon completely recovered. No bellows had been used in the process of inflating the lungs, but a simple tube was introduced into the nostril, keeping the other closed at the same time.

Dr. J. Johnson thought that the case was valuable, inasmuch as it showed how beneficially so very simple a mode of inflating the lungs might be employed.

The President mentioned an instance in which a patient, sinking under the effects of a strong tobacco injection, administered with a view to the reduction of a strangulated hernia, was revived by insufflation, and artificial respiration, managed with a pair of bellows.—Adjourned.

LECTURE ON CORNS AND BUNIONS,

Delivered in the Theatre of St. George's Hospital, January 19, 1836,

By Sir Benjamin C. Brodie, Bart.

It cannot be doubted that the physical condition of man is, on the whole, much improved by civilization; but it is not so in all respects, and the usages of society are productive of some evil, combined with much good. The evil affects the weaker more than it does the stronger sex; and among the former, those who belong to, what are called the higher classes, suffer more than those who belong to the lower. Young ladies, living much in heated rooms, taking little exercise in the fresh air, over-educated as to the acquirement of accomplishments, and using their muscles too little, lose the beautiful figure with which they were endowed by nature, and become afflicted with curvatures of the

spine, and weakness and distortion of the ankles. The same mode of life renders them liable to the innumerable varieties of hysterical disease, which in so many instances destroy the whole comfort, and I may say the dignity, of existence, enervating both the body and the mind, and making their condition altogether much less desirable than that of the poor peasant girl.

There is another order of diseases which we meet with more frequently among females of the higher classes than among other persons—namely, corns and bunions; and it is to this last humble, but not unimportant subject, that I propose to call your attention in the present lecture.

A corn is in the first instance a thickening of the cuticle. Whenever the cutis is habitually subjected to the influence of pressure, it secretes a thick and horny cuticle. We find examples of this in the hands of many mechanics, and in the soles of the feet in those who walk much. But every thickening of the cuticle is not a corn, and this name is applicable only to those cases in which the cuticle is thickened over a projecting portion of bone, on which the pressure is, as it were, concentrated. Corns may occur in any part of the body in which this combination of circumstances exists; but, for obvious reasons, they are met with in the feet much more commonly than any where else.

If shoes were constructed of the shape of the human foot, neither too large nor too small, and making an equal pressure every where, corns and bunions of the feet would never exist. But, unfortunately, shoes are seldom made after this fashion, and in ladies' shoes especially there are generally two signal defects: first, the extremity of the shoe is much too narrow for that part of the foot (namely, the toes) which it is to contain; and, secondly, for the purpose of displaying as much of the foot as possible, the whole of the tarsus and metatarsus is left uncovered, and the pressure of the shoe in front is thrown entirely upon the toes. The toes are thus first squeezed against each other, and then pushed out of their natural position; and all the projecting points, chiefly where the joints are situated, are pinched and tormented either by the neighbouring toes or by the leather of the shoe, and thus it is that corns of the feet are generated.

In order that you should understand the precise situations in which corns are most likely to take place, you must consider more particularly the effects, which the pressure of the shoe produces on the toes. The little toe is pushed from its parallel position, so that it is in fact underneath the fourth or adjoining toe, and corns are generated on its outer surface over the prominences of its joints. A corn is also frequently met with in the angle between the little toe and the next toe, where the first phalanx of the former is pressed against the head of the metatarsal bone supporting the latter. Sometimes the consequence of wearing a very narrow shoe is, that one of the toes (and it is generally the second or fore-toe) is pushed upwards, so that it lies over the two adjoining toes, that is, over the great toe and the third toe, the extremities of which come in contact underneath; then the leather of the shoe is drawn tight over the upper surface of the second or displaced toe, and corns are produced over one or more of its articulations. At other times one of the toes (and in this case also it is generally the second toe) is displaced in another way. The extremity of it is pushed downwards, so that it lies beneath the extremities of the two adjoining toes, which come in contact over it. But this change cannot take place while the three phalanges of the displaced toe remain in a line with each other. The first and second phalanx make an angle, projecting upwards. The second joint of the toe becomes prominent above, and a corn is formed over it. If the shoe, instead of being too narrow, be too short, for the foot which it contains, the last phalanges of all the smaller toes are kept constantly in a half-bent state, and a row of corns is generated, one being situated on the upper part of the last joint of each of these toes. I have endeavoured to enumerate what may be regarded as the most ordinary localities of corns; but of course they may be produced any where else, according to the shape of the shoe, the mode of walking, and other circumstances.

I have said that a corn is, in the first instance, a thickening of the cuticle secreted by the cutis, when it is kept in a state of constant irritation by the operation of external pressure squeezing it against a prominent surface of a bone. But a complete corn is more than this. A bursa, or bag of synovial membrane, similar to those bursæ which are of original formation, but of a very small size, is formed between the thickened cuticle and the cutis; and it is this combination of thickened cuticle, with a subjacent bursa, which constitutes a perfect corn. This is a fact which you may easily verify for yourselves, as the opportunities of dissecting corns are abundant in the dead house of the hospital.

The thickened cuticle of those corns, which are situated externally, becomes dry, and hard, and horny; while that of the corns which are situated between the toes remains soft, and to a certain degree moist; and this gives rise to the distinction between hard and soft corns. I shall speak

to you of hard corns first,—of soft corns afterwards.

A hard corn, when it begins to be formed, is productive of no other inconvenience than of a slight degree of pain and tenderness after much exercise. The pain and tenderness increase, so that the patient in the evening is glad to take off the leathern shoe, and put on a large slipper. Then the whole foot, after exercise, is hot and uneasy. These symptoms subside with rest, and the absence of pressure, during the night, but return with the wearing of the shoe and exercise during the day. By and bye the bursa under the horny cuticle becomes inflamed, and distended with fluid, and the pain is much aggravated. But the sufferings are greatest in those cases in which the bursa suppurates. An abscess forms in parts which are incapable of distention, and you know how much mischief even a very small collection of pus, under such circumstances, may occasion. I was sent for to an old gentleman who was suffering excruciating pain in the whole foot, which was red, and much swollen, the swelling extending up the leg considerably above the ankle. In one toe, and in the neighbouring part of the foot, the tenderness and other marks of inflammation were greatest, and here I discovered an old neglected corn. He could scarcely bear the corn to be touched; however, I carefully removed the hard cuticle with a scalpel, and made an opening into the bursa under it. Not more than a drop of matter escaped, but this was sufficient to give immediate relief. On the following day he was well. I was desired to see another patient, a young lady, under the same circumstances, except that the symptoms were more severe. The inflammation involved nearly the whole leg, and there was a frequent pulse, and much general excitement. I removed the thickened cuticle of a corn on one of the toes, and allowed a very small quantity of pus to escape which was collected beneath it. This gave immediate relief, and on the following day she was all but well. Several similar cases have fallen under my observation.

I have already mentioned that the most common seat of a soft corn is in the angle between the little toe and the fourth toe, over the head of the metatarsal bone which supports the latter. Occasionally, however, a soft corn occurs elsewhere—as, for example, on the inside of the little toe, opposite to the last joint of the fourth toe. Such corns are even more painful than hard cones, except when suppuration takes place in the bursa, and then the suffering is less in proportion, as the thickened cuticle of a soft corn admits of distention more easily than that of a hard corn.

Under ordinary circumstances, it is easy to give temporary relief to a patient who suffers inconvenience from a hard corn. The thickened cuticle should be removed, so as to lessen the pressure on the parts below; and this may be accomplished in various ways. *First:* If the corn be of long standing, and a piece of linen or thin leather, spread with some mild plaster (diachylon for example), be applied, and worn over it, it will sometimes exfoliate or separate without further trouble. *Secondly:* The corn may be rubbed with the nitrate of silver, or (which is indeed preferable) the concentrated nitric acid may be applied by means of a probe armed with lint. The texture of the cuticle being thus destroyed, exfoliation will take place, so that in the course of a few days the corn may be readily peeled off. *Thirdly:* The corn may be reduced in thickness by scraping its surface with a very fine steel or fish-skin rasp. And, *fourthly:* The corn may be removed by means of a fine cutting instrument. This last is the shortest and simplest method; and the patient may keep himself in a state of comfort by procuring the assistance of a dexterous chiropodist at stated periods, who will perform this operation for him better than he can perform it for himself.

With a view to a permanent cure, however, it is necessary to have recourse to other methods of treatment. In some way or other all undue pressure must be removed from the part on which the corn is situated. *First*, the shoe must be made as nearly as possible to the shape of the foot, and it must cover the metatarsus and a portion of the tarsus, so that the whole pressure may not be thrown on the toes; or a boot made to be laced or buttoned may be worn instead of a shoe. In some cases it is advisable that the shoe or boot should be made, not of ordinary leather, but of very soft and flexible buckskin, or of cloth. A material for shoes and boots is sold under the grandiloquent name of *pannus corium*, which answers the purpose intended in these cases very well. It is really a kind of cloth, but it has the appearance of leather, and is very soft and pliable. *Secondly*, if any of the toes are displaced in any of the ways which I have before described, we must endeavour to restore them to their natural position. In young persons this may be generally accomplished. A contrivance made use of by the bandage makers is very useful on these occasions. It consists of a thin plate of metal covered with thin leather, or a piece of strong leather, fitted to the lower surface of the foot,—not to the whole of the surface, but extending from the extremities of the toes nearly to the tarsus. Slits are formed in this plate of metal or leather,

and tapes are passed through these slits, forming loops above, by means of which the toes are bound down and retained in their proper places. In many cases the same object may be attained by simpler means. A stripe of linen, spread with adhesive plaster, about two-thirds of an inch in breadth, may be passed over the toes which are too elevated, and under the others, the extremities of the plaster being made to cross each other over the metatarsus. If this be neatly applied, it will keep the toes parallel to, and on the same level with, each other. Whichever of these methods be employed, it is necessary that it should be persevered in for a considerable time. In older persons, in whom the toes have been long displaced, they have sometimes become so adapted to their unnatural position, that it is almost needless to attempt to alter it. Under such circumstances we are sometimes compelled, in hospital practice, even to amputate one of the toes, in order that the patient may not be disabled from gaining his livelihood; and this may be occasionally necessary even in private practice. A young lady of rank suffered from a displacement and a distortion of the second toe, such as I have already described. The extremity of it lay under the extremities of the two adjoining toes; the second and third phalanges were nearly anchylosed at a right angle to each other, and a corn was formed on the second joint, where it made a considerable projection above. She applied to me to amputate this offending toe. I answered, " that I would do no such thing; that I might do it for a labouring person, but that her case was entirely different, as she had not to earn her livelihood by her bodily labour." She replied, " You seem to treat the matter very lightly, but this toe and corn make my life miserable: I can take no exercise, I am unfitted for society, and I have tried all other methods of relief without success." On inquiry, I was satisfied that she in no degree exaggerated her sufferings, and I therefore complied with her wishes, and amputated the toe at its first joint..

A very simple, but scientific, method of relieving, and indeed of curing, corns is practised by the chiropodists. A piece of buck-skin leather, spread with some adhesive plaster, is applied on the toe on which the corn is situated, there being a hole in the leather corresponding to the corn. Thus the pressure of the shoe is taken off the corn, and thrown on the surrounding parts. If this be kept constantly applied, and proper shoes be worn at the same time, the corn will gradually disappear.

In some cases a hard corn is formed on the lower surface of the foot, over the head of one of the metatarsal bones. A corn in this situation is especially troublesome, rendering the patient absolutely lame; but it may be relieved or cured by the method which I have just explained, only one slight modification of it being required. The hard cuticle being removed, a broad piece of buck-skin leather is to be applied, having a hole in it where the corn is situated. But a thin piece of calico spread with adhesive plaster, and having no hole in it, is to be applied first; that is, between the leather and the foot. Without this last contrivance the flesh of the foot, when the patient walks, bulges or projects into the hole of the leather, so as completely to fill it up, and the patient's condition is rendered rather worse than better. The calico with adhesive plaster prevents this inconvenience, at the same time that it does not prevent the leather answering the intended purpose of taking the pressure off the corn, and throwing it on the surrounding parts. I may observe, by the way, that the same method is applicable to some other affections of the lower surface of the foot, as well as to corns.

When abscess is formed in the bursa under a hard corn, the treatment to be employed is very simple, although the relief it affords is immediate and great. You are to pare off the hard and thick cuticle, and open into the bursa, so as to allow the small quantity of pus which it contains to escape. Thus the corn is effectually destroyed, both the cuticle and the bursa; and it is very easy, by means of the expedients which I have just recommended, to prevent it being regenerated.

The treatment of soft corns is to be conducted on the same principle with that of hard corns; some modification of it only being required, on account of their peculiar texture and situation. The thickened cuticle may be removed by means of the concentrated nitric acid, applied so as to penetrate into its substance, but not to the parts beneath. This destroys its texture, causing it to become dry and shrivelled; and in the course of a few days it begins to exfoliate, and is then readily peeled off. If an abscess forms in the bursa of a soft corn, it should be treated in the same manner as that in the bursa of a hard corn.

In some cases, even though there be no abscess underneath, a soft corn becomes exquisitely sensitive, so that the patient cannot bear it to be even touched; and he is made as lame as if he suffered from the gout or any other painful malady. Such a case fell lately under my observation, which I mention, not because it was peculiar, but because the sufferings of the patient were unusually severe. There was a broad soft corn on the side of one toe,

where it came in contact with the side of the adjoining toe, and not in the angle between them. The patient could scarcely walk, even with a loose slipper, and the corn itself was so exquisitely sensitive, that the slightest touch could not be borne. This state of things had existed for many weeks, the corn itself being of a much earlier date. I applied the strong nitric acid until I had reason to believe that it must have penetrated through the thickened cuticle. An increase of pain followed the application, and continued for some hours. On the following day there was a manifest improvement. I was now enabled, without any difficulty, to remove the corn with a fine scalpel. The recovery of the patient was immediate and complete, so that, having been previously quite lame, he was enabled in less than twenty hours to walk as well as ever.

The first thing to be done for the permanent cure of a soft corn is, that the patient should be provided with a shoe of a proper shape, and that the toes which are in any way displaced should be brought back into their proper position. Now I have already observed that the most common situation of a soft corn is between the fourth toe and the little toe, over the head of the fourth metatarsal bone, and that in this case the little toe, towards its extremity, is always pushed more or less underneath the second phalanx of its neighbour. You will sometimes succeed in bringing the little toe to its proper place by means of a stripe of adhesive plaster, applied round it in the manner of a loop, and then encircling the foot.

In other cases you will find the following method more convenient than that which I have just described:—A piece of *very thick* buck-skin leather, spread with adhesive plaster, is to be applied on the inside of the little toe, so as to occupy the whole of the inner surface, from the apex to the second joint. The leather should be cut so as to be thin at its margin; and it should be sufficiently broad to admit of being doubled over a good part of the upper and under surface of the toe, as well as its extremity. This contrivance will keep the little toe at some distance from the next toe, and prevent it from sliding again under it. If both of these expedients fail, the patient must be content to wear for a time the metallic or leathern plate, with loops of tape for inclosing the toes, which I have already described.

The bunion which is frequently formed on the inside of the ball (as it is called) of the great toe, differs in some respects from the disease of which I have hitherto spoken.

The great toe ought to be in a line with the metatarsal bone, by which it is supported. But a shoe which is too narrow at its extremity, causes it to incline towards the outside, displacing, in a greater or less degree, the toe next to it, as I have explained already. In some cases, the effect of pressure on the great toe is actually to alter the position of the joint between it and the metatarsal bone; a portion of the articulating surface on the extremity of the latter being absorbed, and a new articulating surface being made to supply its place more externally than the old one. The existence of these changes I have ascertained by dissection. Now, the consequence of all this is, that the head of the metatarsal bone makes an unnatural prominence, and is more acted on by the pressure of the shoe than it would be otherwise. The cuticle becomes thickened, not at one particular point, but over a considerable surface, and underneath the skin a large and very distinct bursa is generated between it and the bone. The difference between what I have now described and a common corn, may reasonably be attributed to the large size of the head of the first metatarsal bone, and to the consequent diffusion of the pressure over a broad surface.

When a bunion is once formed, the bursa belonging to it is liable to become inflamed after any unusual degree of exercise, or on it being subjected to the pressure of a more than commonly tight shoe. Serum is then effused into the cavity of the bursa; the swelling is much increased, and it becomes at the same time exquisitely painful and tender. If the patient remains at rest, the inflammation subsides, the serum effused into the bursa becomes absorbed, and the additional swelling disappears without any further ill consequences. If, however, he continues to walk about, wearing at the same time a tight shoe, the inflammation proceeds further; suppuration takes place, and an abscess is formed. Such an abscess is slow in reaching the surface, and the patient generally suffers severely before it bursts externally; and when it has burst, as the synovial membrane of the bursa granulates with difficulty, the healing of the abscess is very tedious, the parts remaining all the time in a very irritable and painful state.

For the relief of this bunion, when it is free from inflammation, or inflamed only in a slight degree, the following plan of treatment should be adopted:—The patient should be provided with a shoe of sufficient dimensions, of a proper shape, and made of cloth or a soft and pliant leather. A piece of thin calico, spread with diachylon plaster, should be applied over the bunion, covering also some of the surrounding

parts; and over this he should wear a piece of thick buckskin leather, spread with adhesive plaster, and having a hole in it, corresponding in size and figure to the bunion from which it is intended to remove the pressure. If the bursa be much inflamed, the patient should be confined to the couch, without a shoe: leeches may be applied in the neighbourhood, and warm fomentations may be employed also. If an abscess be formed, it should be freely opened with a lancet. For some time after the abscess has been opened, no other treatment is required than the application of a poultice, which may be changed afterwards for calamine cerate, or some other simple dressing. Perhaps the abscess may now gradually heal, and no other treatment may be required; otherwise it will be necessary to destroy the inner secreting surface of the bursa by means of some kind of caustic. The concentrated nitric answers this purpose very well. The end of a dressing-probe may be armed with lint, then dipped in the acid, and applied for a few seconds to the internal surface of the bursa. A thin slough will, of course, be formed, on the separation of which it may reasonably be expected that the remains of the bursa will contract and granulate: otherwise the application of the caustic must be repeated.

After what I have already said, it is needless for me to trouble you with any observations as to the means which should be adopted for the purpose of preventing the bunion being regenerated.

A case came lately under my observation, in which what appeared like a bunion on the inside of the ball of the great toe contained an albuminous substance, of the consistence of the vitreous humor of the eye, similar to that which is found in the ganglions, which occur in the neighbourhood of the wrist and in some other situations. Whether this was an ordinary bunion, in which the vessels of the bursa secreted this peculiar substance, or whether it was really a ganglion, I was unable to determine. The treatment which I adopted was that of opening the cyst freely, and applying the concentrated nitric acid to its inner surface. It was necessary to do this with some caution, lest I should injure the joint or bone underneath; and therefore several applications of the acid were required. My object was to destroy the secreting surface, and obtain a granulating surface in its place; and when I last saw the patient, previously to her returning to the country, I had reason to believe that I had succeeded: but I have not heard of her since.

A tumor is occasionally formed on the instep, which, though not exactly a corn, bears a near relation to it. It is met with in young men who wear tight boots, and the usual situation of it is over the articulation, between the internal cuneiform bone and the metatarsal bone of the great toe. The tumor is under the skin, hard and immoveable, so that it seems to a superficial observer to be an enlargement of the bone itself. The skin over it is in a natural state, except in cases of long standing, in which the cuticle becomes somewhat thickened. I have had no opportunity of dissecting the parts affected with this disease, and am uncertain, therefore, whether it be formed in the ligaments of the joint or periosteum, or in the ultimate fibres of the tendon of the tibialis anticus muscle, or in what other texture.

Such a tumor is productive to the patient of as much inconvenience as a corn, and it requires the same kind of treatment. He should, for a time, leave off boots altogether; or if he cannot do this, the boot-maker should be directed to provide a last with a projection in that part of it which corresponds to the situation of the tumor, so that the boot may not exercise any pressure on it. A piece of thick buck-skin leather, with a hole in it to receive the tumor, will also give the patient immediate relief, and ultimately effect a cure: but the cure, of course, will not be permanent, if he continues to wear tight boots afterwards.

I have seen a tumor apparently similar to that I have now described, in school-boys, situated over the head of the tibia, at the insertion of the tendon of the extensor muscles, commonly called the ligament of the patella, and apparently the result of kneeling, or clambering on the knees: and a tumor of the same kind is sometimes met with on the inner condyle of the femur in those who ride much on horse-back. In either case the avoiding pressure is sufficient to relieve the patient of all the inconvenience which the disease produces. I have known cases, however, in which there have been some remains of a tumor over the head of a boy's tibia ever afterwards.

EXTRACTS

FROM

DR. ASHWELL'S REPORTS OF OBSTETRIC CASES.

Amenorrhœa.—There have been seventeen cases of this affection. We might designate eleven of them as simple amenor-

rhœa; two as occurring in highly-vascular plethoric habits; and the remaining four as associated with great constitutional debility. The treatment pursued in the first-mentioned cases was variously modified. In one instance leeches were applied within the vulva, at the time when lumbar pain, with other symptoms of uterine irritation, indicated a natural effort for the establishment of the catamenia. After the second application the secretion appeared. In several cases an injection of the liq. ammoniæ and milk, in the proportions of ♏x. of the former to ℥j. of the latter, was employed. This proved in some instances highly efficacious, while in others it failed. The two plethoric females were bled locally and generally; and this, associated with the full operation of purgative medicines, completed their cure. Tonics and emmenagogues were the remedial agents for those in whom constitutional weakness was predominant. The metallic tonics, such as the iron and zinc, were preferred.

Dysmenorrhœa.—Five of these cases occurred in females of diminished constitutional power; and one was associated with a robust plethoric diathesis. In those cases wherein false membranes were thrown off, and which were attended with a scanty secretion of the catamenia, the pain preceded the discharge. In one instance, while the pain lasted, the discharge was more abundant than natural, and was mixed with coagula, evidencing its menorrhagic character. In one case the pain supervened on the cessation of the catamenial period; while in another, this affection was complicated with epileptic fits. These fits, which came on about ten days after her admission, were preceded by an *aura*, which appeared to arise from the sacrum. A moxa was applied over the spot; the effect of which was an immediate abatement of the fits, and subsequent recovery from them. A proneness to abort characterized another case. The treatment pursued with these patients varied with their several peculiarities. In all, however, two great points were kept in view; viz. 1. To alleviate the pain during the menstrual period; and, 2. To regulate the general health during the intervals. To fulfil the first indication, opium, combined with camphor, hyoscyamus, ipecacuanha, and conium, were ordered: while the second was answered by the use of the metallic tonics, especially iron and zinc.

Chlorosis.—Examples of simple, confirmed, and complicated chlorosis were met with in these cases. Purgatives, tonics, and emmenagogues, were severally employed; proportioning their amount, character, combination, and time of employment, to the peculiar features of the case. The iodide of iron, as a remedial agent in this affection, deserves especial mention; its exhibition has been attended with the most marked success, particularly when glandular enlargements, and other indications of a strumous habit, have been associated with this disturbance of the health. It should at first be given twice a day, in two grain doses, suspended in any vehicle which does not possess tannin or other astringent matter. The iodide of iron may, in some constitutions, occasion headache, vertigo, nausea, heat, and a sensation of weight in the hypogastrium; which symptoms may be removed by taking the carbonate of magnesia at night, by the suspension of the medicine, or even by diminishing the dose.

Irritable uterus.—Pain in the lower abdomen, shooting into the loins, and coursing the brim of the pelvis, aggravated by the erect, and mitigated by the sedentary or recumbent position, attended these cases. In some instances the pain was increased before, in others after, menstruation. An internal examination detected the exquisite sensibility of the uterus. The first indication in the treatment was, to relieve the pain; the second, to improve the constitutional power. The recumbent position rigidly enforced—the local abstraction of blood from the loins or vulva—camphor, hyoscyamus, and poppy, in combination—the injections of poppy—fomentation, with the frequent use of the hip-bath, answered the first indication. In one case permanent relief was obtained by the application of a moxa over the sacral region. The constitutional treatment comprised the exhibition of tonics, bitters, &c. The iodide of iron was thought the most useful. The bowels were kept regular by a mixture of salts and magnesia.

Leucorrhœa.—These cases were confined to women of feeble constitutional power, and occurred mostly in young unmarried females. They were carefully distinguished from cases of inflammation of the os and cervix, by testing the discharge, which invariably resisted admixture with water. Neither was the leucorrhœa the effect of structural lesion, nor a symptom of displacement of the uterus; but the result of constitutional debility, not unfrequently associated with catamenial derangement. The symptoms present were, pain in the back, pale surface, a tendency to œdema, impaired appetite, susceptibility of fatigue, breathlessness, with more or less hysteria, &c. The remedial plan consisted in the administration of tonics internally, with local astringent injections. The quinine, cinchona, gentian,

calumba, were the principal vegetable, the zinc and iron the principal mineral tonics. The efficacy, however, of the secale cornutum, in these cases, demands especial notice. An injection composed of a strong decoction of the secale, with the addition of some nitrate of silver, proved very serviceable; while its internal administration, in five-grain doses, made into a pill with confect. rosæ, was attended with marked success.

Menorrhagia.—Under this term are comprised only those ca es in which the menstrual secretion is mixed with a discharge of pure blood. The number of cases afforded examples both of the active and passive menorrhagia: the former were recognised by symptoms of plethora, such as a sense of fulness and throbbing, with a feeling of heat and weight about the pelvis, and a pungent sensation about the pudenda; the mammæ became tumid, painful, and hot, and the pulse accelerated; the discharge, too, varied in its mode of escape; sometimes it flowed abundantly, causing faintishness, and even syncope; at others it proceeded more slowly, forming coagula within the vagina: these acted as a barrier, preventing the further flow, and were eventually expelled by voluntary efforts. Bleeding, both local and general; refrigerants, such as nitre, with digitalis, and other remedies calculated to obviate excessive vascular action, were employed in these cases. The instances of passive menorrhagia were by far the most frequent, occurring in both married and single women. The secale cornutum proved very valuable in arresting the flow, by its specific action on the uterus. The sulphate of zinc was exhibited internally, as a tonic, in the absence of the catamenia.—*Guy's Hospital Reports.*

NEW ELECTRO-CHEMICAL APPARATUS.

M. Becquerel, of Paris, has introduced to the notice of the scientific world, a modification of the Electro-chemical Battery, which reduces it to a striking simplicity. And yet he finds that all bodies exposed to it are either decomposed or attacked, exactly as if they were submitted to the voltaic pile; that its power of action continues uninterruptedly; and that the intensity of its current is not affected, in any appreciable quantity, by the causes which tend to weaken the electro-chemical effects of the pile.

Simple as M. Becquerel's apparatus is, its arrangement has already been improved by M. Aimé, who describes the newer instrument as follows:—A U-formed tube, pierced with a small hole at its lowest part, is to have its legs half filled with very fine sand. One of the legs is then to be filled up with diluted sulphuric acid, and the other with a concentrated solution of sea-salt. The two fluids will descend through the sand, and combine at the lower part of the tube. As soon as this combination is effected, the fluid, resulting from the combination, escapes immediately through the hole, which is loosely plugged with a bit of asbestos to prevent the escape of the sand. A slip of platina is dipped into each leg, and wires from these slips connect them with a galvanometer. On making contact, the needle immediately indicates, by the change in its direction, the formation of the current produced by the action of the two fluids on each other. The intensity of this current varies with the degree of concentration of the solutions, and with the rate at which the combination is made: to facilitate the latter, the size of the hole may be increased.

When a constant and regulated supply of the solutions, and a proportionate discharge of them, after they are combined, are obtained, the process can be carried on for any desired period without intermission. There is, however, one precaution necessary,—the solutions must be so prepared and proportioned, that they shall not, on combining, form a salt and obstruct the discharge. From the experiments of M. Becquerel and M. Aimé, there seems already ground for believing, that electro-chemical currents of determined intensity may be produced and maintained during whatever time may be necessary to effect chemical decomposition, by this powerful agent.—*Magazine of Popular Science*, No. 1.

SAFETY IN THE MINES.

The high character bestowed on Upton and Roberts's lamp by the Parliamentary Committee last session, has induced most of the mine-owners throughout the kingdom to give the instrument a trial; and nothing can be stronger than the certificates by which they attest its complete efficacy. From a letter, dated Cophall Colliery, 10th December, 1835, the following passage may be quoted:—"A double-gauze Davy lamp and Upton and Roberts's new safety-lamp, were both taken in our presence to a blower of gas, the aperture $2\frac{1}{2}$ inches in diameter, and placed a short distance from it. The Davy instantly filled with flame, the action of which appeared so violent, that it was considered necessary to withdraw it as speedily as possible, to

prevent explosion; while the new lamp was of itself extinguished. On relighting the latter, and placing it farther from the mouth of the blower, it was found to burn, but show the presence of the inflammable gas by the light of the flame, which was about the thickness of the little finger, and smoky at top; at the same time the Davy-lamp, which was held in the headway, quite out of the stream of the blower, filled with flame, so that it again appeared necessary to remove it from its situation." Another letter to the same effect, dated from Dudley, is signed by about fifty proprietors, who had witnessed the experiments, and were anxious to record the satisfaction they experienced at the results.

SIXTH EDITION OF A DENOUNCED NOTORIOUS MEDICAL WORK.

To the Editor of the Medical Gazette.

SIR,

ON the advertising sheet of a new and respectable journal, is the announcement of "Mr. Lawrence's Lectures on Physiology, Zoology, and the Natural History of Man, reprinted *verbatim* from the original edition, suppressed by the Author;" to which advertisement is annexed a paragraph, sneering at the clergy, whose "hatred he encountered;" at the Governors of Bethlem Hospital, whose "resentment" he incurred; and at the decision of Lord Eldon, "who refused him a right of property in them:" in consequence of which these Lectures have become "public property." "They have already reached a sixth edition!" It furthermore states that "Mr. Lawrence was a materialist when these lectures were delivered!"

With the honour of the publisher who can seize on property so confiscated, or with the moral honesty of the man who can plunder his fallen brother and rob him of the only gains of their mutual unhallowed craft, I have nothing to do. May those stings of conscience find him out *in time*, which can paint to his reckless imagination, in its true colour, the *cruelty* he has inflicted on the youth to whom he has sold the whole of his six editions.

But, sir, with Mr. Lawrence, as a public teacher, I have thus to do: *I* ought to *know*—and all who, like myself, send yearly young men to London, are bound to ascertain, into whose hands they are to commit them—whether by the open avowal of atheism, or by the more specious garb of materialism, or of scepticism, we are to find those principles of religion in which our pupils or our children have been instructed, are to be destroyed or disturbed.

May I venture to hope, through *your* agency (believing you to be a watchful guardian of our professional character, and, I trust, not less so of those Christian and moral principles which alone give lustre to our profession), that your power will be exerted to repress the sale of this publication; and may I venture to hope that Mr. Lawrence will himself stand forth and deny that this work is edited under his sanction; that he will denounce those writings as the early phantoms of his ardent and highly-gifted mind, which sober reflection and calm inquiry has induced him to recal, and show that he has suppressed them at the bidding of his own conviction, rather than the fear of public censure, and thereby the loss of public favour and support.—I am, sir,

Your obedient servant,

W. J. WICKHAM.

Winchester, Feb. 2, 1836.

FEELING IN RESPECT TO PUBLIC EXAMINATIONS.

To the Editor of the Medical Gazette.

SIR,

FROM an extensive intercourse with pupils belonging to various medical schools in London, I have had frequent opportunities of learning the sentiments of many regarding the plan of a "public examination," so much agitated of late by your notoriety-seeking contemporary, and I assure you that I have found a great majority of young men decidedly hostile to it: they have generally expressed it as their opinion that "rejections" would be much more frequent than they are at present, if it were adopted. It is devoutly to be hoped, sir, that you, who have always shown yourself the true and disinterested friend of the student, will not abate your powerful advocacy of their cause and true interests in the present instance, when it will be so valuable to them; and I have no doubt that by the instrumentality of your able and respectable journal,—which has always proved itself an antidote to the pernicious influence of the Lancet,—the minds of the few young men who have been led into error by that publication, will be disabused. A public examination, as a *dernier ressort* in case of alleged grievance, would by the medical students generally be received favourably.—I remain, sir,

Yours respectfully,

A BOROUGH STUDENT.

February 5, 1836.

PROTEST OF THE STUDENTS OF GUY'S HOSPITAL,

AGAINST THE LATE PROCEEDINGS AT THE CROWN AND ANCHOR.

To the Editor of the Medical Gazette.

SIR,

We have great pleasure in transmitting to you a list of those gentlemen, constituting a large majority of the students of Guy's Hospital, who have entered their protest against the *ill-judged* and *violent proceedings* of the meeting at the Crown and Anchor Tavern, held January 18th, 1836; in which certain resolutions were carried and agreed to, as if emanating from the *general body* of the *medical students in London*, which were in their nature *ill advised*, and in the manner of the whole proceeding characterized throughout by *party violence*.

We are, sir,

Your very obedient servants,

H. C. MUGG,
J. BENT.

Guy's Hospital, Feb. 9, 1836.

"The undersigned have perceived, with much surprise and regret, that at a meeting, which was held at the Crown and Anchor Tavern, on the 18th of January, 1836, certain resolutions were proposed and carried, as if emanating from the general body of medical students in London, which were in their nature ill-advised, and in the manner of the whole proceeding characterized by party violence. They desire, as students of Guy's, to disclaim all participation in the transaction, and wish it to be distinctly understood that they do not agree in the resolutions above referred to."

Signed by *Ninety-four* students.

[The original document, with the autograph signatures, is before us, but we regret we have not room to give the names at length.—ED. GAZ.]

PROFESSOR GEIGER.

WE learn from the German journals, that Dr. P. L. GEIGER, of Heidelberg, died on the 19th of last month, in the 48th year of his age. His name is well known in connexion with pharmacy: his *Journal* has had an extended celebrity, and his excellent *Handbuch der Pharmacie* has gone through several editions.

NEW MEDICAL WORKS.

New Treatment of Malignant Diseases and Cancer, without Incision. By A. M. B. Riofrey, M.D. 8vo. 3s.

On Dropsies connected with Suppressed Perspiration, &c. By J. Osborne, M.D. Post 8vo., 5s. cloth.

Dr. Marshall Hall's Observations on Blood-letting. 2d Edition, 8vo., 10s. 6d. cloth.

Practical Observations on Homœopathy. By W. Broackes, M.R.C.S. 8vo. 5s. cloth.

Obstetric Tables. By G. Spratt. Part II. 4to. 24s. bds.

Practical Observations on Midwifery. By James Hamilton, M.D., &c. Part I. 8vo. 7s. 6d. bds.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

February 11, 1836.

Clement James Hawkins, Cheltenham.
John Corney Slaytor, Woolpit, Suffolk.
Dennis Adams, Over, Cambridgeshire.
William Gosse, Poole.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Feb.* 9, 1836.

Age and Debility	72	Indigestion	1
Apoplexy	6	Inflammation	33
Asthma	32	Bowels & Stomach	5
Cancer	3	Brain	3
Childbirth	6	Lungs and Pleura	4
Consumption	68	Insanity	6
Constipation of the Bowels	1	Liver, diseased	15
Convulsions	23	Measles	8
Croup	6	Mortification	2
Dentition or Teething	9	Paralysis	7
Diabetes	1	Rheumatism	1
Dropsy	22	Scrofula	1
Dropsy on the Brain	11	Small-pox	13
Erysipelas	2	Spasms	1
Fever	5	Stone & Gravel	3
Fever, Scarlet	1	Thrush	2
Fever, Typhus	1	Unknown Causes	19
Heart, diseased	3	Casualties	9
Hooping Cough	6		

Increase of Burials, as compared with the preceding week . . 115

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Feb. 1836.	THERMOMETER.		BAROMETER.	
Thursday . 4	from 31	to 39	29·65	to 26 01
Friday . . 5	31	39	30 09	30·05
Saturday . 6	28	39	29·91	29 90
Sunday . . 7	33	48	29 73	29·85
Monday . . 8	30	49	29 98	29·83
Tuesday . . 9	41	51	29 84	29 95
Wednesday 10	43	50	29 87	29 72

Prevailing winds, N.E. & S.W.

Except the 6th, generally cloudy, with frequent showers of rain.

Rain fallen, 55 of an inch.

CHARLES HENRY ADAMS.

NOTICE.—"A Guy's Student," who complains of the trickery of a medical bookseller, should authenticate his letter.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 20, 1836.

LECTURES
ON
MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XXI.

INORGANIC MATERIA MEDICA.

IN this lecture, gentlemen, I have to examine those articles of the materia medica which are derived from the inorganized kingdom; which, however, cannot be denominated minerals, since most of them are products of art.

In the first place, let us fix on the most appropriate methods of arranging them. You are aware I purpose adhering, as much as possible, to a natural-historical order; but we have now to notice a class of substances for the most part not natural bodies. Let us inquire, therefore, what methods the mineralogist adopts in the classification of the objects of his study, and thereby determine whether the inorganic materia medica can be arranged on a similar plan.

Karsten, Haüy, Berzelius, Leonhard, Alexander Brongniart, Beudant, and Thomson, have arranged minerals according to their *chemical* properties. Mohs, on the other hand, formed a classification in which all chemical properties are purposely excluded, and *crystallographical* characters principally relied on: this he denominated a natural-historical method. Now all systems founded on principles so exclusive as this, have always appeared to me highly objectionable. Why should a naturalist not employ any properties of a body by which he can characterize it? If the zoologist and botanist have not hitherto resorted to chemical characteristics, it arises in a great measure from chemistry being unable to give much assistance in distinguishing animals and vegetables. In mineralogy, on the other hand, chemical properties are of essential service, and, therefore, I conceive ought to be employed, though not to the exclusion of assistance derivable from physical characters. Werner, and, more recently, Necker, have made use of both classes of properties.

You see that a considerable number of mineralogical writers have employed chemical properties, either partially or wholly, in their arrangements and classifications: you will not, therefore, be surprised at my selecting these for the basis of the order in which I shall discuss the articles of the inorganic materia medica, more particularly as the order followed by Mohs is inapplicable to them. These articles I shall arrange in four classes, thus:—

Class the first will contain an account of the Elementary non-metallic substances.

Class the second, the non-metallic inorganic Acids.

Class the third, Water.

Class the fourth, Metals, and their compounds.

Before, however, I proceed to discuss the individual substances of these classes, I must offer a few remarks respecting crystalline forms, since, in speaking of the salts, I shall frequently have occasion to employ terms used in crystallography.

Fundamental forms of crystals.—A very superficial acquaintance with mineralogy and inorganic chemistry, will convince you, that there is a prodigious variety of shapes presented by crystallized substances. A late writer on mineralogy tells us, that the crystalline forms of one substance (carbonate of lime) exceed 1400! The accumulated labours of late crystallographers have shown, that the thousands of known crystalline forms may be reduced to six, which are called *fundamental forms*, or *types*; and that all the others are *modifications* or *derivations* of these. Hence,

therefore, six *groups* or distinct *systems* are now usually admitted: they are as follows:—

GROUP 1st. This is denominated the REGULAR, TESSULAR, TETRAHEDRIC, or CUBIC system; and includes all those crystals whose fundamental form is the *hexahedron* or *cube*. The *primary* forms belonging to this group are the cube, the tetrahedron, the regular octahedron, and the rhombic dodecahedron. To this system belong *common salt, alum, muriate of ammonia*, and *arsenious acid*.

GROUP 2d is called the SQUARE RIGHT PRISMATIC SYSTEM; the fundamental form being a *right prism, with a square base*. The *primary* forms belonging to this group are the octahedron, with a square base, and the right square prism. To this system belongs *calomel*.

GROUP 3d constitutes the RECTANGULAR OR RHOMBIC RIGHT PRISMATIC SYSTEM; the fundamental form being a *right prism, with a rectangular or rhombic base*. The *primary* forms belonging to this group are the rectangular octahedron, the rhombic octahedron, the right rectangular prism, and the right rhombic prism. To this system a considerable number of pharmacological agents belong; for example, *citric acid; sulphate, nitrate*, and *bitartrate of potash; sulphate of magnesia; tartarized soda: tartarized antimony; nitrate of silver; corrosive sublimate; sulphate of zinc*; and *sulphur* (both the native and that crystallized from its solution in bisulphuret of carbon).

GROUP 4th. This is termed the RECTANGULAR OR RHOMBIC OBLIQUE PRISMATIC SYSTEM; the fundamental form being *an oblique prism, with a rectangular* or *rhombic base*. The *primary* forms belonging to this group are the rectangular oblique prism, and the oblique rhombic prism. It contains *tartaric acid; phosphate, sulphate, and subcarbonate of soda; sulphate of iron; sulphur* (when crystallized by slow cooling); and *chlorate of potash*.

GROUP 5th is denominated the DOUBLY OBLIQUE PRISMATIC SYSTEM; the fundamental form being *an oblique prism, with a rhomboidal base*. In this group we find *sulphate of copper*.

GROUP 6th. This is called the RHOMBOHEDRIC SYSTEM; the fundamental form being a *rhombohedron*. The *primary* forms belonging to this group are the rhombohedron, the six sided or hexagonal prism, and the double six sided pyramid (sometimes called the bipyramidal dodecahedron). To this system belongs *carbonate of lime*.

Having premised these remarks on the pharmaceutical crystals, I proceed now to examine the individual substances of the inorganic materia medica.

Class I. UNDECOMPOSED NON-METALLIC PHARMACOLOGICAL AGENTS. To this class belong *oxygen, chlorine, iodine, carbon, sulphur*, and *phosphorus*.

I. OXYGEN GAS.

History, synonymes, and etymology.—This gas was discovered on the 1st of August, 1774, by Dr. Priestley, who denominated it *dephlogisticated air*. In the following year, Scheele also discovered it, without knowing what Priestley had done, and he called it *empyreal air*. Condorcet termed it *vital air*, and Lavoisier gave it the name it now usually bears (oxygen), deriving the word from ὀξυς, *acid*, and γενναω, *to produce*.

Native state.—It makes up $\frac{3}{4}$ of the known portion of the terraqueous globe. Thus it constitutes $\frac{8}{9}$ of water. Now three-fourths of the surface of the earth are covered by water. I think we may safely say $\frac{1}{3}$ of the known solid crust of our globe is oxygen. You will also recollect that it constitutes a considerable proportion of the atmosphere—namely, $\frac{1}{5}$ of the air properly so called, $\frac{8}{9}$ of the aqueous vapour, and $\frac{16}{22}$ of the carbonic acid. It is an essential constituent of the organic kingdom.

Production.—The methods of procuring oxygen are numerous. I shall only notice three.

(*a*) *By heating chlorate of potash in a green glass retort.*—By this process we obtain pure oxygen gas. Every atom of chlorate of potash yields six atoms of oxygen and one atom of chloride potassium.

Reagent.	Result.
$\dot{K}\ \overset{.....}{Chl}.$	6 O
	K Chl.

(*b*) *By heating peroxide of manganese in an iron bottle.*—This is by far the cheapest method, and, for pharmacological purposes, the product is sufficiently pure. If any carbonic acid be suspected, we may purify the gas by passing it through lime-water. Two atoms of the peroxide are resolved, by heat, into one atom of deutoxide and one atom of oxygen.

Reagent.	Result.
2 $\ddot{M}ang.$	1 O
	$\underline{\dddot{M}}$

(*c*) *By heating peroxide of manganese with about its own weight of strong sulphuric acid in a green glass retort.*—This process is only to be followed when you have not the apparatus for procuring it by the second method. The results are proto-sulphate of manganese and oxygen gas.

Reagents.	Results.
1 $\ddot{M}ang.$	1 O
1 $\dddot{S}$	1 $\dot{M} + \dddot{S}$

Properties.—It is elastic, colourless, odour-

less, tasteless, incombustible, but a supporter of combustion. Its specific gravity is 1·1026; its atomic weight is 8, its combining volume 0·5. It is sparingly soluble in water.

Characteristics.—If a taper or match be plunged into this gas after the flame has been blown out, but while the wick or charcoal remains ignited, the flame is reproduced. The only gas likely to be confounded with oxygen in this respect, is the protoxide of nitrogen, from which oxygen is distinguished by exploding with hydrogen. A mixture of oxygen and hydrogen yields, by explosion, water only, whereas the protoxide of nitrogen with hydrogen, yields water and nitrogen.

Physiological effects. — Let us examine these under the two following heads :—

(*a*) *Effects of the inhalation of oxygen gas.*—We know that oxygen, as a constituent of atmospheric air, is essential to the existence of all living beings, and hence its common appellation—*vital air.* At one period the nitrogen of the atmosphere was supposed to be a negative agent—that is, it was thought to be a mere diluent to the oxygen. This notion doubtless led to the opinion of Priestley, that as a candle burns out much faster in oxygen than in common air, so we might, as it may be said, live too fast, and the animal powers be too soon exhausted in this pure kind of air.

Without occupying your attention unnecessarily, with preconceived notions respecting the effect of this gas, I may state that later experimenters have proved, no very obvious effects result from a few inhalations; that an animal will live longer in a given volume of this gas than in the same volume of atmospheric air, but that the continued use of it causes death. It quickens the respiration and excites the vascular system, then causes feebleness and insensibility; to which symptoms succeed loss of voluntary motion, and, ultimately, death.

The following remarks made by Nysten deserve notice in connexion with the foregoing :—"Respired in the pure state, this gas does not excite any catarrhal affection; injected into the pleura it is very quickly absorbed without causing inflammation, as I have many times assured myself; lastly, dissolved in the venous system, as happens when we have cautiously injected it, no sensible effect is produced on any part of the animal economy."

(*b*) *Effects of oxygenated water.*—By strong pressure water may be made to dissolve a certain quantity of oxygen, and the liquid thus produced has been denominated *oxygenated water.* It must not be confounded with the peroxide of hydrogen. This oxygenated water is said to act as a slight excitant, promoting the appetite and increasing the secretions.

Uses.—In asphyxia, arising from either deficiency of atmospheric air, or from breathing noxious vapours, the inhalation of oxygen has been said to be useful. So also during an attack of spasmodic asthma, when the system seems to be suffering from a want of atmospheric air, oxygen might, perhaps, be inhaled with advantage; but it can only be useful as a palliative. Chaussier has recommended its use in children apparently still-born. It has also been employed in diseases supposed to arise from, or be connected with, a deficiency of oxygen, as malignant cholera, chlorosis, &c. It is, however, rarely employed.

2. Chlorine Gas.

History and synonymes.—This gas was discovered by Scheele in 1774, who termed it *dephlogisticated muriatic acid.* Berthollet, in 1785, named it *oxygenated muriatic acid*, from an erroneous notion of its composition. Sir H. Davy, who determined its elementary nature, called it *chlorine* (from χλωρος, green) on account of its colour.

Native state.—It is found in both kingdoms of nature. Thus it exists native in combination with a number of metals, as sodium, lead, silver, &c. Free hydrochloric acid is found in the neighbourhood of volcanoes, in all probability being evolved from some chloride or muriatic salt; and, according to Dr. Prout, it exists in the free state in the stomach of animals during the process of digestion.

Preparation.—The best method of procuring chlorine is to introduce a mixture of one part common salt and one part powdered oxide of manganese into a retort, and then add two parts strong sulphuric acid (diluted with two parts water). By the application of a gentle heat the gas is copiously evolved. In the absence of peroxide of manganese we might obtain chlorine by adding muriatic acid to the chloride of lime, which is now found in most chemists' shops.

The *theory* of the process is this :—One atom of sulphuric acid acting on an atom of peroxide of manganese generates the proto-sulphate of manganese, while one atom of oxygen is set free; this immediately combines with an atom of sodium of the common salt, to form soda, with which some sulphuric acid unites, producing sulphate of soda : the chlorine of the common salt is set free.

Reagents.	Results.
$N + Chl.$	$Chl.$
$\ddot{M}$	$\dot{N} + \dddot{S}$
$2\ \dddot{S}$	$\dot{M} + \dddot{S}$

Properties of the gas.—It is elastic, and has a yellowish green colour, and a pungent suffocating odour. Its specific gravity

is 2·47. It is not combustible, but is a powerful supporter of combustion. Some substances, as phosphorus and antimony, take fire when introduced into it. It destroys vegetable colours when water is present; its efficacy depending on its decomposing the water and uniting with the hydrogen to form hydrochloric acid, while the colouring matter combines with the disengaged oxygen.

Oxide of colouring matter (i. e. colourless matter).

Colouring matter	Oxygen	*Water.*
Chlorine	Hydrogen	

Hydrochloric Acid.

The atomic weight of chlorine is 36: its atomic volume 1.

Characteristics.—The colour, odour, and bleaching property of chlorine, readily distinguish it. Dissolved in water, small quantities of this gas are distinguished by the power the solution has of dissolving leaf gold. In common with free muriatic acid and the chlorides, it precipitates the nitrate of silver white; the chloride of silver thus formed being soluble in ammonia, but insoluble in nitric acid.

Physiological effects.—Chlorine gas is a *local* irritant. Mr. Wallace tells us that diluted with air, or aqueous vapour, of 116° Far., and applied to the skin, it produces peculiar sensations, similar to those caused by the bite or sting of insects: this effect is accompanied with copious perspiration, and a determination of blood to the skin, sometimes attended with an eruption of minute papulæ, or even vesicles. Applied to the skin in a pure form, its action is similar, but more energetic.

If an attempt be made to inspire undiluted chlorine gas, immediate death may be the consequence, from spasm of the glottis. If the gas be mixed with air, it enters into the bronchial ramifications, produces a sensation of tightness and suffocation, and violent cough. Twice I have suffered most severely from the accidental inhalation of it; and each time I have been impressed with the belief, founded on my own feelings, that it caused a diminution of the calibre of the air-tubes, arising from a spasmodic condition of the muscular fibres of the bronchial tubes. The attack usually goes off in increased secretion from the mucous membrane. When diluted with a large quantity of air, chlorine may be inhaled without exciting cough: it occasions a sensation of warmth in the respiratory passages, and promotes expectoration.

The irritating effects of chlorine are less powerful on those accustomed to inhale it; as I have repeatedly seen in patients who were using the gas, and which is also shown by a statement made by Dr. Christison. "I have been told (says he) by a chemical manufacturer at Belfast, that his workmen can work with impunity in an atmosphere of chlorine, where he himself could not remain above a few minutes."

The constitutional or *remote* effect caused by inhalations of chlorine, is increased frequency of the pulse and of respiration. But this effect may be in part owing to the increased muscular efforts of the patient. Mr. Wallace states, that the application of chlorine to the skin also occasions soreness of the mouth, fauces, and œsophagus; increased vascularity, and even minute ulcerations of these parts, and an alteration in the quantity and quality of the salivary and biliary secretions. He thinks also that it has a tranquillizing, and at the same time exciting power, with respect to the nervous system.

When applied to the skin, or bronchial membrane, does chlorine gas become absorbed? If Mr. Wallace's observation be correct, we must infer that it does, and that it is thrown out of the system by the kidneys; for he says the urine acquires bleaching properties. Nysten has tried the effects of injecting the gas into the blood; and found small quantities produced little effect, but larger ones occasioned difficult respiration, and in three minutes death: the blood was fluid, and, even in the left auricle and ventricle, had a venous character.

Uses.—(*a.*) *As a fumigating agent, disinfectant, and antiseptic*, chlorine, I believe, stands unrivalled. Hallé, in 1785, appears to have been the first person who employed it as a disinfectant; but we are greatly indebted to Guyton-Morveau for the zeal and energy he manifested in his attempts to introduce it into use. For destroying miasmata, noxious effluvia, and putrid odours, it is the most powerful agent known; and is, therefore, well adapted for disinfecting prisons, ships, hospitals, dissecting rooms, and all other places, the air of which requires purification. The best method of fumigating a large building is that adopted by Dr. Faraday, at the General Penitentiary at Milbank. One part of common salt was intimately mixed with one part of the black oxide of manganese; then placed in a shallow earthen pan, and two parts of oil of vitriol, previously diluted with two parts by measure of water poured over it, and the whole stirred with a stick. Chlorine continued to be liberated from this mixture for four days. To show you on what an immense scale these operations were conducted, I may add, that he consumed 700 lbs. of common salt, 700 lbs. of oxide of manganese, and 1400 lbs. of sulphuric acid. The disinfecting power of chlorine

is supposed to depend on its affinity for hydrogen, by which it effects the decomposition of water or aqueous vapour, the hydrogen of which it unites with, while the oxygen oxidizes the organic matter; or it may act merely by abstracting hydrogen from the putrid miasmata.

(*b.*) *As an antidote in poisoning by hydrocyanic acid, sulphuretted hydrogen, or hydrosulphate of ammonia*, chlorine gas is a very valuable agent. In a very recent lecture, I sufficiently examined the efficacy of this antidote in poisoning by the hydrocyanic acid. I believe, however, that the chloride of lime will be found a more convenient, safe, and opportune agent: its activity depends on the chlorine present. The beneficial influence of chlorine in the treatment of animals asphyxiated by sulphuretted hydrogen, doubtless arises, at least in part, from its chemical properties; for when the two gases are mixed, we obtain chloride of sulphur and hydrochloric acid. The best method of applying the remedy is to diffuse a little chlorine in the air, and then to produce artificial respiration.

(*c.*) *Inhaled in chronic pulmonary diseases* it is sometimes a useful remedy. I have carefully watched its effects in phthisis and other chronic diseases of the lungs; and the result of my observation is, that chlorine is rarely serviceable. Frequently, after the first and second inhalations, the patients fancy their breathing much relieved, while the expectoration is promoted, but the amendment is seldom permanent. I need hardly say it has no pretensions to the cure of phthisis, but it may be useful as a palliative (sometimes diminishing the sweating); and I can readily believe that occasionally in chronic bronchitis it may be of essential service, though, I confess, I have never found it so.

The mode of administering the gas I have already detailed in a former lecture *. You may put either the aqueous solution of chlorine or a small portion of the chloride of lime, into the inhaling bottle: if the latter be not sufficiently strong, a few drops of muriatic acid are to be added.

(*d.*) *In diseases of the liver*, not attended with active inflammation, Mr. Wallace has successfully employed baths of gaseous chlorine, either in the pure state or diluted with air or aqueous vapour. The benefit of chlorine in these cases has been confirmed by others. The temperature of the bath, and the time the patient ought to remain in it, will vary in different instances; but Mr. Wallace thinks, that, in the greater number, 115° Fah. will be found to answer best, and the proper time about half an hour. The benefit obtained is in part referable to the heat employed, in part to the irritant effect of the chlorine on the skin, and (according to Mr. Wallace) in part to the specific influence of chlorine on the liver. Ziese, an apothecary at Altona, has also employed chlorine baths in these cases with advantage.

* See Lecture V. p. 132 of the present vol.

Antidotes.—The inhalation of ammoniacal gas, of the vapour of warm water, of spirit of wine, or of æther, has been recommended, to relieve the effects of inhaling chlorine. I tried them all when suffering myself, but without the least apparent benefit. In a case related by Kastner, and which is reported in Wibmer's work, the inspiration of sulphuretted hydrogen gave uncommon relief. If you employ this agent, do it cautiously; recollect the proposed antidote is a powerful poison.

(*a.*) *Solution of Chlorine.*

Preparation. — Chlorine dissolves in water, forming the *liquor chlorinii*, or *liquid oxymuriatic acid*. At the ordinary temperature and pressure of the atmosphere, one volume of water can dissolve two volumes of the gas. The easiest way of procuring this solution is the following: —generate the gas (from the ingredients already described) in a common oil-flask; and by means of a curved glass tube (one end of which is adapted to the flask by a cork), pass the gas into water contained in a double-necked bottle.

Properties.—A saturated solution of chlorine has a greenish yellow colour, and a strong odour of the gas. It bleaches vegetable colours—as tincture of litmus, turmeric, &c. By exposure to light, the water is decomposed, the oxygen is evolved, while the hydrogen unites with the chlorine to form muriatic acid.

Characteristic tests.—Like muriatic acid, the muriates, and the chlorides, it causes a precipitate with nitrate of silver, which is soluble in ammonia, but is insoluble in hot nitric acid. It is distinguished from them by its bleaching properties, its dissolving leaf gold, and its removing the blue colour of iodide of starch and of sulphate of indigo.

Physiological effects.—In a concentrated form, the aqueous solution of chlorine acts as a corrosive poison. Somewhat diluted it ceases to be a caustic, but is a powerful local irritant. Administered in proper doses (a few drops), and properly diluted, it is given internally, as a tonic and stimulant. The continued use of it is said to have caused salivation.

Uses.—Solution of chlorine has been employed therapeutically both externally and internally.

(*a*) *Externally*,—it has been used in the concentrated form as a caustic, applied to wounds caused by rabid animals; diluted, it has been employed as a wash in skin diseases, namely, in the itch and porrigo, and also as an application to cancerous and other ulcers attended with a fœtid discharge. In the latter cases I have repeatedly employed it with advantage, though I give the preference to a solution of the chloride of soda.

(*b*) *Internally*,—it has been administered in those diseases denominated putrid; for example, in the worst forms of typhus and scarlet fever. It has also been employed in venereal maladies. The dose will vary with the strength of the solution: if the latter be concentrated, from 20 to 30 drops in about an ounce of some diluent will be the proper dose.

Antidotes.—In cases of poisoning by a solution of chlorine, we are advised to employ those remedies used in poisoning by the acids, namely, magnesia, chalk, &c. A mixture of albumen and water, however, has been recommended by Devergie. For this purpose mix up some white of eggs with water. In the absence of these, use milk, (the caseum of which will be nearly as useful). The compound of chlorine and albumen which is formed, has very little action on the animal economy, and it may be readily evacuated from the stomach. The gastro enteritic symptoms are to be combated in the usual way.

(*b*.) *Chloride of Lime*.

History.—In the year 1798, a patent was granted to Mr. Tennant, a manufacturing chemist of Glasgow, for the manufacture of the substance now usually denominated *chloride of lime*, but which has been known by several other denominations, such as *bleaching powder*; *oxymuriate of lime*; *chloruret of the oxide of calcium*; and *chlorite of lime*. Do not mistake this substance for the chloride of calcium, a compound altogether different in its properties; nor for the chlorate of lime.

Preparation.—This substance is prepared on a most extensive scale for the use of bleachers; and a full account of its manufacture has been published by Dr. Ure, in the 13th vol. of the *Quarterly Journal of Science*. The principle of the process is simple, being that of merely bringing the gas in contact with recently slacked lime. On the large scale, the gas is generated in large, nearly spherical, leaden vessels, heated by steam; the gas produced is then washed by passing through water, and is carried by a leaden pipe into the combination room, where the slacked lime is placed in shelves or trays, piled one over another to the height of five or six feet, cross bars below each, keeping them about an inch asunder, that the gas may have free room to circulate.

Properties.—Chloride of lime, as met with in commerce, is a white pulverulent substance, having a feeble odour of chlorine, and possessing bleaching properties. Chemists are not agreed as to its nature. Thomson regards it as an atomic combination of chlorine and lime: Ure says it resembles "rather a mixture (or at most a saline solution) than a true atomic compound." Berzelius and Dumas, on the other hand, consider it to be a mixture of *chlorite of lime* and *chloride of calcium*,—its essential properties depending on the chlorite. They suppose that when the chlorine comes in contact with the lime, part of the latter is decomposed into its elements oxygen and calcium; the first forming, with chlorine, *chlorous acid*, which, with some undecomposed lime, forms *chlorite of lime*, while some free chlorine combining with the calcium, furnishes *chloride of calcium*.

Reagents.	Results.
4 Chl.	3 ($\dot{\text{Ca}}$. $\dddot{\text{Chl}}$).
4 $\dot{\text{Ca}}$.	Ca. Chl.

I have mentioned this opinion of Berzelius and Dumas, because from the high characters of these philosophers every theory of theirs deserves attention; but I confess it appears to me that we have not a sufficient number of facts to induce us to adopt their opinion, while several strong objections may be urged against it.

Characteristic tests.—The chloride of lime is recognized by the following characters: On the addition of sulphuric or muriatic acid, it readily gives out chlorine; its solution bleaches; it precipitates a solution of nitrate of silver white (the precipitate being soluble in ammonia, but insoluble in nitric acid); and a white precipitate is produced on the addition of oxalic acid or oxalate of ammonia. The last test shews the nature of the base, the others recognize the chlorine.

Physiological effects.—The effects of the chloride of lime on the system, have hardly as yet been properly determined. Locally, it evidently acts as an irritant; indeed, the lime present, and which the chlorine very imperfectly neutralizes, would render it so, and doubtless in its concentrated form it is to be regarded as a caustic. What the remote or constitutional effects of the chloride of lime may be, have not yet been ascertained experimentally. Cima regards it as a stimulant to the lymphatic glands, but without any good evidence.

Uses.—I shall speak of the uses conjointly with those of chloride of soda.

(c.) *Chloride of Soda.*

History.—This substance is sometimes termed *chloruret of the oxide of sodium, oxymuriate or chlorite of soda:* it constitutes the *liqueur de Labarraque.* In some works it is by mistake called *chlorate of soda,* a preparation never used in medicine. You must not confound chloride of soda with common salt (chloride of sodium).

Preparation.—It is prepared by passing chlorine gas into a solution of the subcarbonate of soda. The following quantities will produce a solution about the strength of that recommended by Labarraque:—Dissolve one ounce of subcarbonate of soda in four fluid ounces of distilled water, and into this solution pass chlorine gas generated from 2 drachms of peroxide manganese, 2 drachms of common salt, and 4 drachms of sulphuric acid, mixed with 4 drachms of water.

Payen has proposed to prepare it thus: Dissolve 2 parts of subcarbonate of soda in 18 parts of water, and add 1 part of the chloride of lime: filter. If only $1\frac{38}{100}$ of the subcarbonate be employed, the chloride will be neutral. If the subcarbonate of potash be employed in place of the soda, we should obtain a solution of the chloride of potash, formerly called *Eau de Javelle.*

Properties.—Labarraque's solution contains carbonate, bi-carbonate, and chloride of soda; or, to adopt the theory of Berzelius and Dumas, it is a mixture of carbonate, bi-carbonate, and chloride of soda, with chloride of sodium. It bleaches like the chloride of lime.

Characteristic tests.—These are similar to those mentioned for the chloride of lime, except that neither oxalic acid nor oxalate of ammonia causes a precipitate, showing that no lime is present: the chloride of platinum occasions no yellow precipitate, proving it is not the chloride of potash which is present. The solution, evaporated to dryness, leaves a residuum which renders the outer cone of the flame of a candle, or the flame of a spirit-lamp, yellow.

Physiological effects.—A strong solution of the chloride of soda acts as a corrosive poison, diluted as a local irritant merely. We are at present very imperfectly acquainted with its remote action.

Uses of the chlorides of lime and soda.—I propose to examine the uses of the chlorides of lime and soda under one head, and thereby to save much repetition, since they act in a very analogous manner, and are employed in the same cases. As the chloride of lime is manufactured on a very extensive scale for the use of bleachers, it can, of course, be obtained at a cheaper rate than the chloride of soda, and on this account, therefore, will probably always be more extensively employed in medicine, besides which, it is much more frequently met with in the shops, and hence can be obtained with greater facility. If, however, I were confined to the use of one chloride only, I should choose the chloride of soda, having frequently found this serviceable when the chloride of lime was not. Labarraque also gives the preference to the soda preparation, on the ground that by the process of disinfection these chlorides lose oxygen, the chloride of lime becoming chloride of calcium, while the chloride of soda is converted into the chloride of sodium or common salt. Now these two resulting chlorides possess different properties: chloride of calcium is a very deliquescent salt, while chloride of sodium is designated a "very dry salt." By attracting water, the chloride of calcium furnishes one of the conditions fit for re-producing the putrefactive process, whereas chloride of sodium "acts as a preservative, by coagulating the principle which commences putrefaction." These reasons (not very valid ones, I confess) led Labarraque to the conclusion, that while chloride of lime will serve for simple disinfection, chloride of soda is preferable when we wish at the same time to prevent the renewal of putrefaction. The chemical influence of these chlorides, or disinfectants, depends on the chlorine, which is separated from the base, by the union of the latter with the carbonic acid of the air.

1. *As disinfectants, antiseptics, &c.*—I have already stated my opinion that chlorine is unrivalled for its disinfecting and antiseptic properties; but you are not, therefore, to imagine, that to gain these effects we must necessarily employ it in its gaseous form. If you are called on to disinfect large uninhabited buildings, you could not do better than employ chlorine fumigation, as already described. But this process would not be admissible in inhabited places, on account of the irritating properties of the gas. Now it is in these cases that the chlorides are found so superior in practice to the chlorine fumigations. They evolve chlorine gas, it is true, but in such small quantities, as not to exercise any noxious effects; while they possess the same kind of disinfecting and antiseptic properties. Thus dissecting-rooms might be readily disinfected by throwing about a solution of these chlorides, while the anatomist was pursuing his studies.

These chlorides also possess another advantage: they may be applied with the greatest facility to substances in a state of putrefaction, so as not only to disinfect, but to stop the putrefactive process. It is with this view that we apply them to gangrenous parts, or to ulcers attended

with a fœtid discharge, thus ameliorating the condition of the patient, and at the same time counteracting what would otherwise be a great annoyance to his friends and attendants.

I should give but a very imperfect idea of the value of the chloride solutions in these cases, did I lead you to imagine their efficacy was confined to an action on the dead parts, or on the discharges from wounds or ulcers; they are also of the highest benefit to the living parts. Frequently by their employment the further progress of gangrene is stopped, the separation of the dead from the living parts promoted, and the secretions altered both in quality and quantity. To enumerate particular instances is, I conceive, unnecessary; it will be sufficient to say, that in all cases of gangrene, in ulcers attended with sloughing, or with a fœtid sanious discharge, whether phagedenic, cancerous, syphilitic, or scrofulous; in compound fractures also, which are frequently attended with most offensive discharges, you will find these chlorides of essential service, not only by their disinfecting and antiseptic qualities on non-vital substances, but also by their beneficial action on the living parts. On many occasions the chlorides are invaluable agents in the sick chamber, either by destroying unpleasant odours, or contagious or infectious matters. I have frequently recommended, with the greatest advantage to the attendants, a handkerchief dipped in a weak solution of the chloride to be suspended in the chamber of the patients suffering with fever. To counteract the unpleasant smell of the dressings or bandages, or of the secretions from patients, these chlorides will be found most useful.

2. *Chloride gargles.*—On many occasions gargles containing the chloride of soda or of lime prove of service: for example, in ptyalism and ulceration of the mouth, whether originating from the employment of mercury, or arising spontaneously; in ulceration of the throat, particularly that kind observed in malignant scarlatina, I have obtained beneficial results from the employment of chloride gargles.

3. *In poisoning by sulphuretted hydrogen gas, by the hydrosulphuret of ammonia, by the sulphurets of potassium and sodium, and by hydrocyanic acid* (some have added carbonic acid), the chlorides are of signal utility: in fact they may be regarded as genuine antidotes, since they decompose those poisons, and render them comparatively inert. When the patient is in a condition to swallow, I would recommend that a solution of the chloride should always be administered, in addition to the inhalation of the vapour. The latter is to be effected by dipping a handkerchief in a solution of the chloride, and applying it to the nose. It was by breathing air impregnated with the vapour arising from the chloride of lime, that Mr. Roberts (the inventor of the miner's improved safety-lamp), was enabled to enter, and traverse with safety, the sewer of the Bastile, which had not been cleansed for thirty-seven years, and which was impregnated with sulphuretted hydrogen. You will find an interesting account of his experiments in the late Mr. Alcock's "Essay" on the Chlorurets. If you were required to enter any place suspected to contain sulphuretted hydrogen, the safest mode of proceeding would be to place a handkerchief moistened by a solution of chloride of lime over the mouth and nostrils, so that all the air inspired would by this means be purified.

4. *In burns and scalds.*—Lisfranc has employed with great benefit lotions of the chloride of lime, sometimes immediately after the accident, sometimes after the application of emollient cataplasms. His method of using it is this: lint moistened with a solution of chloride of lime is applied to the part, and over this a waxed cloth. In one case, where almost the whole of the lower limbs, the arms, and face, had been burnt, the use of the chloride recovered the patient from the stupor into which he had fallen, at the end of four days; and a perfect recovery was effected two months after the accident.

5. *Injections* of the chlorides have been employed in diseases of the uterus, in gonorrhœa, in fistulous ulcers, &c. with advantage. In several very obstinate gleets I have tried a solution of chloride of lime, but without benefit. The chloride of soda is, I think, more useful.

6. In chronic *skin diseases*, where stimulating washes are usually applied, the chlorides have been beneficially employed. Thus in porrigo, impetigo, scabies, prurigo, and other cutaneous affections, lotions containing the chloride of soda have been found very beneficial. In a very obstinate case of prurigo pudendi muliebris, related by Dr. Darling, the chloride of soda afforded almost instantaneous relief.

In the year 1810, Cluzel observed that some Spanish prisoners were cured of the itch by dipping their hands into a solution of chlorine, for the purpose of preserving them against contagious fever. Derheims has used with the most perfect success, in the same disease, a lotion composed of three ounces of the chloride of lime to a pint of water. This is a much more pleasant and agreeable method of curing the itch than employing sulphur frictions.

7. *Ophthalmia* has been benefited by the chloride of lime solution. In the Medical and Physical Journal for November, 1827,

Dr. Varlez, surgeon to the Military Hospital at Brussels, states that in 400 cases this remedy never disappointed him; and Mr. Guthrie also reports favourably of it in three cases. MM. Colson, Delatte, and Raynaud, also speak in high terms of it. The strength of the solution employed by Dr. Varlez varied from one scruple of the chloride to three or four drachms, dissolved in an ounce of water. This was dropped into the eye, or injected by a syringe, or applied by means of a camel's hair pencil. Other means, however, must be conjoined, namely, bleeding, purging, cold, and, in chronic cases, blisters. I have seen a weak solution of great benefit in several cases of the purulent ophthalmia of infants.

8. *Internally* the chlorides (particularly the chloride of soda) have been employed in infectious fevers, venereal diseases, dyspepsia, &c.

Antidotes.—In the event of poisoning by chloride of lime or soda, you must carefully avoid administering acids, as they would cause the evolution of chlorine gas, which would not only irritate and inflame the stomach and œsophagus, but being inhaled, might perchance cause asphyxia or bronchial inflammation. The best plan, therefore, would be to administer albuminous or caseous liquids (as eggs beat up with water, or milk), or flour and water, or oil, or mucilaginous drinks, and to excite vomiting.

OBSERVATIONS

ON

URINARY DEPOSITS.

To the Editor of the Medical Gazette.

Sir,

Should the following observations on urinary deposits, which formed the subject matter of a thesis lately read before the Physical Society of Guy's Hospital, be deemed of sufficient interest, you will oblige by giving them a place in the columns of your valuable journal.

Your obedient servant,

R. H. Brett, M.R.C.S.

1, Upper Sussex-place, Old Kent Road,
Feb. 4th, 1836.

Comparison of Vital Chemistry with Inorganic.

The great advantages which have accrued to science from the investigation of chemical pathology must be manifest even to those whose attention has not been particularly directed to this branch of inquiry, the difficulties of which have certainly not diminished in proportion as its advocates have passed from the consideration of general principles to the careful examination of minute details.

The two great kingdoms of nature, that which comprehends inert and inanimate matter, and that which claims as its own the complex forms of living structures, are of a nature so widely different, that we can hardly be surprised that that mode of research which has been so pre-eminently successful in bringing before our view the elementary constituents of the one, should, in many instances at least, be found totally inefficient, when applied to the investigation of the other. Those laws which preside over the external conformation or molecular arrangement of inorganic matter, have, as far as our experience has hitherto enabled us to judge, remained immutable, so that those elements which have at one time under certain circumstances given rise by their association with other elementary principles to a definite form of matter, shall at another time under the same train of circumstances give birth to an analogous form, thus giving us the power of predicating certain results. But a similar mode of reasoning will not apply to that species of matter which, disdaining to be trammelled by laws purely physical, arrogates to itself a presiding agent for the regulation of its varied changes, of a nature so subtile as to elude our keenest scrutiny, and baffle all our ablest arts in the attempt to gain a mere glimpse of its true essence. What, indeed, can be more different than the chemistry of life and that of inert matter: the one carried on in all its inscrutable and mysterious process, without the aid of crucible or furnace, in the laboratory of a living system—wonderful in its complexity, though beautiful in its design and construction, with life for the directing alchymist; the other perfected by mere human ingenuity, exercised over the mute and senseless materials of the inorganic world! If, then, our imperfect knowledge altogether forbids us from following vital chemistry in the manifold details of her intricate processes, it is at least given us to examine the products which accrue from such processes; and although our labours in this field of inquiry must be crowned with much less satisfactory results than when engaged in sifting the elements of inorganic matter, still, considering the almost infant state of

this department of chemical knowledge, much that lays just claim to ability, and much that may be applied practically, has flowed from its steady cultivation. Our knowledge of the constituent parts of the various secretions and excretions is of a very different stamp from that which our predecessors enjoyed, and imperfect as it still must be allowed to be, has nevertheless led to a much more scientific and much more efficient mode of treating disease. Correct information relative to the proportions in which certain proximate and even ultimate principles enter into the constitution of that pabulum of our existence—the blood, when in a healthy condition, is absolutely necessary, in order to allow of our detecting any deviation from that state, which, none but the most determined solidists will deny, may be the fruitful cause of disease. What that aberration from the normal standard precisely may be in the varied forms which disease puts on, must, I fear, be left for a more enlightened chemical era than the present to determine; but doubtless, considering the rapid advances which science now makes, the time may be confidently looked forward to when such a knowledge shall enable its possessors to apply their remedial agents more directly to the vital fluid itself, be its diseased condition the result of a deficiency or redundancy of its healthy constituents, or be it caused by such a perverted or morbid state of the latter as to give rise to new products. And when we remember that the ultimate elements of all organic principles strictly called animal, however different in their physical and chemical nature such principles may be, actually exist in the blood, it requires no inordinate or unreasonable stretch of the imagination to allow that vital agency may so alter the atomic arrangement of such elemental bodies as to give rise to proximate principles of a nature altogether different from those which we meet with in healthy blood; by a mode of synthesis, indeed, totally removed from our powers of comprehension.

The condition of the urinary secretion has of late years been very attentively studied, and its departure from the healthy standard very carefully marked; and there is, perhaps, no secretion so prone to undergo changes from the influence of morbific causes. There is scarcely a disease in the whole catalogue of ailments to which human nature is subject, where this fluid will not be found more or less altered from its healthy condition. In the phlegmasiæ its aberration from that state is constantly, or almost constantly, manifested: in those diseases, too, which, if inflammatory, are of a decidedly specific character, such as rheumatism and gout, both acute and chronic, this secretion has long been noticed as undergoing some peculiar alteration. In the varied forms of dropsy this fact is not less obvious. In those morbid states of the system in which the lumbar portion of the spinal nerves appears principally affected, a corresponding derangement of the urinary secretion is commonly met with. In those proteus-like forms of disease, hysteria and dyspepsia, changes of a no less marked character in the urine are constantly being brought before our notice. In that hitherto opprobrium of the medical art, both in respect to its pathology and treatment, I mean diabetes, an extraordinary and very unusual change is observed to take place.

A knowledge of these several facts has justly led medical men to attribute no trifling importance to the study of the chemical pathology of the urine, and the happiest results have followed its careful investigation, both in throwing an additional light upon the pathology of the urinary organs in particular, and in rendering diagnosis more clear and perfect, and consequently fixing the treatment of disease upon a more scientific and more efficient basis.

To enter upon a subject which, although it has been much agitated by some of the ablest investigators, is still very far (owing to the nature of the subject) from even an approach to perfection, needs, I think, no apology, more especially as the remarks which are about to follow will not be directed so particularly to the urine itself, as to the deposits which that secretion under various circumstances gives rise to; nor will exclusive attention be paid to those deposits which are commonly classed under the term urinary.

Nature of the present Inquiry.

It is my intention in this paper to treat in the first place of those deposits, or sediments, the constituents of which may be found in healthy urine, though not precisely in the same state of combination as we find them in that secre-

tion, and which become manifest either from disordered action of the kidney or urinary bladder. Secondly, of these which result from certain ingesta being taken up by the absorbents, and transferred without undergoing decomposition into the urinary passages, the constituents of which are not to be found in healthy urine; and lastly, those deposits, also consisting of principles not to be found in healthy urine, and which accrue either from a deranged action of the kidney, of a nature different from that observed as giving rise to the first class, by which the blood is allowed to pass into the urinary canals without undergoing that change which the healthy action of secretion always produces, or from actual disorganization of the kidney, or a morbid secretion from the urinary canals or bladder. And whilst noticing these various deposits, I shall endeavour to point out what appears to be the best means of distinguishing them from each other, independently of their mere physical differences, without which knowledge a correct diagnosis can hardly be formed, or satisfactory plan of treatment pursued.

Deposits, Organic or Inorganic.

Those deposits which consist of the same constituents as are found in healthy urine, may be divided into those which are of a strictly organic nature, into those which are made up of an organic acid and earthy or alkaline base, and into those which consist of an inorganic acid and earthy or alkaline base. The organic deposits consist of mucus, lithic acid, and the colouring principle of the urine; which last, when precipitated, is always combined with some of the saline constituents of the urine, and never in a free state. Those which consist of an organic acid and earthy or alkaline base, are made up of lithate of ammonia, lithate of soda, lithate of lime, and some other salts, commonly in minute proportions, and not essential to the deposit. Those consisting of an inorganic acid, and earthy and alkaline base, are made up of phosphate of lime and phosphate of ammonia and magnesia.

Mucous Deposits.

The organic deposits must be now noticed; and first, mucus. This substance, which is a secretion from the mucous membrane of the bladder, is always deposited even from healthy urine, upon cooling, although in small quantity. It is probably from the presence of this substance that healthy fresh urine gives evidence of a certain degree of viscidity when agitated. It is most probable that the mucus is not in a state of solution in the urine, but only suspended through that fluid in a state of exceedingly fine division. When urine is first voided, the mucus cannot be perceived, in consequence of its power of refraction being about the same as that of the fluid through which it is diffused; when, however, the latter cools, the mucous particles cohere, become increased in density, and consequently in refrangibility. It must not be supposed that the mucus of the urinary bladder is precisely similar to that secretion as found on other mucous surfaces; they all, indeed, have one physical property in common—viz. that of viscidity, but, nevertheless, experiment has shown that the mucus obtained from the gall-bladder, that from the stomach and intestinal tube, and, lastly, that obtained from the urinary bladder (which is the species to be presently more fully noticed), all differ in some of their chemical habitudes. Thus the mucus obtained from the lining of the gall-bladder, is, according to Berzelius, totally insoluble in acids; whereas that obtained from the urinary bladder is soluble, to a certain extent, both in diluted acids as well as in alkaline solutions: nor is it to be deemed, I think, a matter for surprise that this should be the case, when we reflect upon the different conditions different structures supplied with mucous membrane are placed under. Thus the bronchial tubes, the stomach and intestinal canal, and, lastly, the urinary bladder, are very differently circumstanced; the first being constantly exposed to the action of the external atmosphere, the second to the influence of an endless variety of ingesta, and the third to a highly irritating and saline fluid. The following are the appearances which the small quantity of mucus which always comes down from healthy urine upon cooling assumes. After the urine has stood for some time in a tall glass vessel, the lower strata of the fluid will be found to have lost their transparency, and an exceedingly light nebulous-looking substance will be observed floating in it: if the whole be thrown upon a filter, with a view of examining the

nature of this cloudy deposit when separated from the supernatant fluid, so exceedingly light and inconsiderable in quantity is the mucus, that no appreciable residue will remain on the filter, unless a very considerable quantity of urine be employed. But although the mucous deposit from healthy urine is thus inconsiderable in quantity, it is very different in disease; then it is that the deposition for the most part takes place before the urine is evacuated from the bladder; consequently that fluid will be found turbid, and sometimes even ropy, at the moment of its emission. If urine of this character be allowed to stand for a few hours in a tall glass vessel, a very abundant opaque deposit will take place: this deposit is ropy and tenacious, and devoid of the yellowish green colour which is so characteristic of the purulent deposit to be noticed hereafter. When collected upon a filter and washed, it appears semi-gelatinous from the absorption of water, is somewhat less opaque, and sometimes, from its exceeding tenacity, may be drawn out into threads. When allowed to dry on the filter it shrinks considerably, and assumes the character of a yellowish varnish: if it be then moistened with water it regains its former pulpy appearance and viscid character, and will be found to be soluble in caustic alkali, and, to a certain extent, also in acetic acid. It is sometimes found associated with a very considerable quantity of phosphate of lime; at other times, however, the proportion of the earthy phosphatic salt is inconsiderable, and in some cases the urate of ammonia is found in combination with the mucous deposit. The best mode of distinguishing this vesicle mucus from pus will be noticed when the latter substance is treated of.

There is a peculiar substance which appears to me to be a species of mucus, found not unfrequently under the following circumstances, but which I do not find noticed any where. I have observed in several instances, more particularly where the urine has deposited that form of lithates which is either so pale as to approach closely in physical character to the phosphate of lime, or accompanied with only a very small proportion of the colouring matter of the urine, giving it a yellowish or very pale fawn colour, that although the urine thus rendered turbid by the deposit in question, clears entirely by the application of heat, even below the boiling point of water, still, if, after it has been rendered thus transparent, brisk ebullition be kept up for a short time, it becomes again turbid; not, indeed, to the same extent as at first, but nevertheless manifestly so, and this takes place without any diminution of that acidary reaction which the urine has been observed to possess in these cases, both before and after boiling. If a drop or two of concentrated nitric or muriatic acid be added to this turbid urine, it becomes in no degree clearer; such a deposit cannot, therefore, be phosphate of lime: that it is not albumen is shown, both by the effect of a temperature applied below the boiling point of water, which does not cause this deposit to take place, although it always coagulates albumen; in addition to which it is found, that if nitric acid be added to such urine as soon as it is voided, and whilst perfectly clear, no turbidity is produced.

Lastly, it cannot be considered identical with that form of mucus which Dr. Prout speaks of as being thrown down by heat from certain specimens of urine, and which, he considers, is probably derived from the prostate gland, for this last, according to Prout, is coagulated by acetic acid, which is not the case with the peculiar substance in question; for acetic acid added to such urine, recently voided, causes no alteration. Its properties were further examined by collecting it on a filter, and in doing this care must be taken to throw the urine on the filter whilst hot, otherwise the lithates will become deposited and mixed with the organic substance. When thus separated by filtration it assumes the appearance of a semitransparent, somewhat tenacious substance, behaving towards re-agents much in the same way as mucus obtained from healthy urine upon cooling. The fact of this substance being thrown down from urine by boiling might lead one to suspect the presence of albumen in the fluid, and I doubt not this mistake has occasionally been fallen into. If urine, yielding this organic deposit by boiling, be rendered transparent by warming, so as to take up the lithates diffused through it, and then treated with tincture of galls or corrosive sublimate in solution, a slight troubling is induced; this, however, is not caused by albumen, for mucus and the extractive matter of

the urine, which is soluble in alcohol of a certain specific gravity, cause a troubling when tincture of galls is added to their solutions, and it is only when tincture of galls causes a considerable precipitate in urine that we have very strong reasons to suspect the presence of albumen. The phosphates and lithates contained in the urine may also, under certain circumstances, precipitate corrosive sublimate, for the phosphate of the peroxide of mercury is insoluble in water, though soluble in an excess of phosphoric acid: neither can this substance be confounded with caseous matter, for this last is thrown down—viz., caseous matter, by acetic acid; whereas it has been already shown that the urine in question is not affected by that acid. It is not improbable that the peculiar substance Prout speaks of as derived from the prostate gland, is caseous matter.

Uric or Lithic Acid Deposits.

The next deposit of a strictly organic nature which comes under consideration is the lithic or uric acid. This principle is never deposited in a state of perfect purity, it being invariably combined with a larger or smaller proportion of the colouring matter of the urine, and the merest trace of earthy phosphate: it constitutes the crystalline sediment or gravel of Dr. Prout, and this chemist is of opinion that the acid does not exist in a free state in the urine, but in combination with ammonia. He also considers that urate of ammonia is not incompatible in the same solution with the acid phosphate of ammonia. This statement would lead us to the belief that phosphoric acid is capable of dissolving free lithic acid, for we can hardly conceive it possible that lithate of ammonia could exist as such in association with free phosphoric acid, considering the very superior acidifying properties of the latter acid compared with uric acid. This, however, is not precisely the case, for if we take a solution of lithate of ammonia in water, and treat such a solution, divided into two equal portions, with acetic acid and phosphoric acid, the uric acid is thrown down in both cases: it is true, that in the urine the quantity of free phosphoric acid is inconsiderable, and in a state of much dilution, which circumstance might perhaps prevent the precipitation of uric acid. Dr. Prout also argues against the probability of free uric acid existing in the urine, from the fact that the latter acid requires a much larger proportion of water for its solution than it meets with in the urinary secretion, the proportion in that fluid being estimated at 1 part in 1000: but this does not decide the question; for that illustrious chemist Berzelius, has shown that certain inorganic substances are rendered much more soluble in water by the mere addition of certain binary soluble compounds to that menstruum. Thus, he found that iodine, which, in a free state, requires as much as 700 parts of water for solution, is taken up in much larger proportion when the muriate of ammonia or soda is also present, although we are not acquainted with any actual definite compound consisting of these last salts and iodine. Reflecting upon these discrepancies of opinion, it becomes a matter of importance to determine, by direct experiment, whether free lithic acid is, or is not, capable of being dissolved in larger proportion in water, when that menstruum holds in solution the same, or some of the same, salts as are found in healthy urine. For this purpose the acid phosphate of ammonia, with phosphate of soda and muriate of ammonia, all of which are to be found in urine, were dissolved in water, and then a portion of uric acid, much more considerable in quantity than the water in which the above salts were dissolved could have taken up by itself, was added. After submitting this mixture even to a boiling temperature, no appreciable quantity of the uric acid was dissolved. It might, at first sight, appear that this experiment was conclusive of the question, but, upon further consideration, it appeared to me that the following objection might be raised against it:—The lithic acid, in the experiment above detailed, was not recently precipitated or in an hydrous form, for it had been collected on a filter, after throwing it down from the urine by means of sulphuric acid, well washed, and dried at the ordinary atmospheric temperature: it could not, therefore, be fairly regarded as analogous to the same principle when just secreted by the kidneys: for although, in the present state of our knowledge, it is quite impossible to determine in what exact state the uric acid exists when first separated from the blood by the mysterious agency of its natural secreting organ, of this, I think, we may be certain, that it cannot be in

an anhydrous state, or, perhaps, even in a solid form. Admitting the truth of this position, it appeared necessary to alter the conditions of the experiment, and try the effect of the salts above-named upon the acid principle before it could have undergone those manifest, but not readily explicable, changes which matter, both of an organic as well as inorganic nature, undergoes by being brought to a dry state. For this purpose the following experiment was had recourse to:—A portion of urate of ammonia was dissolved in water, and an excess of muriatic acid, so as to precipitate free uric acid, added; the whole was then thrown upon a filter, and the acid washed with cold water until the fluid which came through no longer coloured litmus paper. The uric acid diffused through the last wash-water was divided into three equal portions: to the first of these a given measure of distilled water; to the second, an equal bulk of a solution of the acid phosphate of ammonia, phosphate of soda, and muriate of ammonia; and to the third, the same proportion of fresh healthy urine, were added: these several solutions were then heated for the same length of time: in neither case was a solution of the uric acid obtained. The insoluble residue was also found of nearly equal quantity in all three cases. Hence it may be inferred that uric acid is not rendered more soluble in water when that fluid contains in solution the acid phosphate of ammonia, phosphate of soda, and muriate of ammonia, neither does urine, with its various saline and organic principles, appear to possess the property of holding in solution any additional quantity of uric acid above that which is found as its ordinary complement, and which may possibly exist in a free state. How far certain of the extractive animal matters contained in the urine, with or without any association with its saline constituents, may favour the solubility of free uric acid in water, remains yet to be determined. Dr. Prout has characterized the free lithic acid deposit as crystalline, in which circumstance he considers it differs from the amorphous deposits of urate of ammonia, which do not assume a marked crystalline appearance. Berzelius states that the free uric acid is deposited in an argillaceous form, and acquires, when collected and dried, the appearance of thin plates; the more decided the crystalline form assumed by the precipitate, the less pure is the lithic acid, and the larger the proportion of bases united with it. He states also that a deposition of urate of ammonia from urine, upon cooling, is rare; and believes that the presence of mucus in the urine has a considerable share in causing the deposit of the latter salt. Thus, if two portions of urine be taken, and one carefully filtered whilst warm, so as to free it as much as possible from mucus, the deposit which afterwards takes place from such urine is uncombined lithic acid, in the form of plates, without any marked crystalline structure; whilst the deposit which ensues from the filtered portion of urine assumes a striking crystalline appearance, and contains a considerable quantity of ammonia. This statement of Berzelius respecting the different appearance of these deposits, does not appear to be borne out by facts of an every-day occurrence; by far the greater proportion of urinary deposits are of the non-crystalline kind, and are evidently combinations of uric acid and a base. The consideration of these will not be now entered upon, as they will have to be noticed in full when I come to them in the arrangement started out with, but shall confine my remarks to that deposit which strictly consists of uric acid unassociated with a base, and endeavour to point out in what manner it may be distinguished from all other deposits found in the urine, both as regards its physical properties and its behaviour towards different re-agents.

Tests of Uric Acid.

This peculiar substance, which occurs much less frequently in a free state as a precipitate than when combined with bases, appears to possess a greater affinity for the reddish-brown colouring matter of the urine than any other principle contained in that secretion; it is consequently deposited, for the most part, of a deep red or brown colour; sometimes, indeed, the colour is that of the darkest mahogany: this depth of tint, however, is not very commonly met with, and is, I believe, always co-existent with urine highly impregnated with colouring matter. This sediment is found to adhere with some tenacity to the sides of the containing vessel; and in some cases, when collected upon a filter, washed and dried, assumes an appearance so closely resembling red ferruginous sand, as per-

haps not to be distinguishable from it by mere inspection. The grains of which it is composed have little or no cohesion, possess a brilliant aspect, similar to powdered quartz or sand; are rough to the feel, and speedily sink in water: if boiled in repeated portions of water, they lose some of their colour, but not their brilliancy or specific gravity; they are not soluble to any extent in water, requiring as much as 10,000 times their weight of that fluid at 60° F., for solution. If a small portion of the lithic acid deposit be ignited, it blackens, and is dissipated, without leaving any appreciable residue; if, however, a larger portion be employed, a very striking odour is evolved during the application of heat, similar to that of prussic acid, and a small white ash remains, consisting generally of phosphate of lime. A solution of caustic potass or soda readily dissolves this deposit; and if it have been previously well washed, no ammoniacal odour is evolved. Any of the common acids, even the carbonic, throw it down again from its solution. The most characteristic test of all, however, is the action of nitric acid, diluted with two or three times its bulk of water: upon applying heat, the uric acid is speedily dissolved; and if the fluid be cautiously evaporated to dryness, a pink stain is produced, much heightened in colour by the addition of a little caustic ammonia.

Colouring Matter.

These means will be found amply sufficient to distinguish the lithic acid deposit from any other which may be met with in the urine; and its mode of detection has been somewhat fully entered upon, since in forming a diagnosis it is of great importance that the true nature of the deposit should be understood; for the fact that such a precipitate takes place from the urine, would seem to point out a much more serious derangement of the kidneys, as well as general health, than is indicated, in most cases at least, by the deposition of the non-crystalline lithic deposits. In those painful attacks called gravel, the lithic acid is for the most part the prevailing sediment; whereas the amorphous deposits may occur from simple derangement of the stomach, and in a variety of other morbid conditions, without there being any of those symptoms present which are ordinarily associated under the term gravel. A knowledge, too, that this species of sediment exists, might also induce us to administer alkalies more freely than might be deemed necessary, or even prudent, in those cases where, from the circumstance of the acid principle being combined with a base, it would, from its more ready solubility in the urine, be less likely to form those concretions which, being too large to pass with the urine, become shut up and augmented in the urinary bladder. In the administration of carbonated alkalies for this class of sediments, the effect produced on the urine should be watched, for a too free exhibition of the remedy might have a tendency to diminish the solubility of the lithic acid; for it is found by experiment that a portion of lithic acid which would be dissolved in a diluted solution of carbonate of soda or potass, is positively rendered in great measure insoluble by employing a more concentrated solution of the salt. Thus Wetzlac found that when he employed a solution of carbonate of potass in the proportion of one part of the salt to twelve parts of water, that it did not dissolve a certain portion of lithic acid which was added to it; and although it yielded a portion of its alkali to the acid, and became itself partly bi-carbonate, still the uric acid was not dissolved; when, however, the fluid contained only one half per cent. of alkali, the uric acid was dissolved readily enough. These circumstances would also seem to point out the propriety of employing diluents pretty freely in this colouring matter.

I shall now conclude what I have to say of that subdivision of the first class of deposits, viz. those of a strictly organic nature, by a brief notice of the colouring principle of the urine. Dr. Prout is of opinion that there exists in healthy urine two descriptions of colouring matter, one of which is capable of uniting with and imparting its colour to lithate of ammonia, which the other is not. Be this as it may, it is quite certain that lithic acid and its compounds have a greater affinity for the colouring matter of the urine than any other of the principles contained in that secretion; and, as has been before stated, the uncombined lithic acid deposit is invariably of a darker colour than in those cases in which the acid is associated with a base; but whether the colouring principle in these cases be of a different nature, or only modifications of the same thing, does not appear to be as yet satisfactorily determined;—of course, I here

leave out of the question that peculiar pink colouring matter which is so frequently found in union with the non-crystalline lithic deposits. Water is capable of removing some of the colouring matter from both these classes of deposits, and alcohol to a still greater extent; and by evaporation of either menstruum, the colouring principle is obtained in the form of a reddish-brown extract. I shall not enter in this place upon the consideration of the pink colouring matter so often found combined with certain urinary deposits, since almost all I have to say on that point is already detailed in a series of experiments, instituted in conjunction with my friend Mr. Bird, for the purpose of ascertaining whether it differed from purpurate of ammonia in its behaviour towards re-agents. The result of our experiments led us to conclude that it was a distinct colouring principle, in no respect, saving in colour, identical with that salt. An account of these experiments will be found in the Medical Gazette for July 26, 1834, and in the number for August 23, of the same year.

[To be continued.]

ON THE COMPLICATED FORMS OF VENEREAL DISEASES.

By D. Henry Walne, Esq.

In considering the manner in which "*the forms of venereal diseases are complicated with each other*," it remains to be shewn that primary venereal affections "may occur in persons already the subjects of secondary symptoms, which have originated in other primary affections no longer existing."

The carelessness and irregularity of patients in reference to symptoms which have declined, almost to disappearance, or have totally disappeared for a very short time, and sometimes their penuriousness, or their suspicion of mercenary motive in their professional adviser, induce them to proceed without advice in an imperfect and irregular course of treatment, or prematurely to discontinue all remedies, and so to treasure up the seeds of future ailment in the system.

In consequence of such neglect the constitution continues more or less under the influence of the venereal poison, and symptoms, modified by the previous treatment so as to have very qualified appearances and very uncertain periods of recurrence, are the not uncommon result. That species of practice which was generally resorted to by the adherents of the pseudo-syphilitic doctrines, not unfrequently led to such a result as is here described, and thus furnished the most numerous examples by which the doctrines themselves were ingeniously supported.

So much of mercury and other remedies having been used as would keep the disease in check, without entirely restraining and removing it, after a period of the discontinuance of these the disease puts forth some fresh and irregular, perhaps very slight, appearances; sometimes so questionable as to excite only unpleasant doubts in the mind of the practitioner consulted, at others, sufficiently unequivocal in character.

Under these circumstances the constitutional affection becomes extremely protracted, the primary symptoms in which it has originated being in the meantime very effectually cured. In this state of matters no obstacle is presented by the condition of the person diseased to the re-admission of local poisonous influence of the same kind, namely, syphilitic. Chancres are, I believe, quite as readily produced by the application of fresh infectious matter as if the person were altogether free from disease. They also are as capable of exciting buboes, and there is reason to believe that the fresh poison, if suffered to produce its effects uninterruptedly, finds the constitution still susceptible of its influence. Since no strict analogy seems to exist between syphilis and the acute eruptive diseases which originate in infection and have their limited period of existence, (for syphilis affects the same constitution repeatedly, susceptibility not appearing to be in any degree diminished by ever so many recurrences of that disease), we cannot infer, from observing its phenomena, what may be the laws which regulate its course. The local effects of poison afresh introduced during the short continuance of the constitutional influence of the infection of small-pox and of cow-pox, are much accelerated in progress, so that their advance and disappearance bear a relation to the progress and decline of the affections

induced by a previous infection still acting. It would not be right, however, to infer from this circumstance that a similar acceleration of the local symptoms, produced by a new infection in the person of one already constitutionally infected with syphilis, would take place, although I have sometimes thought that something of the kind occurs. In short, each disease having its own habits, these must be observed with care to obtain accurate information of their character.

On the subject of new local symptoms there is no difficulty in shewing that they may be produced by fresh poison, whilst the effects of a previous venereal infection continue in the constitution, and that they proceed without interruption from that circumstance, if they be not aggravated by it. This being the object of the present paper, I proceed to record an example, which is one of many, and is selected for the purpose of exhibiting, at the same view, an outline of the effects of a kind of treatment regarding which much difference of opinion appears still to obtain amongst British practitioners; my experience of the benefits of which has led me to entertain of it a very favourable opinion.

On one point in particular I am disposed to insist, when alluding to the practice of using argenti nitras as an application to chancres, which is, that so far from occasioning buboes, as some practitioners have supposed, it affords, in my judgment, the most ready and most generally successful means of preventing their occurrence. It contributes even, in some cases very evidently, quite indisputably, after the occurrence of buboes, and when suppuration seems otherwise inevitable, to their resolution, by diminishing the sensitiveness and the pain of the ulcers which have occasioned them.

Let any one observe the circumstances under which buboes connected with chancres ordinarily appear, and let him note the periods at which they begin to shew themselves, dating from that of the first appearance of the chancres from which they arise. He will find that they observe no rule as to time. That they may commence at any moment after the first symptom of chancres, but that they do not necessarily appear at all, though the constitution should become affected. That chancres may exist for weeks without producing them, if themselves in a quiet, somewhat indolent state, and if the patient be little excitable, be of temperate habits, take little exercise, and is not exposed to chill of the skin. Let him consider these circumstances, and others to which his attention will be desired when I recur to the subject of buboes, and he will see how little they depend upon the influence of the poison, taken by itself, as a cause of their origin and various states and characters. Then let him consider the means which are most effectual in causing their dispersion, as well as those which fail to arrest their progress, and he will see reason perhaps to join me in deeming the practice adopted in the following case to be desirable in many.

A gentleman who had long been the subject of secondary syphilis, which, owing in part to his own irregularity, and in part to uncontrollable circumstances, was still imperfectly cured, although all vestige of the chancres in which it had originated had long disappeared, exposed himself to a fresh cause of disease, and two chancres were produced, having the most distinct characters. He was by various obstacles prevented from seeing his medical adviser until a bubo formed, which very rapidly proceeded to a state bordering very narrowly on suppuration. In this condition he consulted me, the bubo being of a bright red colour, exquisitely tender, and softer at the middle of the swelling than elsewhere, pressure there leaving a little indentation. It was painful, hot, and throbbed incessantly. The chancres were also very painful, their margins red and hard, their cavities rather foul. My patient's constitution was naturally good, and seeing that if the irritation of the chancres were not subdued, but a few hours could elapse before the bubo would be in a state of suppuration, an inconvenience which he greatly dreaded, I applied, with the greatest attention, the argenti nitras to every part of the hollow of each of the chancres, freely, and not suffering any point of these ulcers to escape being touched. Rough usage to irritable sores as this might appear, it proved quite otherwise. Two hours sufficed for him to experience that his suffering was much relieved, not only in the soreness and pain of the chancres being less, but in the throbbing and pain of the bubo having diminished.

A spirit and goulard lotion was constantly and drenchingly applied to it, for there were difficulties in the way of leeches. A dose of calomel and opium (gr. v. and gr. i.) was given, and followed by a brisk purgative. After an interval of thirty hours the argent. nitr. was renewed, as were the internal means, and then the repetition of calomel and opium, two grains of the former, and half a grain of the latter, three times a day, commenced. For three days it was uncertain whether the bubo would suppurate or not, though the pain and tenderness were considerably reduced. Then, however, subsidence of the remaining redness, and of the swelling, began to be perceptible. Under the persevering and assiduous use of the lotion, and the regular continuance of the calomel and opium, this steadily proceeded, and the chancres began to appear cleaner. After an interval of about four days the re-application of the argent. nitr. was required, by renewed pain in one of the chancres, and some threatening uneasiness in the groin. Its effects were again strikingly beneficial, relieving both these as soon as the pain caused by its application had subsided. Proceeding with the mercurial, and regulating its quantity so as to sustain a very gentle influence upon the mouth, the symptoms of recent infection steadily and speedily disappeared, whilst the old constitutional ailments, which had been fixed by time, and only partially relieved by interrupted treatment, still required for their cure the use of efficient and somewhat varied remedies.

There is little room for comments on this case, the circumstances are so easily intelligible, and speak so plainly for themselves. The irritability of the primary sores seems to have measured the progress of the bubo, which accordingly was rapid, threatening immediate suppuration. When the tender and painful surface of these sores had been destroyed, the bubo partook of the ease which followed the application of the argenti nitras. It became amenable to the influence of a very simple remedy. The pain in it abated directly, and had entirely subsided before the calomel could act as a mercurial remedy. Uneasiness was renewed, however, when the sores became again painful, but when their sensitive surface had been again destroyed the bubo became once more easy. Its progress then ceased, and its subsidence soon followed, promoted probably by the mercurial; but no mercurial, according to my experience, would have effected it, without the aid of the application to the chancres.

The case is also conclusive as to the susceptibility of patients to re-infection locally, when still under the influence of a previous syphilitic infection.

Bloomsbury Square,
Feb. 1836.

OBSERVATIONS

ON

THE VENEREAL PRACTICE OF BERLIN.

By Alexander Ure, M.D., M.R.C.S.
Late House-Surgeon to the Royal Infirmary at Glasgow.

[Concluded from p. 721.]

While describing the method of Dzondi, I omitted to mention, that should salivation or diarrhœa supervene, the pills are to be discontinued until these have ceased; that, in addition to the pills, the patient takes daily about a pound of decoction of sarsaparilla; that great attention is to be paid to regulate, by suitable means, the cutaneous transpiration; that the cure lasts four weeks; and lastly, that the only local applications allowed are, either dry lint or lint, spread with simple salves, merely with the view of preventing access of air and of maintaining a gentle warmth.

Before quitting this subject, I ought to state, that I was struck with the general healthful appearance of the patients, when I reflected on the excessive doses of sublimate which some of them were taking. On closely interrogating the medical attendants, I was assured that almost the only disagreeable symptom it is known to create, is a sense of uneasiness about the pit of the stomach, which is allayed by an opiate; that ptyalism is by no means a frequent consequence; and that diarrhœa is comparatively rare.

Basil Valentine, of Erfurth, who wrote towards the end of the fifteenth century, was the first who employed the corrosive sublimate internally. He gave as much as four grains for a dose, and in this practice he was followed by

the French surgeons of the time. But the energy with which these powerful doses acted, led prudent persons to consider the preparation as a poison, and to proscribe its use.

Most modern practitioners are agreed that the perchloride is a valuable remedy when we wish to make a sudden impression upon any particular symptom which is gaining ground rapidly; and then it may be administered to the extent of one-eighth of a grain, twice or thrice in the day*; but it is seldom deemed safe to exceed this quantity, for fear of giving rise to serious irritation of the stomach and bowels, to inflammation of the pleura and lungs, or to violent ptyalism, with its concomitants.

Dzondi's method, of which the chief peculiarity seems to be a connecting of the old and exploded plan with that now in practice, by supplying the intermediate links, it is certain gives rise to little or no inconvenience.

Whether it is from thus exhibiting the remedy in graduated doses that the system becomes habituated to its action, and so enabled to exert a sort of tolerance of its baneful influence when larger quantities come to be forced upon it, as is known to be pre-eminently the case with regard to tartar emetic, is at best highly problematical. Yet Lagneau, one of the principal French authorities on the subject, mentions his having been informed by a learned Athenian, that the fakirs, or Mussulman priests, in the spirit of mortification, abstain from opium eating, but substitute for the favourite oriental indulgence the sublimate, which they take at first in small doses, and gradually augment the quantity till they come to experience an agreeable excitement or exhilaration†.

A more likely way of accounting for the negative effects of the larger doses of sublimate appears to be this:—As it is a soluble salt, taken into the stomach immediately following the principal repast of the day, a variable portion of it will in no long time, by the action of the animal principles, in the secretions and accidental contents of the stomach, be converted into a comparatively innocuous compound: thus all risk of corrosion of that organ is avoided. Should any excess remain unmodified, as is soon evinced by the griping and other uneasy feelings of which the patient complains, it will in its turn be deprived of all deleterious properties by the administration of the opiate always provided against such accidents; for it is ascertained that the operation of corrosive sublimate on the animal body is almost entirely prevented by opium*.

In cases of violent salivation from mercury, Professor Kluge has found iodine to be the most powerful means of checking it. The dose is a grain of iodine in solution four times a day, increased to two grains†.

The presence of the menses, the period of utero-gestation ‡, and the supervention of hemoptoë, contra-indicate, as a matter of course, the employment of internal remedies, and suspend for a time the cure.

The treatment of gonorrhœa, balanitis, phymosis, and paraphymosis, is simple, and offers nothing deserving of notice; the former affection, as above stated, becoming seldom the subject of treatment in the hospital.

Bubo.—The inflammation of the congeries of inguinal glands, so designated, is frequently met with in females in whom no other morbid trace can be detected than a leucorrhœal discharge. Such females are for the most part of an irritable habit of body, and the discharge is of a very acrid nature. In men the buboes consequent on syphilitic sores are found to be as frequent after the internal use of mercury, as where that medicine has not been given.

As to the local management, the practice here is to open buboes very early, and that by rather a small incision. The advantage obtained is, that the smaller wound, which is much sooner filled up, heals with a cicatrice. After the incision is made, chamomile fomentations are laid on the part, and continued until the granulations have reached the level of the skin. Should,

* Bacot, Treatise on Syphilis, London, 1829, p. 257.

† Lagneau, de la Maladie Venérienne, p. 170. Paris, 1812.

* See Pettenkoffer's explanation given in Dr. Christison's work on Poisons, p. 398; also Caillot, in Journ. de Pharmacie, tom. xix. p. 221.

† Med. Zeit. f. Heilk. in Preussen, No. v. 1833.

‡ Dr. Kluge has found that syphilitic pregnant women, who had been treated with calomel, bore enfeebled and miserable children, which die soon after birth; whilst the treatment with corrosive sublimate is not attended with these bad consequences.

as sometimes happens, their continued application produce a state of local relaxation, indicated by the edges of the sore becoming pale and glazed, the granulations flabby, and the pus thin in quality and defective in quantity, then pledgets, dipped in vin. camphor., are substituted in their stead, which usually answer very well in restoring the requisite tone to the part. Lastly, the healing of the sore is accelerated, after a time, by drawing lightly along the centre a pencil of lunar caustic; over the little film thus formed a narrow slip of lint is laid, and allowed to remain until the following visit, when the application is renewed. A sort of thickish sebaceous deposit is apt to concrete around the margins of the newly-formed cuticle, which, by preventing the free access of the air, tends to keep the latter tender, and renders it liable to re-ulcerate on exposure to the slightest irritation; this, therefore, is always carefully removed, either with the blunt end of a probe, or, what answers exceedingly well, a pair of common dissecting forceps.

Where the granulations are luxuriant, the following simple mode of repressing them is sometimes pursued:—A round flattened smooth stone, weighing two or three pounds, is selected, which, being laid on the sore, is allowed to remain thus superimposed for some hours, or as long as it can be conveniently borne. This procedure is continued for some days consecutively. In many instances it is found to serve the desired end, and advance the healing of the sore; in others, again, by bruising the granulations, and irritating the surface, it does rather harm than good, and requires to be discontinued *.

* I perceive that a nearly similar practice has been inculcated by an Italian author. He recommends a large and solid compress to be placed on the bubo, and fixed by means of an appropriate bandage. This compress is to be continually soaked with some astringent liquid, and the patient confined to bed. By this treatment buboes, it is alleged, are cured in forty-eight hours, if suppuration be not present; neither is the presence of pus considered as a contraindication to its use.—Gazetta Ecclettica di Verona, Marzo, 1834.

ON THE PATHOLOGY

OF

RAMOLLISSEMENT OF THE BRAIN.

By John Gay, M.R.C.S.,

Late Clinical Clerk at St. Bartholomew's Hospital.

Of all our bodily organs, probably not one has offered more perplexities to the pathologist, in his search for the connexion which exists betwixt symptoms and disorganization, than the brain; so that after all the investigation which has been bestowed upon it hitherto, it must frequently be observed, that the greatest discrepancy appears to prevail between the indications of disorder which were observed during life, and the character of the appearances presented on post-mortem examination. Rapidly fatal results often supervene upon merely functional disturbance, without organic changes; and as often, extensive disorganization is met with, without the precedence of that long train of serious indications which we might have expected. In the pathological study of any organ, its conformation, relative situation, physical properties, intimate arrangement, and vital functions, ought to have especial attention; and I conceive that nothing has tended more eminently to mystify the nature and processes of diseased action, than its isolated consideration.

If the following observations on Ramollissement, gathered from an observation of cases of cerebral disorder in St. Bartholomew's Hospital, should not receive full sanction, yet should lead to a renewed and more successful research on so interesting a subject, the writer will feel amply repaid and honoured, his motive being rather the hope of eliciting truth than of establishing hypothesis.

On examining portions of brain in different states of ramollissement, we cannot but conclude the process to consist in a gradual degeneration of the affected portion from its primitive condition of firmness to a completely diffluent mass. In the first deviation from the healthy state, the particles appear to lose the force of cohesion which before maintained them in close apposition, and become in themselves soft, and

ultimately fluid. During this morbid change all appearances of arrangement gradually disappear, and the various structures essential to the maintenance of its vitality, nutrition, and function, become involved in the same destructive process, and are at length completely broken down and lost. A further stage (according to writers on the subject) consists in the entire separation of the softened mass from the surrounding living structure,—the formation of an excavation lined sometimes by a thin membrane,—the absorption of the fluid and reparation: of these last processes, however, I cannot speak from personal observation. This change, as will afterwards be shown, is consequent on either of two very opposite conditions of the cerebral vascular system, viz. an increase or diminution of its activity. The first, or inflammatory, may be recognized by the colour, of which there is a variety of shades, and from its limited extent. The colours may be reduced to two, "red" and "yellow," and arise from the admixture of pus, serum, or blood, in different quantities and relations, with the softened tissue of the brain itself. Sometimes, however, the redness is confined to the surrounding parts, where, on making an incision, a large quantity of bloody dots may be noticed, as if in bundles; the vascular tubes themselves appearing to have been obliterated by a process which nature has adopted to secure them against hæmorrhage. The limits are never very great, as inflammatory action set up in the cerebral substance has no tendency to spread itself far. The character of ramollissement arising from diminution in the activity of the vessels, is known by its involving a larger portion of brain, generally an hemisphere, and by its retaining its pale colour; or, as is sometimes the case, the distribution of blood being unequal, a mottled appearance is presented in the affected portion, without any evidence of an increased quantity of blood. This is an imperfect outline of the physical appearances of the several varieties of softened brain. The distinctions which have been observed have a more specific reference to another section of this paper. Taken collectively, they seem to give sanction to the following suggestions, viz. that wherever ramollissement of the brain occurs, whatever be the precise shade or situation, there the integrity of the mass is so destroyed, that its capacity for the exercise of its various functions has ceased; and being no longer co-operative with the surrounding healthy structure in the evolution of nervous energy, it becomes exactly equivalent to a portion of extraneous matter, altering the general configuration, and retarding the general actions of the cerebral system; in short, it has ceased to live. Is it probable, nay, is it possible, that such a broken-down disorganized mass can possess the necessary means of preserving its vitality? Can blood circulate,—animal heat be generated, and the nervous functions be evolved,—from a turbid fluid possessing in many instances not even the trace of blood-vessels, which are essentially necessary to the production of such results in any organ?

Here, then, are complete evidences of loss of vitality in a part of an organ analogous to mortification in other parts. The evidences of death admit of no variety, although they may be modified by external agencies. Every other morbid change in the body must be connected with active organization, although its action and results are of an abnormal nature. Another evidence may be adduced from the fact, that if a brain be allowed to remain within the skull for the space of two or three days after death, the same softened appearances will always be found. Dr. Baillie notices this in his Morbid Anatomy, where he describes the brain as assuming, after death, the character of a "pultaceous mass." But there are differences betwixt it and mortification in other parts. How may they be reconciled? They consist of a want of fœtor and the usual dark colour; the latter circumstance depends on the peculiarity of the cerebral tissue, the former on its entire removal from the agency of the atmosphere. And here I would make use of the distinction which this want of fœtor establishes between mere death of parts, and such death connected with decomposition; the first being the condition of a softened brain, the latter of a mortified extremity.

Ramollissement has been considered, from its physical signs, to be death, resulting from inflammatory action or diminished supply of blood. I shall now attempt to explain the frequency of its occurrence, and show what are the causes which most powerfully induce such a change of structure.

Excessive diminished vascular activity in any organ or texture, are well known to be capable of producing mortification; but this is comparatively seldom the case, except in the brain, where they are frequently followed by the death of the part. These two states just mentioned are the immediate causes, however induced. What, then, are the causes which predispose to such a termination, and what are their modes of operation? The following may be regarded as of most importance, viz. age; sex (the male being most liable); plethoric state of the body, especially when occurring in those whose habits are sedentary and gross; mental disquietude; suppression of accustomed evacuations; diseases of other organs, especially the heart, stomach, lungs, and kidney; and the vascular arrangement of the organ itself. Of these the last-mentioned appears to me to demand most attention, since it affords the most satisfactory explanation of the phenomenon in question. One of the most prominent peculiarities of the brain, is the security which is given it by its coverings, especially the skull. No other organ is so powerfully protected against external agencies; and this protection appears to me to hold a powerful relation to the arrangement of its vascular system. It has been said, that of all the organs in the body, the brain receives the largest supply of blood; and this may be true, taking collectively the brain and its membranous envelopes. But different parts of the organ itself have very differet degrees of vascularity. Beclard states, whilst speaking of the capillaries, that "they are more abundant in the envelopes of the brain and its cineritious substance, than in the medullary;" that "the cineritious matter of the encephalon and the nervous ganglia possesses a great number of capillaries of all sizes. The white matter, on the contrary, whether of the brain or nerves, possesses only very small capillary vessels, and in a less proportion." And in another part, speaking of relative degrees of vascularity, he classes the pia mater next to the lungs, which are the most vascular organs in the body; the cineritious matter next to the glands, and above the muscles; but the medullary between the adipose tissue and bones. Thus we see that the medullary part of the brain, where ramollissement most frequently occurs, is supplied with but a very inconsiderable quantity of the circulating fluid.

In an organized and living body, there is a constant struggle against all the powers that tend to interrupt its function or annihilate its existence. Vitality appears to depend for its maintenance, in any organ or tissue, on the reciprocal action of circulation and innervation; and for its intensity, on the degree in the scale of organization to which such organ or tissue belong: consequently, where organization is most perfect, and independent vitality most intense, there the inroads and effects of diseased action are most successfully opposed. In order to obtain a knowledge of the degree of vital energy in any part, physiologists have referred to the quantity of blood which circulates in that part, with the healthy performance of its functions, as a criterion; hence the reason why, after the removal of a limb, the person who was before debilitated and unhealthy, becomes robust and vigorous; and why parts are liable to mortify where circulation is impaired. Now the medullary part of the brain has been said to possess but a very small proportion of circulating blood, consequently its independent vitality is feeble, and the cranium appears, with its other uses, to supply the place of its inherent death-resisting principle, by defending it from numerous agencies to which other parts, from their condition of organism, may be less hazardously exposed. To this circumstance, therefore, I refer, as the reason why, in the brain, and especially in its medullary substance, a loss of vitality so frequently occurs, and why comparatively trivial degrees of vascular excitement are capable of producing it. Its vitality is languid, consequently deranged action has more power; and if the degree of disturbance is superior to the degree of resistance, and all efforts to control this disturbance unavailing, death of the part must inevitably ensue. Of the other predisposing causes, habits and age appear to have the most important co-operative influence. Every one must be acquainted with the limited power possessed by the victims to intemperance, and the enfeebled by age, to resist maladies in themselves trivial; and if other parts of the system suffer from such circumstances, how much more must a part which, in the vigour

of youth and constitutional strength, is ill able to withstand the withering breath of disease.

The immediate causes are those which are immediately capable of setting up inflammation, whether acute or chronic; or of causing a diminished flow of blood to the brain, or of causing a disturbance in the balance of its circulation: as blows on the head, tumors, extravasation, scrofulous deposits, ossification of arterial or venous tubes, pressure on their parietes, and large general depletion. I have thus hastily advanced one step further, and have shown that the natural state of the organization of the brain predisposes it to suffer greatly from any noxious disturbances; hence it is ill able to bear the excitement of inflammation; and when that process is known to have commenced, there is every reason to fear that its termination will be in the death of the part. In proportion to the degree of the inflammation will be the probability of such a result; and this probability will be much strengthened by its taking place in an individual whose constitution has been debilitated by age or intemperance. Also whilst increased activity of the cerebral circulation may thus be influenced in its termination by softening, so diminished vascular activity will lead to the same results more speedily and effectually, under the fostering influences of the two last-mentioned predisposing causes.

It is my design now to point out the symptoms of ramollissement. In the cases I have observed (regarding them from the commencement of indisposition to its close), the symptoms have been divided into two distinct classes, differing from each other fundamentally. A fear of intruding too much on the pages of the Gazette has induced me to waive the detail of cases; and I do so the more readily, as a large number of instances are recorded in the works of Abercrombie, Bright, and Rostan, which concur with my own experience.

The first class of symptoms includes a variety which are produced by inflammation itself, chronic or acute, if the disease be idiopathic; or by the specific affection of the brain, which produces the inflammation. These are obviously not of a uniform character, if we recur to the list of exciting causes; and the duration of this stage is longer or shorter, according to the degree of disturbance and other concomitant circumstances. These symptoms cannot be relied on for the production of the second series, which are ever and alone the indications of the existence of ramollissement; the probability that they will induce it being dependent (as has been before stated) on the activity of the disturbance, the age of the individual, and the condition of constitutional power. Let it be understood that the aid of many other circumstances may be called to assist in prognosticating this affection; but these appear to be the most important.

The second series are more decisive, and generally exhibit the same indications in each individual case. Their duration is protracted, and they consist of some of the following, or all combined—viz. excessive debility, delirium, coma, convulsions, stertor, and paralysis; and may be comprehended under the evidences of prostration of strength and cerebral compression. Such symptoms do not generally exist beyond a few days, being interrupted by the hand of death; although I have seen them continue twelve and even fourteen days; their aggravation daily and hourly increasing. This must be the case if ramollissement be a death of the part involved; for as soon as the mass becomes lifeless, in every instance that mass must have the same effect on the surrounding sound structure; and being to a very small degree under the influence of the healthy parts, the length of time for the succeeding changes to accrue must, in every case, be nearly equal, and the symptoms must become more and more aggravated. Dr. Bright, in his excellent Reports on the Brain, gives sanction to this view of the nature of the symptoms. Dr. B. says they are generally those of pressure, with convulsion, but goes on to state, "that neither these nor the previous occurrence of pain in the parts afterwards affected with paralysis, can be considered as essential to the softening of the brain, which is a state of the organ apparently depending on various causes;" and after citing a variety of these causes, Dr. B. says, "that partial softening owes its origin to a variety of causes, and to a considerable extent of morbid action; and we may therefore conclude, that however much the symptoms of the confirmed disorganization may approach in all cases, and generally become proofs of pressure

and interrupted nervous susceptibility and action, yet that the symptoms which mark its approach and progress will vary greatly."

If we are to attach any degree of importance to the symptomatic evidences of disease (and I conceive the study of morbid anatomy to be to its full extent useful only when combined with a minute observance of symptoms); and if the most satisfactory conclusions drawn from those two sources are those in which the least disagreement prevails, I submit that the facts already stated give some degree of plausibility to the theory now advanced; and that, in addition to what I have already suggested with regard to the nature of ramollissement, the character of the symptoms, their disagreement at one period, and their uniformity at a subsequent stage, tend to confirm these suggestions. In some cases I have observed the first order of symptoms strongly marked, indicating a chronic affection of the brain, or some part of it; but the terminating series were not indicative of the confirmed disorganization in question, and no such change has been found. But I must advance towards a conclusion.

It has been asserted by some writers, amongst whom is Dr. Sims, that this disease is reparable, and that a reparation is frequently effected. In a paper lately published in the Royal Medico-Chirurgical Transactions, Dr. S. attempts to prove such a position, and believes the facts which he brings forward "sufficient to set the question at rest, on the solid basis of pathological anatomy." After carefully reading this production (with all due deference to the talents of Dr. Sims, and disavowing any other spirit but that of defensive objection), I beg leave to submit, that although the basis of pathological anatomy is undeniably of the utmost importance, still, without the support of other auxiliaries, and especially symptomatology, that basis cannot be sufficiently firm and extensive for such a structure as Dr. Sims has attempted to raise upon it. Dr. S. confesses, that "perhaps he has to assume in this argument that ramollissement is by some means or other stopped in its progress;" and he proceeds—"if this be granted, the preceding detail of morbid appearances will readily fill up the several steps of the process of cure." Thus, on the mere appearances presented in various brains, which have been the seat of some disturbance, Dr. S. contends for the reparation of a morbid change, which he has not shewn one particle of reason for supposing to have ever existed. Surely such a condition of such an organ cannot take place without some positive signs; but in some of the cases in the paper of this talented author, where it is said to have existed, no evidences of cerebral disturbance are mentioned to have ever been observed. If the diagnostic marks of ramollissement had been cleared up, the theory alluded to would, I imagine, be placed on a much more satisfactory and substantial foundation.

As to its reparation, it appears to me that, in all morbid changes, nature provides most beautifully for their cure. This is strikingly noticed in the more obvious diseases, such as in some kinds of diseased joints, where nature appears to provide for ankylosis of the joint by a removal of the cartilage, without which the process of ossification could not be established, and the union of the two ends of bone confirmed. So in ramollissement of the brain, the fluidity of the mass which gradually obtains seems to indicate a provision for its more easy and speedy absorption; but whether such absorption has been ever permitted, by a continuance of life to a sufficient length, has never, I believe, been satisfactorily determined, and, if the theory now advanced be correct, is rendered highly improbable.

One word to the supporters of the theory that ramollissement is a disease *sui generis*, amongst whom we find many illustrious pathologists. Disease *sui generis* implies morbid action. Now morbid action has no character in common with loss of vitality. Parts undergoing such alteration possess organization and life; but the formative action is disturbed, and diseased structure is the result. But in ramollissement of the brain, all traces of circulating vessels, and other evidences of active organization, are generally entirely gone; and if the vessels themselves should not have disappeared, they present a ragged appearance, as if undergoing the changes of mortality.

Again, diseased structures may be generally injected, but no injection can be made to enter into the vessels of a softened mass of cerebral matter; it is, in fact, dead. Many of the old writers,

as Bonetus, Morgagni, in later days Drs. Baillie and Rostan, favour the theory of its being analogous to mortification, although the difference in the symptoms, and the respective duration which mark the primary affection and the terminating disorganization, have never, I believe, been pointed out.

I must now conclude these imperfect remarks on a difficult subject; they might have been much extended, but are sufficiently detailed to shew the character of the theory, and its claims to plausibility. My motive has been the hope of leading to renewed inquiries after truth on this interesting subject, deeply convinced that sophistry, however ingenious and attractive it might be made to appear, is still in every case dangerous and reprehensible. Its semblance in the garb of theory is frequently insinuating, whilst in reality it is but equivalent to the varnish of a falsehood, or the seductive blaze of the ignis fatuus. If these considerations are found to be erroneous, the writer will be the first in their condemnation; if, on the other hand, they gain consent—"si quid habent veri præsagia, vivant."

9, Artillery Place, Finsbury Square,
Feb. 8, 1836.

ON DOSES OF MEDICINE;

WITH SOME REMARKS ON THE

HOMŒOPATHIC SYSTEM.

By Dr. Leonard Stewart.

Read at the Westminster Medical Society.

To prescribe a dose of medicine is considered by the unsophisticated as the most obvious and simple part of our professional duties; and we continually see those who shrink from all investigation into the nature of disease, step forward as the most confident in proclaiming the virtues of remedies, and putting in practice what *they* consider as nine-tenths of the healing art, and what *we* call polypharmacy!

It requires not much experience nor much reflection, however, to convince the faithful practitioner of the natural predominance of scientific pathology over therapeutics. In important cases, a thousand circumstances are to be borne in mind, a thousand contingencies glanced at, in the rapid manner which habit makes familiar, before matters are at all prepared for a fair experiment: for I consider every first administration of a dose of medicine an experiment, rendered, however, more or less clear, rational, and satisfactory, by studying before-hand the indications for its employment, and by striking out, as much as we can, what may obscure or obstruct its effect.

Nothing tends so much to rectify our notions about the power of doses of medicine as the study of pathological physiology, or, in other words, of the natural history of diseases. This at once presents us with three *practical* divisions into, 1st, diseases which cure themselves; 2dly, diseases which are cured or influenced by treatment; 3dly, incurable diseases. I do not pretend to show the proportion in which they all stand. I am quite aware that these three classes are not distinctly separated—that very many begin with similar phenomena—that we continually see cases which pass from one stage into another. Thus a catarrh, or a cut finger, may become a serious, or even fatal, disease. So also an ague or an inflammation, though at first showing the usual and *normal* symptoms, will at times gain or lose its intensity; there being occasionally an *under-current* at work, which embarrasses our judgment, and sweeps the patient on to health, or dissolution: and lastly, the records of medicine are not wanting in examples of the most extraordinary recoveries from maladies which, by common consent, have been considered hopeless. Still, although the only sure appreciation of the fatality of diseases is retrospective, the accumulated experience of the profession has elucidated the relative strength of nature and of art, of diseases and doses of medicine, much more accurately than the uninitiated are ready to allow; and it may safely be asserted, that for one instance talked of, which has puzzled the skilful practitioner, a hundred, a thousand, are forgotten, in which he has been sufficiently close in his judgment.

While, then, any person of good sense and good faith applies *only* the subordinate or *ancillary* parts of our art to a vast share of cases, he reserves the employment of active or *heroic* remedies (as they are called) for those conditions which are important but not incurable—which would be fatal, or become chronic, if left alone—in which,

after all has been arranged that prophylactic, dietetic, palliative, and chirurgical medicine can do, there yet remains to be done *something more*

This "something more" is, with many people, a very simple matter. The plan of universal medicines is the most obvious, and at once recommends itself as abridging or superseding all those tedious intricate occupations and exercises of the faculties called the medical sciences; the whole cumbrous details of which are exchanged for an easy and agreeable routine!

What a pity, then, that a method so admirably fitted for getting rid of the Doctor should not have equal potency with reference to the disease: but perhaps it is too much to expect a double effect from any attempt so single and so *simple!*

I would be understood as speaking without "respect of persons," and as condemning at once the blue-pill plan, the Morison-pill plan, the bread-pill plan, or any other undiscriminating plan, which pretends to reduce to uniformity what is in its nature infinitely varied and complicated. I consider a due variety of remedial measures (under the personal direction of the medical man) as the distinguishing mark of rational practice.

But we find, that even after allowing a choice of means, there have been very opposite ends proposed in their employment.

The contra-stimulant and Homœopathic systems stand remarkably contrasted; not only as to their increasing and diminishing the dose of medicine, but also from there being, in the first plan, very little regard paid to the phenomena and symptoms produced by the remedies; in the latter, the affectation of a great deal more than usual. The disciple of Rasori brings his powerful remedy, and lets it grapple with the antagonist disease, and conquer it in a close and mysterious embrace; there being no physiological effects, and little inquiry into details. It would, however, require a good deal of time, and would yield no new information, to dwell upon the very extraordinary statements of the contra-stimulant school. I need not remind you of the comparative neglect into which the doctrine has fallen; indeed I do not know of much that has been retained, besides the administration of emetic tartar in pneumonia; and here it is not used in the large doses once talked of.

Homœopathy (as we are made acquainted with it) comprehends not only the imitation of disease, but the diminution of the dose of medicine. With regard to this ingenious supplanting of disease, as recommended by Hahneman, I cannot help thinking that (coupling with this design the extremely small and microscopic means by which it is attempted) there generally exists a delusion; the copy existing only in the mind's eye, while the reality is going on uninfluenced by the phantom of physic which is to frighten it away. We have heard of a figure of real flesh and blood passing for a very good portrait; and so it may be with these factitious pseudo-maladies. Looking, indeed, at the liberal allowance of time which is made (often from a week to a month between the doses), and calculating the comparative and relative agency of the elements of a Homœopathic case, I conclude that it is not the small dose of *medicine*, but rather the large dose of *time*, which works the miracle. In this way we can account for the long-continued operation assigned to these very minute doses, which otherwise would be one of the most staggering parts of the scheme. This alliance between the dwarf and the giant may make the last day of the week, or month, as energetic as the first.

I need not tell *you* that, as far as the mere principle of homœopathy, or the plan of treating *similia similibus*, goes, there are many examples of this which are not new. Many have cured diarrhœa with purgatives, scalds and burns with stimulants and rubefacients, gonorrhœa with irritants—nay, one has heard of sailors cutting short an old gleet by contracting a fresh clap, which is in *practice* strictly homœopathic, to say nothing of their *principles*. But this is surely a very different thing from singling out a part or individual symptom of a disease, and attacking it exclusively by an infinitely subdivided dose of medicine. You will find, in one of the Italian books on Homœopathy (under the head Vertigo), a distinction made of six different modes of falling down, with six or eight separate remedies for each kind of fall, besides other drugs for all the other symptoms! I think, then, I am justified, not only in questioning the propriety of encouraging, in all cases,

rather than checking, the natural symptoms of disease, but also in doubting the mere facts; in denying that the phenomena which are said to be produced by the homœopathic doses are in reality attributable to them; indeed, in some cases, in absolutely denying their existence.

Placing this criticism, then, merely on the ground of the existence or non-existence of the facts, I care little about the name of quackery, which is often mixed up with this and other doctrines. We have, I think, a simple way of deciding this point: by asking—is any part of this or that system concealed, or is it laid before the profession in a fair and manly way, open to inquiry and experiment? The essence of quackery I consider to be mystery and concealment: whatever else of error, enthusiasm, and imprudence, mixes up with these questions, is a matter of much less consequence, because all this soon rectifies itself.

We thus fall back upon the more sober quiet method of employing ordinary doses of medicine, upon the plain ground of analogous circumstances, and experience in former cases of their remedial power. Without generalizing too much, or taking aim at each symptom,—without too close attention to those elective affinities, if I may so speak, which determine certain local, specific, physiological, and pharmacological phenomena upon the exhibition of most drugs,—we regard their general curative agency. And while attentively watching and controlling their operation, do not forget that the whole affair is one of fact and observation, and not capable of systematic minuteness and exactitude.

That many clever fine-drawn transcendental systems may be indirectly useful, by superseding blind perseverance in gross mechanical mischievous practice, I mean not for a moment to deny, but I am now reviewing the true principles, and not the mere expediency of the plan, I have mentioned.

I shall not now be expected to go into a detail of common-place cases which have been treated in the usual way, nor to deduce the well-known doses of mercury, opium, bark, &c., which are laid down in the familiar manuals.

The medical world seems, in the present day, to follow, with regard to all systems, the practice of Frederick of Prussia with the philosophers whom he invited to his court. His boast was, that he knew how to squeeze the orange, and throw away the pulp I do not know a better system-squeezer than a medical society; but we cannot always obtain much juice!

But notwithstanding I have here declared the personal superintendence of the medical man to be of more importance than any thing else, and have considered the virtue of the healing art to consist, not in the doses of medicine alone, but in the dexterity with which they are used, I am very willing to be told that I am in the wrong. I shall be much edified by the discovery of any *short cut* to remedial skill, which will save me the trouble and responsibility of attending to diagnosis and prognosis.

I am quite ready to employ the largest doses or the smallest, to go to Brobdignag or Lilliput, or to give no physic at all, if I can be convinced that there is good reason for so doing.

I am quite sure that the suffrages of the rising generation would be secured by diminishing or suspending the dose; and as for those "children of a larger growth" who are always craving for doses of medicine, while time only is required, I am willing to take lessons in

> "—— the art of amusing the patient,
> While nature cures the disease."

ON THE TREATMENT OF HYDROCELE BY SETON.

To the Editor of the Medical Gazette.

Sir,

In your Gazette of the 30th of last month, there is related in the analysis of the "St. Thomas's Hospital Reports," some observations by Mr. Green, surgeon to that hospital, on the treatment of hydrocele by seton; and as it seems to imply a claim to originality in the mode of management, I beg leave to request you will be kind enough to make room in your next number for the following extract from my book on Hydrocele, p. 20, &c., published in 1825, by T. and G. Underwood, Fleet-street.—I am, sir,

Your obedient servant,

James Holbrook, M.D.

Crickhowell, Breconshire,
14th Feb. 1836.

"As I could not learn from the history of the cases treated by seton, that in any instance suppuration had occurred within the sac, but that the cure was performed on the common principle of exciting adhesive inflammation, it appeared to me as quite unnecessary that the seton should remain in for twelve or fourteen days, as it not only rendered the operation much more tedious, but also increased the risk of suppurations and abscesses taking place in the cellular substance of the scrotum. The only object, therefore, to be derived from the seton, was the producing of a certain degree of inflammation; and as that object appeared in most cases to be obtained in about three days, or *sometimes sooner*, I was of opinion the seton ought then to be withdrawn. But to this there was the objection, that it frequently contracted some adhesion to the tunica albuginea, which rendered the early withdrawing of it painful. In order to obviate this objection, it occurred to me that the introduction of a smaller seton would have that effect, and at the same time be capable of exciting a sufficient degree of inflammation; and in this opinion I was strengthened from learning that Mr. Pott had, in the latter part of his life, used but a very small seton.

"Bearing these circumstances in mind, I determined the first opportunity that might occur to operate on those principles, which, although founded on the operation by seton, would in many respects be essentially different, and the troublesome and complicated mode of int oducing it by means of a second and longer canula, passed through the canula of the trocar, together with the long needle having a moveable point, as recommended by Mr. Pott, be rendered unnecessary. A case, fortunately, soon presented itself, and I operated in the following manner:—

"Having discharged the contents of the swelling by means of a common trocar, I grasped the scrotum, together with the loose tunica vaginalis, with my fingers, close to the testicle. This being done, and an assistant holding one side of the skin, in the same manner as in making a common seton, I at once passed a straight suture needle, armed with a common ligature, through the skin, the scrotum, and tunica vaginalis, &c.

"On the following day the scrotum began to swell and inflame, and the testicle was enlarged and painful.

"On the 4th the thread was withdrawn.

"The cure was accomplished in about three weeks."

Page 30:—"Should any difficulty be apprehended in being able to pinch up the tunica vaginalis with the scrotum, in order that the needle may pass properly through the sac, a probe may be introduced through the canula into the sac, which can be used as a guide in pinching up any part of the tunica vaginalis that may be considered necessary."

Page 29:—"In children I have always found a single thread of silk sufficient."

CASE OF HYDROPHOBIA;

WITH THE POST-MORTEM APPEARANCES.

To the Editor of the Medical Gazette.

SIR,

SHOULD you deem the following case worthy of notice, I shall feel obliged by your inserting it in your valuable periodical.—I am, sir,

Your obedient servant,

FREDERICK PRITCHARD.

57, Dorset-Street, Salisbury Square,
Feb. 12, 1836.

In the latter part of October last, Joseph Robbins, a robust healthy young man, æt. 20, residing at Shottery, near Stratford-upon-Avon, Warwickshire, was bitten on the thumb by a strange dog. It appears that the man was in a neighbour's house, when the dog entered; and upon his attempting to secure it he was bitten in three places. The dog escaped, and from subsequent inquiry there is no doubt but that it was in a rabid state.

Robbins took no notice of the wounds, but followed his usual occupation, which was that of a boatman. In the course of a week the parts cicatrized, and he remained perfectly well until about 7 o'clock on the morning of Tuesday, the 12th of January, being a period of ten weeks from the time he was bitten. In the afternoon he walked from Stratford to Shottery, a distance of one mile, and on his road he called upon his sis-

ter. He complained to her of great indisposition, and requested that he might have some food. She wished him first to wash his face and hands; but he replied, "*that the wet cloth would choke him.*"

Soon after this my brother, a medical practitioner at Stratford, was requested to visit him, which was about five o'clock in the evening. He found him oppressed with anxiety, and complaining of stiffness and constriction about the throat and chest. The pulse and general temperature of the skin did not at that time vary much from their natural state. The respiration was hurried, and interrupted by sighs. My brother observing these symptoms, offered him some tea to drink; but the moment the fluid came in contact with his lips, it brought on a violent convulsive paroxysm, which seemed almost to threaten suffocation, and he dashed the cup from him.

The patient, on being questioned whether he had been bitten by a dog, answered in the affirmative, but did not betray any alarm. There was no uneasiness or stiffness about the thumb, and the cicatrices were of a pale colour. My brother being satisfied as to the nature of the disease, sent to inform me of it; and I, together with two medical friends (who were at Stratford at that time), met him there shortly afterwards. We agreed to bleed the patient, and to administer large doses of opium combined with calomel, and to employ the mercurial inunction; but the parents, as well as the man, obstinately refused to allow us to resort to any means for his relief, and we were in consequence obliged to leave him.

Early on Wednesday morning we again visited him, in conjunction with my father, and found all the symptoms aggravated in the greatest degree. He had dressed himself, and was down stairs walking to and fro. There was an appearance of horror and despair in his countenance beyond description; his eyes were wild and glassy; his mouth was filled with a viscid saliva, which he was perpetually endeavouring to spit away from him in every direction, in the most impatient manner. His pulse was quick, and his aversion to fluids as well as solids was greatly increased; even the name of either would cause great disturbance; also the sound of any fluid being poured into a vessel. The simple agitation of the air by the opening of the door, or even the breath of a person, would bring on a paroxysm of general convulsions. We placed him in the erect position, and bled him largely, hoping to make some impression upon the system; and also rubbed a considerable quantity of the unguentum hydrarg. fort. along his spine, and upon his arms.

At eleven A.M. Dr. Thomson, of Stratford, saw him, and suggested that a tobacco enema should be administered; but the moment he heard the gurgling noise in its passage up the rectum, he darted from us. Dr. Conolly, of Warwick, also visited him in the afternoon; but the patient's sufferings at that time were so great, that he would not permit us to approach him; and in consequence of his escaping into the village, it was found necessary to confine him with a strait waistcoat; after which the extract of belladonna was applied to the external part of the throat, but without any beneficial effect. The paroxysms continued unabated until one o'clock on Thursday morning, when he appeared worn out; and half an hour afterwards he expired calmly, at the distance of forty-two hours and a half from the time of his feeling unwell. The patient never lost his senses throughout the attack, but talked of the certainty of dying, and wished to see all his friends; but he did not allude in any manner either to the dog or to the part which had been bitten.

AUTOPSY.—*Cavity of the Cranium.* Upon removing the calvarium, and examining the sinuses of the brain, we found that they were gorged with dark-coloured blood. There was effusion of serous fluid between the arachnoid membrane and pia mater, and also an effusion of about an ounce of fluid in the lateral ventricles. The choroid plexus in each lateral ventricle was of a dark colour. The substance of the brain was natural in most parts, excepting the medulla oblongata, which was more injected than usual. Upon exposing the theca vertebralis, it was found highly vascular and injected; and also the pia mater, but not to so great an extent. The medulla spinalis was in a very softened state throughout its whole course.

Throat, chest, and abdomen.—The mucous membrane lining the larynx, trachea, and bronchiæ, presented a dark congested appearance, resembling black jelly. The substance of the lungs was very much congested. There was no vascularity about the pericardium, neither was there more fluid than usual. The external part of the heart looked paler, and felt softer, than natural; but the lining membrane was congested in the same manner as the lining membrane of the trachea and bronchiæ. The pharynx and superior part of the œsophagus were of a pale colour; but towards the inferior part, near the cardiac orifice of the stomach, patches of extravasated blood were seen, and also the same appearances towards the pyloric orifice. The serous membrane investing the stomach and intestines was in several parts a good deal injected. The liver, spleen, pancreas, and kidneys, were all of them of a leaden hue throughout; and the bladder was collapsed and perfectly empty.

PREMATURE VACCINATION.

To the Editor of the Medical Gazette.

Sir,

In a former number of your journal I have asserted that no child ought to be vaccinated during the period of lactation, by which I mean during the first nine months; in confirmation of this statement the following report may perhaps serve as an example:—

Mr. Als had three daughters vaccinated, the eldest and youngest before the sixth week, the second not until she was a year and a half old. About three years ago the eldest had the small-pox very severely, was nursed by the second sister, who escaped it. About three weeks back I re-vaccinated the second and youngest. The cow-pox in the youngest ran its natural course; the patient laboured under considerable febrile action from the eighth to the twelfth day. In the second sister it appeared to rise regularly till the third day, and then died away. The eldest is now about thirty, and the youngest about twenty years old.

I lately saw four children who had been inoculated for the small-pox: only one of them had been vaccinated. She had between thirty and forty pustules; the other three were very full. On inquiry, I learned that the child had been vaccinated before it was two months old.—I remain,

Yours most respectfully,
John Grantham.

Old Crayford, Kent,
Feb. 9, 1836.

REMARKS ON COMMON, MODIFIED, & SPECIFIC SENSATION,

PARTICULARLY WITH REFERENCE TO THE SENSE OF TASTE.

To the Editor of the Medical Gazette.

Sir,

From the whole tenor of Mr. Bishop's communication, inserted in the last number of the Medical Gazette, I can only think that we do not rightly understand each other. It appears to me that we are verging upon a controversial discussion without having previously determined the exact sense in which *terms* are to be employed. To avoid (so far as I am concerned) all possibility of mistake, I will define what I have meant, and mean, by "common sensation," a "modification of common sensation," and a "specific sensation;" and the definition which I am about to give will be found, I believe, to accord with what physiologists ordinarily understand by these terms.

Common sensation is that general sensibility pervading the whole of the external surface of the body, and, to some extent, almost the whole of its structure; and is illustrated by the simple notion of *resistance*. The *modifications* of common sensation include the various impressions furnishing the mind with its ideas of the hard, the soft, the rough, the smooth, the painful, the titillatory, and so on. The perception of these different impressions is common to every structure possessing common sensation; and when, from any cause whatever, the impression of simple resistance is not communicated to any part by the contact of a tangible object, no ideas of the rough or the smooth, the hot or the cold, or any other mode of common feeling, will be received through

the affected medium; and hence these perceptions are regarded, not as distinct senses, but as modifications of that sensation *common* to most of the tissues of the animal economy. *Specific* sensation implies the perception, through some special medium, of impressions originating notions of the qualities of objects not perceptible through any other; as the eye, for example, is alone the medium of the *veritable* perception of colours, the ear of sounds, and so on.

Now if I understand the argument of Mr. Bishop correctly, he would refer the operation of all the senses to a modification of common sensation; and would, in this respect, regard "taste" as holding precisely the same relation to ordinary feeling as "sight," "hearing," and "smell;" and therefore a modification of common sensation. I must here respectfully submit to Mr. Bishop that this is conceding the whole question; for what is the position that I have wished to establish? Why, that "taste" is as independent of common feeling as the rest of the senses; and that (as in the case of sight, hearing, and smell) we must look for a *special* nervous provision. No axiom in physiology can be better established than that one nerve, if simple in its origin, subserves the performance of one function only; and hence, to prove the independence of any function, is either to overturn the above axiom or to create a necessity for "seeking a separate nervous supply."

I do not undertake to declare, or even to suggest, in the absence of any facts, what is the nerve of taste; only this I contend, that the nervous filaments supplying the tongue and palate with common sensibility—that is to say, with the power of perceiving the hard, the soft, the hot, the cold, the rough, the smooth, and so on—do not endow those structures with the capacity of appreciating the bitter, the sweet, the sour, the saline, and the like qualities. Here, in the publication of the cases relating to this subject, "I *have* abstained from expressing any *mere* views, and confined myself to the fact, and the suggestion of what appeared to be the legitimate induction." In other words, I have, without *comment*, established the premises, and then suggested the proper conclusion to be drawn from them.

I must differ from Mr. Bishop with regard to the inference fairly deducible from his own published case. Because "taste" and "touch" in the tongue were both abolished, coincidently with destruction of the lingual branch of the fifth pair, it certainly is not, "*therefore*, quite unnecessary to look for a separate nervous supply." For if the lingual branch of the fifth pair of nerves *do* endow the tongue with its peculiar faculty of "taste," it may, like the symmetrical nerves of the spine, be a compound nerve; and as these latter comprise filaments both for motion and sensation, so may the lingual branch of the fifth pair include separate filaments for common and for specific sensation; in which case there would most unequivocally be "a *separate* nervous supply." And, indeed, this too is most distinctly admitted by Mr. Bishop, where he states,—"the nerves of the fifth pair doubtless contain *filaments specially modified for the purpose of taste also;* and as compound nerves derive their origins from separate portions of the central mass, it is not at all surprising that one portion of the nerve may be paralyzed without implicating the other,—a circumstance often illustrated by paralyzed parts possessing either motion without sensation, or sensation without motion."

Mr. Bishop has most certainly admitted every thing which it was the object of my papers to prove.

I have the honour to be, sir,
Your obedient servant,
DANIEL NOBLE.

Manchester, Feb. 9, 1836.

MEDICAL GAZETTE.

Saturday, February 20, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

ON SOME RECENT INQUIRIES CONCERNING INSANITY.

FEW things connected with the administration of justice in this country must seem more anomalous, to those who quietly reflect upon them, than the proceedings that take place at lunacy in-

quests, instituted under the authority of the great seal, and those which casually occur before coroners, and on trials for criminal charges. The length of time devoted to the investigation of cases of the former description, where the disposal of property is chiefly concerned, contrasts strongly with the haste in which the other questions are settled—though involving, in general, decisions respecting life or death.

The case of Lady Kirkwall, which has just closed, is a good example of what takes place in civil cases. Here was a commission of lunacy held in order to ascertain the soundness, or unsoundness, of mind of a lady, whose actual state for some years since, upon the slightest inquiry, could scarcely admit of question. Eight entire days were spent in the investigation of all the facts which had, or were supposed to have, any relation to the subject—accumulation of evidence, even to the most extravagant redundancy, was adduced—and several thousand guineas were expended in fees to commissioners, counsel, witnesses, and all who had any pecuniary claim, interest, or concern in the transaction.

Put the case that Lady Kirkwall was on her trial for some alleged criminal offence: how differently, in the manner of its investigation, would the whole affair have been disposed of! The result, no doubt, so far as the finding of the jury, would have been precisely the same; for no twelve men, however plain their intellects, could fail to come to the identical conclusion arrived at, after eight days' deliberation, by the three-and-twenty special jurors engaged in the late inquiry. The facts to be appreciated were strikingly simple and obvious; and one only wonders that half an hour should not have been sufficient to form a judgment of their import. The insanity of Lady Kirkwall clearly involved a derangement both of the intellectual and moral powers. Her mind was highly cultivated in early life, and the beautiful letters written to her children, many years ago, excited the admiration of all who heard them read in court. In 1803, she gave birth to the present Earl of Orkney, and afterwards to his brother the Hon. Col. Fitzmaurice: but for several years she has persisted in denying that they are her children, although in their infancy and boyhood she always behaved to them as the fondest of mothers. In this respect, surely, there is the most evident proof of a complete subversion of the moral—the natural, feelings. Again, she holds that her husband the late Lord Kirkwall, who was separated from her in the year 1810, and died in 1820, is *not* dead; but that she sees him frequently, and that he and others are in a conspiracy against her liberty and life. Also that the late Countess of Orkney, who died in 1831, is not dead—although she was even present at her funeral. Other instances of manifest lesion of the powers of her mind are found in the somewhat ludicrous facts—that she has had no less than *twenty-five cats* at a time lodged in her bed-chamber, and treated as valued guests—that she has fancied she smelt poisonous gases puffed through the walls of her apartment—that she has been more than once shot at in the street, and stabbed in the back, &c. In short, as Drs. Monro and Macmichael, who were examined, said—never was there a more complete case of insanity made out. The unfounded suspicions of poisoning and conspiracy, the unreasonable aversions, and equally gratuitous attachments, which she had exhibited, were perfectly characteristic and decisive as to the nature of the mental disorder with which her ladyship was affected.

Now all this, one should have thought, might have been gathered in the shape

of legal evidence in a very short time: but no; eight days, and the expenditure of large sums of money, were deemed essential, before the very simple verdict could be returned—that Lady Kirkwall was of unsound mind, incapable of managing her own affairs, and had been so since the year 1831.

Of the mode in which inquiries of a like kind are despatched, when criminal charges have to be considered—even in instances where the proofs of insanity have been beyond all comparison more delicate and precarious—we cannot adduce a better example than that which occurred some few months ago, in the case of Stanynought. This unfortunate man, it will be recollected, took away the life of his child, whom he fondly loved; but though there was no proof of his labouring under any delusion—no evidence, in fact, of his intellects being disordered antecedent to, or at the time of, the commission of the fatal act—he was speedily acquitted of the charge of murder, on the ground of insanity, and consigned to an asylum for the insane. His subsequent suicide must remove any doubt that the unhappy man was affected with a species of moral insanity; but certainly, at the time the case was so summarily disposed of, misgivings might be, as they were, entertained as to the real nature of the circumstances. It is, perhaps, not too much to say, that had the mental state of Stanynought been made a subject of inquiry before lunacy commissioners, as it might have been had property been at stake, and the overt act of his alleged disorder not involved the question of criminal responsibility, it is not unlikely that it would have occupied much time, and created a large expenditure in the investigation. So much for the difference recognized in this country between criminal and civil cases.

Let it not be supposed that we are advocates for an undue haste or precipitancy in inquiries of this nature: we merely express a doubt of the fairness of spinning out investigations relative to the disposition of property, while the grave question of criminal responsibility is so summarily despatched. In the case of Stanynought, to which we have just alluded, we believe that the decision, however rapid, was right: but we are full well aware that there was a chance also of its being wrong. In but too many instances, we fear that the practice of leaving to a common jury the determination of the question of soundness or unsoundness of mind, has been attended with lamentable consequences. The case of the man who murdered Mr. and Mrs. Bonner, in Kent, some years ago, must be fresh in the recollection of most readers. The state of derangement under which he laboured was much less obscure than that of Stanynought: he had no delusion whatever—no motive of revenge or avarice for what he did: the master and mistress who fell victims to his sudden and ungovernable desire to kill them, had always treated him with kindness; and to the last the man was unable to give the least account of what had happened, further than that he supposed he must have been prompted to it by the devil. Yet he was executed. The verdict of the jury in Stanynought's case would almost induce us to believe that had this decided instance of homicidal monomania occurred very recently, the catastrophe would have been averted. But it is difficult to speak with any degree of certainty, when the verdict of a jury is proposed as a problem. There is great difficulty, even to the best-instructed minds, in forming a right judgment of many of the common cases of insanity which are met with. The difficulty amounts to danger of an awful kind, when not the seclusion of the subject, but his life or death, is matter of deliberation—his

fate placed in the hands of twelve indifferent persons, who have most probably never given any attention to the manifestations of diseased mind.

The only hope of obviating fatal errors that may arise from this source—of preventing, in fact, judicial murders—is placed in the possible spread of information. We would fain believe that something of this kind has been already effected, and that good has come out of much evil that has been done. At all events the discussion of insanity cases, as often as they come before the public, must, we think, be attended with decided advantage; and it is with that view that we generally conceive it to be our duty to call to them the attention of our readers.

Before we lay aside our pen, it may be as well to mention that a very remarkable case is even now, or has been very recently, a subject of deep interest to the French public, and particularly to the psychologists. We allude to the *affaire* Riviere, in which the accused, a young man of 20, who by all accounts never enjoyed a fair proportion of reason, slew his mother, sister, and brother, and was in consequence condemned to death. But it is thought that a strong representation of the real nature of the circumstances, drawn up by Esquirol, Orfila, Marc, and other distinguished authorities in questions respecting mental derangement, will have the effect of procuring a commutation of the sentence. We may possibly recur to the particulars of this case at another opportunity.

NEW METROPOLITAN UNIVERSITY.

We stated in our last number that it was to Mr. Warburton, in conjunction with the Ministers, that all interested in the subject must look for the new arrangements about to be adopted with respect to the medical profession. Since then an evening paper has published what follows, on the authority of the gentleman above mentioned, and which both corroborates what we said last week, and also affords us some important details on one point in the intended charter. By the by, we were rather amused to observe that Mr. Wakley's grand *coup*—that of gaining some information by questioning Mr. Spring Rice in the House of Commons—totally failed; that right honourable gentleman, though due notice had been given, not thinking it worth his while to be in his place to answer him.

"Edinburgh Town Council, Tuesday.—A letter from Mr. Warburton, M.P., in answer to a letter from the Lord Provost, was read, in which he assures the Council that no one could be more anxious to prevent the establishment of any new monopoly of teaching, and to see instituted some uniform system of medical instruction and examination, to serve as a basis for granting to persons properly instructed and examined the enjoyment of equal professional immunities in every part of the United Kingdom. Whether such a plan could be carried into effect by means of any Crown charter, might reasonably be doubted. He thought government would scarcely be justified in delaying to constitute the proposed new University of London, for the purpose of maturing beforehand, and incorporating with the scheme of that university, a comprehensive measure of medical reform. The Government, he thought, should constitute the new University, investing it with all the privileges appertaining to such institutions, reserving to itself the power of modifying its laws and ordinances, so as to render them generally consonant to any general plan of medical reform that may hereafter be approved of. Except upon certain points, he was not informed of the details of the proposed plan. *The most important that he was aware of, as affecting the interests of medical schools in general, was, that no medical school whatever was mentioned in the charter by name.* A matter of importance for which the London University was contending was, that no teacher of any medical school, and no medical or surgical officer of any hospital, shall be a member of the Board of Examiners."

PROCEEDINGS AT THE CROWN AND ANCHOR.

WE this week publish (see p. 832) another protest, from one of the largest of our metropolitan schools, against the late irregular proceedings of the Meade-and-Wakley meeting. The number of signatures is *one hundred and ninety-seven.* We congratulate those gentlemen who have thus manfully come forward to clear themselves from the imputation of being in any manner connected with the agitators.

MEDICAL EDUCATION IN DUBLIN.

THE SCHOOL OF PHYSIC.

THOSE of our readers who take an interest in watching the signs of the times, and in observing the setting of houses in order,—which is just now the occupation of most of our medical establishments, preparatory to certain anticipated changes,—will probably be not a little amused to learn how such things are managed in the sister kingdom, and more particularly in what is called the *complete* School of Physic in Ireland. The latest intelligence we have received from that quarter apprizes us that *surgery* is no longer deemed requisite to be taught in the University of Dublin: there is, indeed, a professor of surgery attached to the institution, but his office has been rendered this season a sinecure —himself and pupils being put under a ban prohibiting them from following surgical pursuits any longer. We must enter into particulars regarding this curious affair.

Until the year 1832, it was customary for the Professor of Anatomy and Surgery (Dr. Macartney) to teach both branches of medical knowledge in one and the same six months' course, devoting, we believe, a certain portion of that course specially to surgery. But in the year just mentioned, some new Edinburgh regulations were issued, rendering it necessary that surgery and anatomy should be taught in distinct and independent courses, five lectures a-week being given to each. With this the Dublin professor complied, at considerable inconvenience to himself, it being an object, for the benefit of the *complete* School of Physic, that it should be so ordered as that its students should be qualified to take degrees in Edinburgh. Dr. Macartney procured from the University authorities leave to lecture four days a-week, at three o'clock, in addition to his daily lecture at one. This system was pursued steadily for three years, when suddenly the three o'clock surgical lecture was forbidden, and that under such extraordinary circumstances as, in spite of the suicidal mischief done to the school, to render the transaction highly ludicrous. *The doors of the theatre were shut in the faces of both the Professor and his pupils.* It may be asked—is this credible? We can only reply, that the *fact* is vouched for by the following correspondence. The first two letters, it will be observed, were written in 1832, preparatory to opening the winter session: the rest of the documents are recent.

From Dr. Macartney, to the Provost of Trinity College.

My dear sir,—Some regulations were made last year in Edinburgh, by which no course of anatomy or of surgery will in future be recognized as such, unless the lectures be delivered *five* days in the week, during the winter six months.

I do not think this time too great to devote to subjects of so much importance: but if it were otherwise, our school would lose credit by not furnishing as full courses as are expected elsewhere; and the interests of all the professors, as well as mine, would be seriously injured, if the numerous class of medical students that graduate in Edinburgh should be compelled to obtain their qualifications in anatomy and surgery in any school except our own. Much inconvenience has been always experienced both by myself and the students, by having only *one* hour in the day allotted to two courses of lectures (anatomy and surgery), each of which is a subject of greater extent than those taught by the other professors.

I have been obliged, hitherto, to detain the pupils in town after the medical session, and lecture sometimes three times a-day; and even so, have not satisfactorily terminated my lectures. I should add, that I am at present necessitated to give on surgery a *shorter* course than any other on the same subject in the United Kingdom, or, I believe, in Europe. Under all these circumstances, I only wait the sanction of the board, to prolong my courses to the necessary extent; and I shall be much obliged by your bringing the matter before the board, if you can, before you leave town, as the time has arrived for making the winter arrangements.—Yours truly,

JAMES MACARTNEY.

Sept. 28, 1832.

From the Provost, to Dr. Macartney.

My dear Doctor,—The board has agreed

to sanction your proposal of lecturing at three o'clock, *four* times a week, in surgery, in addition to your lectures every day in anatomy.—Yours very faithfully,

BAR. LLOYD.

Provost's House, Sept. 29, 1832.

Now mark the change that took place on the opening of a fourth session.

To Dr. Macartney.

Sir,—I am directed by the board to communicate to you their order of this day, which is as follows:—Ordered, that Dr. Macartney shall not lecture at the hour from three to four o'clock—that hour having been already assigned to another of the professors of the School of Physic.

I am, with much regard,
Your very humble servant,
ROBERT PHIPPS, Registrar.

Trinity College, Oct. 13, 1835.

" Resolved by the board, that this order be communicated to the King's and Queen's College of Physicians, through their Registrar.

The aforesaid order having been violated, it is now further ordered, *that the doors of the Anatomy House shall be closed from three to four o'clock every day.*

By direction of the board,
ROBERT PHIPPS, Registrar.

Nov. 28, 1835.

From Dr. Macartney, to Dr. Phipps.

Sir,—The enclosed Memorial was prepared and signed by a majority of the class attending me; and would have been signed by all (except three or four, who are not seeking information, but the technical qualification of a certificate), when a report was heard, that those pupils should be fined who should sign any memorial.

I therefore send the copy of the one adopted, for the inspection of the board; and beg to know if the Memorial will be received with the signatures of the pupils, without visiting them with any blame.

I am, sir,
Your obedient servant,
JAMES MACARTNEY.

Dec. 4, 1835.

The Memorial of the Students of Anatomy and Surgery in the University of Dublin, to the Provost and Board of Trinity College.

Your memorialists beg leave to state, that they are medical students, attending the lectures on anatomy and surgery in Trinity College; and that many of them have come from England, expressly for the purpose of studying anatomy and surgery in the University. The greater number of your memorialists are matriculated students; but not being students in arts, are precluded from taking a medical degree here, and are preparing themselves for graduation in the University of Edinburgh, or for the diploma of the College of Surgeons in London, in order to which, *complete* courses of anatomy and surgery are required. Of your memorialists, some have become students in arts, for the purpose of obtaining here a complete medical education, and a medical degree; and all have enrolled themselves as pupils of the University, in order to acquire that extended and useful course of medical education, which the present state of medical science, and the high reputation of the professor of anatomy and surgery, led them to expect. Your memorialists further beg leave to say, that they and their friends have made various arrangements, which cannot now be altered without great inconvenience, and some pecuniary loss to themselves, in the expectation of attending the lectures on surgery at three o'clock; and they do not wish, for this reason, to attend any other lectures at that hour. They therefore most respectfully beg that the board will revoke their recent order, and permit them to attend the lectures on surgery, from which they have every reason to expect the most useful information, and certificates of which are, to many of them, indispensable towards their attainment of a medical degree.

From Dr. Macartney, to Dr. Phipps.

Sir,—The board not having signified their pleasure respecting the memorials of the pupils, and the door of the anatomy house having been locked Saturday, and to-day, it may be concluded, that it is intended to persist in the exclusion of the pupils from the surgical lectures at three o'clock. Those who purpose graduating here, are very anxious to know whether the board will grant the licet for examination, without my certificate for surgery; or if they will accept a certificate for the *imperfect instruction* on *both* subjects, which can be given at *one o'clock.* The hour of twelve, (the only one unoccupied by other professors,) would be totally impracticable, as some time is necessary to make arrangements for each of the subjects I teach; and would also be objectionable, as interfering with tutors' lectures, and those on divinity. Some gentlemen are attending me this year, who wish to qualify for both the clerical and medical professions, on account of the present uncertainty of provision in the church. I must beg that the board will condescend to give me an answer on these points, as involving so seriously the interests of the students.—I am, sir,

Your obedient servant,
JAMES MACARTNEY.

Dec. 8, 1835.

From Dr. Phipps, to Dr. Macartney.

Sir,—In replying to your letter of the

8th of Dec. inst., I am directed by the board of Trinity College to communicate, that they will accept your certificate for the attendance of students on your course of lectures, given at *one o'clock, in anatomy and surgery*.—I am, sir,

Your very obedient servant,
ROBERT PHIPPS, Registrar.

Trinity College, Dec. 15, 1835.

In consequence of these proceedings, we understand that Dr. Macartney, in order to accommodate his pupils as far as possible, has determined to give, *during the present season*, five lectures in the week, on anatomy, at one o'clock, and one lecture on surgery in the week; but as the latter arrangement must be deemed insufficient in every institution except Dublin College, he has returned their money to those students who are preparing for the surgical diploma, or for Edinburgh degrees.

Such are the strange things which may sometimes be done by Boards entrusted with the management of medical education. May it not be a type of any metropolitan Board to be hereafter appointed in this part of the empire! But it can hardly be; for the circumstances of the Irish medical school are peculiar. The whole concern is incongruous, being partly belonging to the University and partly to the College of Physicians in Dublin; but at all events the Professorship in question is at the disposal of a non-medical body, who have never been very distinguished for their cordial or liberal bearing to an establishment to which they are far more indebted than they seem to have any disposition to confess.

On the present occasion, the College Board, we think, can scarcely be congratulated save for the original and comic part they have played in the transaction. There has never, we believe, in the whole history of schools and colleges, been a previous instance of the *masters barring out the scholars*, however usual the reverse proceeding has been; yet here we have it performed by the grave seniors of Trinity College, Dublin. And to make the whole affair more ludicrous, the comedy was enacted, too, just before the Christmas holidays; the doors being fast barred even during some part of the vacation!

The consequences, however, of this foolery, may prove more serious than the worthy Board imagine. They have curtailed the usefulness of their Professor; and the lectures on anatomy and surgery in the University of Dublin are now, so far as graduation in any other locality than Dublin is concerned, profitless. This we have thought should be made known to the public; and especially to those students who might, without such warning, repair to Dublin, intending to obtain an education which should enable them to graduate elsewhere. They now know the sort of education they have to expect in what can be, only in Ireland, called the *complete* School of Physic.

ON THE

TREATMENT OF WHITE SWELLINGS.

By M. LISFRANC.

Reciprocal Connexion between White Swellings and Internal Visceral Disease—Effects of Motion—Proper Position—Method of Measuring the Joint—Question of Amputation—Diet—Treatment of the Disease in the Inflammatory Stage—Mercury—Barytes—Bleeding—Baths—Treatment of the Chronic Stage—Leeching—Compression.

General Remarks.

WHENEVER we are consulted by a patient affected with a white swelling, we ought to examine with great care into the state of the thoracic and abdominal viscera, for it is, unfortunately, but too common to find some internal organic disease coexistent with the external tumor. It is unnecessary for me to point out the manner of conducting this investigation, for this would be to describe the various maladies alluded to.

It may perhaps be said, that a white swelling ought to be treated in the same manner, whatever disease may exist elsewhere; but I do not at all coincide in this opinion, because, under such circumstances, when we combat the progress of a white swelling, and attempt to cure it, if we are so unfortunate as to succeed in doing so, the internal disease makes such progress in consequence, as inevitably to prove speedily fatal. On the contrary, in such cases I hesitate not to apply rubefacients, blisters, and other means, to irritate the external swelling, and to prevent it from receding, by which the progress of the visceral disease is retarded. I would add, besides, that even where we have care-

fully examined the internal organs, without finding any thing amiss, disease may yet exist in a latent state, and become apparent just in proportion as the white swelling advances towards a cure; as may be verified by the autopsy when death takes place.

Such cases may be compared to those of amputations performed in those whom we have not suspected of having any internal disease, but in whom, during the fever which follows the operation, the first symptoms of the previously latent affection shew themselves, and rapidly proceed to a fatal termination. There certainly exists between the large joints and the abdominal viscera a degree of sympathy which has not hitherto been sufficiently estimated. The pain and weakness experienced in the joints at the commencement of fevers farther illustrate these sympathies.

Many of you may have observed, a short time ago, a young man (No. 17, St. Louis) affected with a white swelling and phthisis pulmonalis; this patient exhibited, in the progress of these two diseases, some very remarkable circumstances. We saw that in proportion as the phthisis advanced, the white swelling, which at first was very large and painful, gradually diminished and became indolent, so that before death the joint had really regained its natural size. Nothing, in fact, is more evident than such sympathies, and probably they act an important part in the benefit which we derive by producing salivation in cases of white swelling. When we have to deal with articular disease complicated in this manner, we must limit our exertions to the means best calculated to arrest their progress, and direct our main efforts against the internal complaint; or if this be in its nature incurable, then we must be satisfied with palliatives as applied to both. By acting differently we should rather augment the more formidable internal affection; and this point in the treatment of white swellings is extremely important, though not sufficiently attended to.

Unless white swellings be in a perfectly chronic condition, the limb must be kept in a state of absolute repose; and no exception is to be made to this rule, because if the patient use the joint, he incurs the risk of rendering the disease acute. But it must be kept in mind, at the same time, that absolute rest exposes him to the danger of having ankylosis, or, at least, a false ankylosis: it is necessary, therefore, that either the practitioner or the patient communicate a certain degree of gentle and regulated motion to the part every day. There are some rules to be laid down with respect to this also. If the movements give very little or no pain, they may be continued; but if, on the contrary, they excite uneasiness, and more particularly if this remains some time after the movements have been discontinued, they must be altogether abandoned, as it is better to run the risk of ankylosis rather than of acute inflammation, which might hurry on the disease so as to render amputation unavoidable.

Another point, in itself very simple, though not sufficiently attended to in practice, is the position which it is most advantageous to give the limb. This is often neglected; and among the patients who come to this hospital many have the leg bent upon the thigh, so that when the original disease is cured, we have still many months' work in contending against the ankylosis; nor are we always successful even then. If the swelling be in the knee, of course the limb must be kept extended; nor, indeed, is this position more painful than any other; but were it in a slight degree otherwise, still it were better to bear this than have the limb ankylosed in a bent position. I do not think it requisite to point out in detail the position fitted to each member, because this must be obvious from a consideration of its use.

Abscesses often form round white swellings: they must be opened as speedily as possible, because they would otherwise become extensive, burrowing beneath the skin, and forming innumerable fistulæ. When the parietes of these become inflamed, the suppuration is so copious as to endanger the life of the patient.

It is useful, during the treatment, to ascertain the change of volume which the parts may undergo. To accomplish this, three lines may be made at different points around the joint with the nitrate of silver, so as to blacken the skin. Each of these becomes a mark, at which the limb is to be accurately measured; and by repeating the measurement about once a week, the changes of size are readily ascertained. This guides the treatment, and has a moral effect upon the patients; indeed, they will never otherwise believe that the swelling does diminish, owing to the extreme slowness with which any diminution takes place. We ought never, therefore, to omit an expedient which assists the surgeon, and encourages the patient under the long treatment to which he is unavoidably subjected.

When a white swelling cannot be cured, and when we have once thoroughly made up our minds as to this, is it advisable—as some surgeons recommend—to delay the amputation as long as possible, in

order that the patient may become weaker, and the chances of success be increased? This precept is a most unhappy one, and has caused a host of failures. Beware of following it; for while you are procrastinating, the alimentary canal is apt to become implicated, diarrhœa supervenes, ulcerations take place in the bowel, and the case is then lost. If your patient be too plethoric, it is easy to relieve him by blood-letting.

In general too little importance is attached to regimen in the treatment of chronic surgical cases; patients are allowed to continue their accustomed diet, by which the disease is often rendered more obstinate. I have seen many cases which resisted the most approved treatment, and which have begun to yield the moment that the diet was changed. The quantity of food must be diminished in the first instance by one-fourth, and then by one-third, and even, if possible, by one-half; unless, indeed, there be a scrofulous diathesis, or the swelling be in a state entirely chronic. I shall not enter into details on this part of the subject (merely pointing out to you its great importance), but shall proceed to speak of white swellings with *sub inflammation*, and those without this condition.

Treatment of the Acute Stage.

Mercury may be employed, so as to produce salivation; but this sometimes disturbs the stomach before the desired effect is produced, and many persons are unwilling to submit to the treatment, as it is unpleasant, and produces emaciation. Another remedy is presented by the muriate of barytes, particularly in scrofulous cases; but the disease is not always connected with scrofula, and it either fails, or succeeds but partially. Upon the whole the results obtained from both these remedies are often unsatisfactory; when this is the case, recourse must be had to blood-letting.

If the patient be very robust, and the inflammation very acute, two or three palets of blood may be taken from the arm; and this may be repeated next day, or next but one, according to the effects which have been produced. It is of importance to remove the plethoric state, and to empty the vascular system. If after these general depletions the inflammation still persist, recourse must be had to local bleeding; but this must not be pushed too far, because, first you have to deal with patients labouring under a chronic disease, by which they are often already much weakened; and secondly, because the complaint is of long standing, and has acquired, if I may so speak, "a kind of right to the joint," producing in it a change of texture, which cannot return to a healthy state except by slow degrees. Accordingly, when you employ leeches to the extent of fifteen, twenty, thirty, or forty, according to the constitution of the individual, you must not employ them several days successively, because this would reduce your patient too much. It is because these abstractions of blood have been carried too far that some have found them injurious, and looked upon them as inapplicable. It is also necessary to observe how the patient bears depletion. One person scarcely feels any effect from a bleeding, while another becomes feeble and blanched. We must therefore examine the pulse, notice the complexion, &c., and always keep in mind that bleeding ought rarely to be practised oftener than once in six or eight days. Again, if they have already brought the swelling into a chronic state, depletions must be used no longer with the same freedom, or merely as an antiphlogistic means; this would be to weaken your patient to no purpose whatever.

To the abstraction of blood you may join various other antiphlogistics, in the acute stage. If the seat of the disease admits, you may have recourse to topical baths; but take great care that the temperature of these be not either too high or too low; it ought to be such that the patient experiences no sensation either of heat or cold on placing the joint in the bath. Simple emollient fomentations are also advised, but I scarcely approve of them in the acute stage, and think they are only proper when this has passed away. Emollient cataplasms, either simple or with the addition of opium, ought to be applied twice a day. For the most part, the acute stage passes into the chronic in about three weeks, under the use of the above remedies; but sometimes the inflammation lasts much longer than this. I have seen it continue three, four, and even six months; and, indeed, in one case it lasted nine months; after which, the chronic stage which followed likewise endured nine months; when the patient recovered. In such a case, where the inflammation is obstinate, if salivation, or the muriate of barytes cannot be used, or have been so with success, what ought we to do? Are we to apply leeches in large numbers? Certainly not; we should thus reduce our patient too much. We must proceed more gently; applying leeches but only once in each fortnight or three weeks, and choosing for this those times when the inflammation seems to be undergoing an exacerbation. By acting in this manner we frequently contend with equal weapons against this rebellious disease, and we at least succeed in arresting the proper course of the malady. After

a time, in short—either in consequence of the health improving or the disease of the joint having expended its force—the inflammation passes into a chronic state. In such cases the important point is, not to weaken the patient by carrying the antiphlogistic plan too far.

Treatment in the Chronic Stage.

I now proceed to speak of the treatment to be pursued in the chronic stage of white swelling—that is to say, when it is characterized by a complete or almost complete absence of pain and increased heat. It is necessary, in the first place, to establish a principle which is essential to the treatment of white swellings in general, and to this form of the disease in particular. When the acute stage has passed away, if we immediately have recourse to excitants and discutients, even when the chronic condition has existed but a few days, the acute state, which is scarcely overcome, is very apt to return under the use of such remedies. Thus, suppose you observe to-day that a white swelling has become chronic; if to-morrow, or the day after, you apply leeches in reduced number, or pressure, or blisters, or moxa, or douches, you will often see the acute state brought back, because the part has been too much excited by these remedies. It is necessary to wait at least eight days, so as to afford time for the chronic state to be confirmed, before you combat it. I insist upon this the more because it is not sufficiently impressed upon the minds of practitioners.

It is of no great importance in what order we consider the various methods to be employed in chronic white swellings: however, I shall speak first of those which I regard as most efficacious,—most generally applicable, and always to be tried before those more energetic expedients which I shall recount in the last place. The first of these methods consists in the application of leeches in small number at a time.

Mode of using Leeches.

The number of leeches, under such circumstances, may vary from three to six, or at most eight, and this only in robust individuals: it is an excitant means, a true resolvent; and in support of this opinion I shall mention some illustrative facts. In violent inflammation, on an acute attack of peritonitis, for example, we are so much afraid of causing congestion of the part by leeches, that we premise their use by venesection; after which they are applied, but in very larger number. The application of leeches on the upper extremities, as well as small bleedings occasionally repeated, often give rise to symptoms of congestion in the thoracic viscera. Leeches applied in small number, more frequently cause erysipelas than when applied more copiously. If we wish to bring on the menses, we do not apply thirty or forty leeches to the thighs, but limit the application to six or eight; and we should farther endeavour to promote the object by arresting the bleeding from the leech-bites soon after they had dropped off. Barthez held that leeches ought not to be applied till an inflammation was on the decline; and this principle is easily explained, when we call to mind, that, in his time, leeches never were employed in large numbers at a time, and at the onset of an inflammation it were better not to apply leeches at all than to apply but a few.

In patients who have a tendency to apoplexy, when a white swelling or any other tumefaction of analogous character is situated on the upper part of the body, we must abstain from the use of leeches in small number. I have more than once seen cases in which such swellings affecting the elbow, or even the wrist, have had leeches used in this manner, with the result of producing all the indications of apoplexy. You will find in the thesis of M. Costen, how a few leeches, applied to a schirrous breast, have caused congestion of the lungs, or disturbance about the heart; and we ought therefore to abstain from this method wherever there is disease of these organs. It is also clear that the menstrual flux ought to be attended to, and respect had to the six or eight days which precede it; for, by applying leeches at such time to the lower extremities, you will make the period anticipate, and probably render the discharge more abundant; while, by applying the leeches to the upper extremities, you may suspend or retard menstruation. These facts are important, but unluckily there is nothing to be found on the subject in books.

Now, when you apply leeches in small number to white swellings, there may be, 1st, no perceptible effect; you apply four or six leeches to-day, and to-morrow you measure the swelling—you examine it carefully, and find no change;—2dly, there may be an increase of size, to the extent of one, two, or three lines, with a little œdema and a little softening of the tumor. This effect, which astonishes and frightens the patient when it has not been anticipated, is in general very favourable. Absorption soon commences, and in twenty-four or forty-eight hours more, the swelling is found not only to have returned to its former size, but even to have diminished some lines. 3dly, Without œdema or any increase of volume, you determine by the touch that there is a

softening of the tissues; and this will be followed, in the course of two or three days, by a diminution in the tumor: or the excitement may have been greater than we had supposed, in the preceding cases, and then a slight erysipelas comes on. This also is a good omen; for I have never, or almost never, seen an instance in which this was not followed by a marked improvement in the swelling. This superficial inflammation disappears in a few days, and then the tumor, which had at first increased, begins sensibly to diminish. Lastly, common erysipelas may come on: this constitutes a complication of the disease which must not be left to itself, for the inflammation may extend to the subjacent textures, and you know what mischief this may do in textures already so much diseased. Leeches must then be applied in great numbers, and generally succeed in arresting the erysipelas; in which case, the same advantageous effects result as from the milder degree of the disease above alluded to.

When are we to renew again the application of but a few leeches, on the former principle? The period must necessarily vary according to the effect which is produced. When they have effected no visible change, I wait five or six days; but if any effect has followed, I then rest on my oars till this has disappeared for two or three days. Leeches thus applied do not weaken the patient, and may therefore be used many times; but at length they lose their effect, the constitution becomes habituated to them just as to opium, and we must then abandon them for a time for other remedies; after which we may return to them again.

But if this method may be employed alone, it is also capable of being combined with other means. Thus, when the leeching has produced softening of the swelling, but this is not followed by a diminution of its volume, we may then have recourse to compression. After a time the leeches may again be used, and, in fact, the two methods may be alternated.

Of Compression.—The end proposed by this means is double: first, that of lesseni the arterial circulation in the tumor, so as to diminish its irritation, and secondly, that of promoting absorption.

What proves that pressure produces the former effect is this,—that if it be only established on the tumor itself,—if the cones of agaric do not extend at least an inch beyond its circumference, the compression succeeds less fully, less frequently, and less rapidly. Is it not universally known that pressure causes the part to which it is applied to waste, or at least arrests their growth? Is it not thus that a ligature round a tree prevents the growth where it is applied? Is it not well known that too tight garters, especially in women, lessen the volume of the limb, and after a time lead to deformity? Experience proves that in acute inflammations, pressure often renders them more aggravated, while in indolent swellings it often brings back the acute state, so as to require to be discontinued. As we regulate the dose of opium, of antimony, and other medicines, why should we not also measure our dose of pressure? Are swellings always equally hard?—are they always equally sensible? Certainly not: and accordingly we must carefully regulate the degree of pressure, so that it be proportioned to the state of the parts, the duration of the disease, and various other circumstances.

First dose.—Slight pressure, produced simply by circular bandages.

Second dose.—Pressure a little increased by means of agaric cones and bandages, as before.

Third dose.—More energetic pressure, with graduated compresses, and circular bandages.

Fourth dose.—Still stronger pressure, made by means of some hard flat substance, or pieces of money wrapt in linen, and circular bandages. Pressure employed in this way succeeds extremely well on the hand, and indolent swellings which remain after fractures and luxations of the wrist, and which resist in general the other *doses* of pressure. But it is evident that this cannot be applicable in white swellings, or any other tumor which has but recently become chronic.

Fifth dose.—This consists in kneading (*malaxation*). We had lately a patient here who had an induration at the outer part of the knee, which had resisted the other methods of compression, and only yielded to this.

We must always be ready to supend the compression when it passes the degree and effect intended, and its employment must be subjected to all the rules which ought to regulate the practitioner in the use of discutients. Thus, if heat and pain reappear, it is to be left off: if the effects just mentioned last more than twenty-four hours, they are to be combatted by antiphlogistics, and we only return to compression when these effects have entirely disappeared. Some details may be necessary as to the application of the second dose of pressure. The agaric ought to be soft, thick, and good; for if it be hard or thin, and but little elastic, the pressure would necessarily be greater, and become equivalent to a higher dose. The agaric is to be cut into round portions of unequal size, so that when placed one above another, they may form truncated cones of an inch or an inch and a half in height:

the base of the cone ought to be sufficiently large to embrace the tumor, and extend an inch beyond its circumference. In most swellings of this kind, several such cones are required to effect a regulated pressure over the whole extent.

What follows with regard to pressure, applies to other tumors as well as to white swellings. Sometimes the tumors are moveable, and roll so as to escape from the action of a single cone of agaric: nothing is more common in glandular swellings. In order to fix such a tumor, several small cones are to be placed round the circumference of the tumor, and one large one upon it. In the axilla, for example, in order to prevent enlarged lymphatic glands from escaping, a cone must be placed above them, so as to force them downwards, and keep them within the action of the larger cone, which is then to be placed right over them.

The pressure ought not to be kept up during three or four successive days, because the circular bandages get relaxed, and the pressure thus becomes unequal; but the apparatus ought to be resumed once in each twenty-four hours, and the patient allowed to remain about a quarter of an hour without it.

When pressure is applied to the chest, it must be made lightly during the first few days, lest the respiration be too much impeded. M. Recamier has contrived a kind of corset expressly for this purpose.

As to the rest, compression may be employed at the same time as other means calculated to disperse the tumor, such, for example, as frictions with the hydriodate of potass; it ought also to be alternated occasionally with the application of leeches in small numbers. When the swelling is quite gone, it is, nevertheless, prudent to continue the pressure for some weeks, or even months, if the case be one of schirrus, as we thus frequently prevent the recurrence of the disease.

It has been said that pressure would cure cancer in a state of ulceration. I do not think so; and, in truth, if you consult the cases which have been published, you will perceive that pressure has not been employed alone. The centre of the tumor has first been destroyed by means of the knife, or caustic potass, after which pressure has been employed, with a view of dispersing the rest of the disease. I believe that in such cases, where they have proved successful, the cancer has been destroyed by the first part of the process, and that the surrounding induration has been no more than that hardness which we meet with around such tumors, but which has not yet taken on the cancerous action. This is a subject on which I have elsewhere advanced opinions, which appear, to me at least, to be of great importance.

In tumors regarded as cancerous, as well as in all others, I reject the plan of compression — 1st, when the symptoms show a certain degree of acute inflammation, either in the tumor itself or in the surrounding textures; 2dly, when the tumor, although not very large, is hard, unequal, and knotted, adherent to the skin, and particularly when the skin itself is the seat of suspicious-looking ulceration (unless removal of the really schirrous part be premised); 3dly, when the tumor, whatever be its size, presents some points which are indurated, and others which are soft; when these softened portions afford that kind of spurious fluctuation often mistaken for genuine fluctuation, which shows a pultaceous disorganization of the tumor.

It is because these cases have not been distinguished,—it is because the necessary indications have not been attended to,—that compression has been so often r-jected as a dangerous precedent. It was this, without doubt, which led M. Dupuytren to describe it as rendering cancers more adherent, accelerating their progress, and producing ulceration. I have, however, to congratulate myself on having many times succeeded in saving the mammæ, in women in the better ranks of society, after other surgeons had condemned them to amputation, as the only possible means which remained; and this success is the more gratifying, inasmuch as many of these cases are now of several years' standing, and appear to be, to me, radically cured.

When the tumor, then, is not already far advanced, we may hope to be able to cure it; and still more frequently do we succeed in diminishing the swelling, and rendering any operation more simple when at length it becomes necessary. But if a tumor, after having been improved by compression, or without such change, becomes again altogether stationary—if no impression be made upon it by any other discutient means, we must be prepared to see, as I have often done, a sudden and rapid cancerous degeneration supervene. Under such circumstances do not think of pressure, but operate without loss of time. I think, in short, that compression, well regulated, and applied in cases properly selected, may succeed, and do wonders. Patients, women particularly, will always submit to it more readily than to an operation. But if the compression should fail, or even produce any inconvenience, we then have the knife left when we are satisfied that it alone will do.

I shall conclude these remarks by re-

commending you to attend to the medical treatment as well as to the surgical, for you will thus certainly have more success. The treatment by pressure must therefore be aided by such internal remedies as the case may appear to require.

ST. THOMAS'S HOSPITAL.

Mr. Travers on various Causes of Stoppage of Urine—Obstructed Ureters—Burst Bladder —Perforated Bladder—Retention from Paralysis.

There may be many different causes of an obstruction to the passage of the urine. It might happen to you, as it has to me, to be called to a patient whose belly is much distended, who is suffering great pain, and into whose bladder you pass a full-sized catheter without any difficulty, and not a drop of urine escapes. However, you feel assured that you have made no mistake; because that peculiar sensation of free motion of the extremity of the catheter with which every man is familiar who is accustomed to use it, satisfies you that the instrument is in the bladder. But no relief follows; and in the course of a few hours, at all events in a very few days, the patient dies. I was called to such a case a few miles out of town. It occurred in a corpulent gentleman, in whom, after death, I found that both ureters were filled with calculi, one at the pelvic, and the other at the vesical extremity; so that not a drop of water found its way from the kidneys to the bladder. The effect of this was to produce an absorption of urine, and arrest the secretion of the gland. In another case which was under my care many years ago, the same thing happened, only that it was more gradual and complicated. First of all there was great dysuria, and then what appeared to be suppression of the secretion, ischuria renalis: we found, on one side, a very diseased kidney, and a rose-bud carcinomatous fungus shutting up the corresponding ureter at its termination in the bladder; and the other, by which the urine was chiefly secreted, had a calculus impacted in its duct; so that here again it was the ureter, and not the urethra, that was in fault, though the case was strictly one of retention of urine. I need not say that in such a case one can do no good. Some time ago a body was opened in this hospital, in which there was an enormous sac, like a secondary bladder, formed out of the canal of the ureter, which had become obstructed and actually doubled upon itself. My son conducted the examination; it occurred in a patient under the care of Dr. Williams.

Another case I may mention by the way. You are called to a patient suffering great pain, and sudden inability to void his urine, after a fall or blow upon the belly. The catheter passing easily into the bladder, urine flows in small quantity, but suddenly stops.

I was called to a stout heavy man, in middle life, who having fallen over a bench upon his belly, in a public-house scuffle, was seized soon afterwards with acute pain, and found he was unable to relieve his bladder. No sooner was the catheter introduced, and about a quarter of a pint of fluid evacuated, than the stream stopped suddenly, and the bladder was collapsed. In about thirty-six hours he died in great agony. You may anticipate what was found; it was a case of burst bladder, the contents having escaped through a rent in that viscus into the abdominal cavity, the bladder having been at the time of the accident in a state of distention.

A surgeon of eminence told me of a case in which a young practitioner having made a good deal of effort to pass a stricture, thinking that he had at length overcome the difficulty, left the patient, who expressed the sensation of making water. Shortly afterwards he was summoned, and found the man writhing with pain, having passed no water, and his belly swollen: in a few hours he died. The surgeon had actually contrived to drive an instrument of some sort through the coats of the bladder into the peritoneal cavity. Without great ignorance and great temerity, this could not have happened; but so it was.

I doubt if, from simple retention, a bladder would ever be found to burst, as the uterus, for example, is sometimes known to do from its own action, in endeavouring to expel its contents, because the action by which the bladder is emptied depends in a greater degree on the surrounding muscles. It has no great muscular power of its own, as is indeed proved by its incapacity to act at all on its contents, after a certain extent of dilatation has taken place. Extreme dilatation has the effect of paralyzing the organ; and, therefore, we every now and then meet with a case of complete retention of urine from over-distention of the bladder. You know that in accidents of every description, from shock of the nervous system, we are very often obliged to resort to the catheter, sometimes for days, but more frequently once or twice only. But it will often happen, without a practitioner adverting to it, that a man will, for a very long time, labour under a partial retention

of urine; and I have known this inattention, from whatever cause, carried to a fatal extent.

You will be surprised to hear that an old gentleman, whom I was once called to the country to see, had passed no water, unless perhaps a few drops now and then, for nine days; and you will be much more surprised when I tell you, that his symptoms were even referred to other causes, and that I passed a catheter into his bladder with the most perfect ease. There was literally no obstruction worth noticing to the passage of the instrument; but it was some time before any fluid would pass, and when it did, it was like any thing but urine. It was purulent, sanious, and horribly fetid; but either the retention had been overlooked or concealed; for, strange to say, it had not been removed, although, in point of fact, the operation presented no difficulty. It is a lesson, to show that every practitioner should make himself familiarly acquainted with the manual, the little tactic of the catheter, as well as with the phenomena of retention; for it is as much from a want of confidence in some cases, as from over confidence in others, and both due to ignorance, that such mischiefs arise. This old man's bladder presented an enlarged prostate, acting as a valve, and was found to have undergone, from excessive distention, a complete slough of the mucous coat, which was detached, and floating in the form of a flocculus. In such a case you can only get the urine to flow through the catheter by actual pressure on the over-distended bladder.

* * * * * * *

Use of the Catheter—Precautions in forcing a Stricture—False Passages.

If a stricture is causing so much retention as that it becomes justifiable, nay, a duty, to employ force, there are certain precautions by which a man whose hand is practised will prevent the accidents that would otherwise be likely to befal the patient. The most important of these is the guarding of the perineum, that is, the membranous portion of the urethra, by planting the forefinger and the finger next it beneath the bulb and behind the pubes, so as to support the parietes of the canal —always taking care to preserve the median line, for all wanderings from this are dangerous; and keeping the point of the catheter as accurately as possible in contact with the upper or anterior surface of the urethra as it glides along. If you do this, and at the same time support the parietes of the canal, by placing the fingers in the perinæum, one on either side, you are in little danger of doing mischief, especially if you do not use an inconsiderately long or curved instrument.

The old surgeons, especially the French, used to employ a manœuvre, which they called the *tour de maitre*, in passing the catheter. They entered it with the convex side uppermost, and having passed it on until they had reached the part at which the canal quits the bulb and passes through the triangular ligament into the pelvis, they revolved the instrument upon its point. In that evolution, if dexterously performed, it often happens that the instrument will slip, as it were, into the bladder. However, this is a manœuvre that one cannot hold out to general imitation, on any reasonable ground of preference to a simpler method. Every man has a way of his own, but the simplest method is generally the safest. Having carried the point of the instrument to the pelvic or fixed portion of the canal, the remaining manipulation consists only in such a depression of the handle as shall tilt the point of the instrument into the bladder. Amply sufficient force can be thus applied without any onward pressure. The recumbent is preferable to the standing posture, as it takes off muscular resistance and fixes the pelvis. A middle-sized catheter should be tried in preference to a small one. You will readily believe that much depends on habit; and as it is an operation to which the surgeon is often called on urgent occasions, on the result of which sometimes hangs the life of the patient, it is worth his while to make himself throughly versed in it, and to omit no opportunity of practising it.

Except it is a case of absolute retention of urine, you are not justified in using an extraordinary degree of force. But you may be called to a case in which spasm having supervened upon a bad permanent stricture, from the patient having drank too much at a revelling party the preceding night, or delayed too long from inebriety, sleep, or even forgetfulness, to unload his bladder; he is unable to pass a drop of water, and suffering great agony from ineffectual efforts for his own relief. In such a crisis, the state of bad permanent stricture having previously existed, it is loss of time to employ the remedies applicable to the pure spasm; though I know no objection to giving fifty drops of laudanum, and repeating the dose in ten minutes if called for, or to douching the pubes with cold water. But as the state of parts is not propitious to the employment of instruments, though force will be necessary, it should be tempered with caution.

If the perineum be of ordinary dimensions without enlargement, or any great enlargement, of the prostate, you may

materially help yourself by passing a finger into the rectum; the instrument being in the urethra, at the seat of stricture, tilting the point when you feel it bearing on the finger through the wall of the rectum, will very often throw it into the bladder. In some bad cases, after working for a length of time, I have succeeded by that plan.

With regard to the *sonde conique* used by some of the French surgeons, although I once employed it with perfect success in an inveterate stricture in this house, where caustic had failed, yet the use of this and of the stiletted or lancetted instruments I think better avoided in the treatment of stricture, because, in any hands whatever, a great additional risk is run of doing mischief.

Many cases are made worse by treatment; and it is probably not too much to say that more suffer from want of skill, imprudent force, or abuse of the remedy, whether the common or the caustic bougie, where the patients pass through many hands, as those of the humbler class, than even from neglect or intemperance on their part. One great source of these mischiefs is a foolish feeling of baffled pride in the young surgeon if unable to reach the bladder, even though the extension be not urgent. This is most absurd, and greatly to be deprecated. It is by steady repetition of the attempt that the severe stricture is most safely and surely overcome. Absolute retention is the only apology for the employment of force with every precaution. There are, in the collection of this hospital, numerous examples of false passage. One shows a cylindrical perforation of the triangular fold at the base of the prostate, the *trigon vesical*, and another the prostate itself bored through by an instrument in its way to the bladder. Some show nests of abscesses in the bulb of the urethra, and in the prostate, with thickened and variously diseased conditions of the prostatic urethra and bladder. In others we may trace so confirmed a state of false passage, that after quitting the canal for some lines, the instrument regains it before entering the bladder. This may happen anterior to the bulb of the urethra, or in the bulb, or in the membranous portion, or beside the verumontanum. Further, the instrument may pass between the rectum and bladder, or even penetrate the rectum, or between the abdominal muscles and peritonæum; and I have seen urinous abscesses thus formed in the tunica vaginalis reflexa, and in the posterior peritonæal reflection communicating with the abdominal cavity.—*St. Thomas's Hospital Reports*, No. 2.

SMALL-POX AND VACCINATION HOSPITAL, ST. PANCRAS.

Dr. Gregory's Report, addressed to the Governors.

During the ninety years which have elapsed since the foundation of the Small-Pox Hospital, there never, perhaps, was a year in which its advantages to the public have been more conspicuous than in that now passed. 401 persons have been treated within its walls, and so many centres of infection have been removed from this vast metropolis. Of that number, 89 have died, and 312 have been restored to their families and friends.

4140 persons have been vaccinated at the hospital in 1835; and 1767 persons, chiefly medical practitioners, have been supplied with vaccine lymph, for the use of the public, at home and abroad. It is no exaggerated statement to say, that nearly 7000 persons have participated in the benefits of this charity during the course of the past year. It will readily be conceded, that such ample opportunities must have furnished to your physician much useful information. To some points in which the public are interested, no less than the members of the medical profession, he n w proceeds briefly to direct the notice of the Governors.

The records of the hospital, during the past year, sufficiently attest that the hopes once entertained of banishing the small-pox from the earth, are utterly visionary and groundless.

Thirty-six years have elapsed since the general diffusion of vaccination throughout this country; yet small-pox still exists, and, by the bills of mortality, may be shewn to have proved fatal to 863 persons in London alone during the last year. In various parts of the country it has shewn itself during the same period. Nor do other quarters of the globe enjoy an exemption from the disease denied to us. On the continents both of Europe and Asia, small-pox has recently occasioned considerable devastations, and attracted a large share of public attention. Without inquiring how much of this depends upon the defects, and how much upon the neglect, of vaccination, enough appears upon the face of the statement to shew, that an hospital, exclusively devoted to the cure and prevention of small-pox, is imperatively required in such a metropolis as London; and that the patronage of the public cannot be withdrawn from it on any plea of its diminished efficiency.

The experience of the past year shews, secondly, that the confidence of the public

in and about London, in the protective power of vaccination, has in no degree diminished. The numbers vaccinated at the hospital in 1835, *exceed that of any former year*. In no instance has any doubt or distrust been manifested by the parents of the children; and as small-pox infection has been rife in all parts of London throughout the whole of 1835, their confidence can have resulted only from personal experience of the security which vaccination holds out.

The records of the hospital shew further, that this security, though so remarkable in the early periods of life, is not so complete and permanent in after-life as might be wished, and as at one time might reasonably have been anticipated. One hundred and forty-four persons, labouring under small-pox, have been admitted during the year, who in early life had been vaccinated. All of them, with few exceptions, were adults; an interval, varying from ten to thirty years, having elapsed since the date of their vaccination. Two thirds of this number had small-pox in a mild form, wholly devoid of danger. The remaining third were less fortunate, having passed through the disease with greater or less degrees of severity. Still the mortality in this class of patients was very small, hardly amounting to five in a hundred, while the unvaccinated perished in the proportion of thirty-three in a hundred.

These considerations tend to shew, that vaccination is still entitled to the highest praise as a means of diminishing both the quantity and the severity of small-pox—that it is an object of national importance to encourage and foster it—and, lastly, that the attention of medical men cannot be too strongly called to the necessity of carefully selecting the lymph they employ, and accurately investigating the several causes on which its diminished efficiency in the more advanced periods of life may depend.

George Gregory, M.D.
Physician of the Small Pox and Vaccination Hospital, King's Cross.

31, Weymouth-Street,
Feb. 4, 1836.

REMUNERATION TO MEDICAL WITNESSES.

PETITION TO PARLIAMENT.

To the Editor of the Medical Gazette.

Sir,

I enclose you a copy of the petition to the House of Commons prepared at a meeting of the Committee of the Bucks Medical Association, lately held at Aylesbury. It is now in course of signature, and will shortly be ready for presentation. The support of its prayer will be earnestly requested of the county and borough members.—I have the honour to be, sir,

Your obedient servant,

Robert Ceely,
Hon. Sec.

Aylesbury, Feb. 15, 1836.

To the Honourable the Commons of Great Britain and Ireland, in Parliament assembled.

The Petition of the undersigned medical practitioners, in and near the county of Bucks, humbly sheweth,

That your petitioners have at all times been ready and willing to further the ends of justice in their attendance on coroners' inquests.

That the duties of medical practitioners on such occasions are highly important and very responsible, and require, for the due performance thereof, extensive knowledge (acquired at considerable expense) and the sacrifice of much time and labour, often at a distance of many miles from their habitations.

That for the satisfaction of the jury, it is not unfrequently required to pursue a series of delicate, tedious, and intricate chemical investigations.

That still more frequently it is necessary to institute post-mortem examinations with care and judgment, and oftentimes at considerable risk of health and life.

That your petitioners humbly submit, that a coroner's inquest, without efficient medical evidence, can be but an empty form, and consequently a source of useless expense.

That your petitioners, in common with the medical profession, consider it a great hardship that the law does not provide any remuneration for such important services.

That your petitioners respectfully, but earnestly, beseech your Honourable House to afford them substantial relief from so manifest an injustice, by the enactment of such a measure as your Honourable House, in its wisdom, shall think fit; and your petitioners will ever pray, &c.

REMARKS

ON

RURICOLA'S SCALE OF REMUNERATION FOR PAROCHIAL SURGEONS.

To the Editor of the Medical Gazette.

Sir,

The letter of your correspondent, "Ruricola," in a late No. of the Gazette *,

* See Medical Gazette, Jan. 16, 1836.

has afforded me much satisfaction, and I doubt not but that feeling is general throughout the profession. Sincerely do I hope that the principle which he has suggested may be adopted by the different Boards of Guardians throughout the kingdom. Approving, however, as I do, of the principle, there is one suggestion which I am anxious to make respecting the details of the plan, without which, I think, the scale of remuneration will prove too low. I know, from experience, that under the new Poor-law, a more than average share of chronic cases of illness falls to the lot of the parochial surgeon; some of those cases being of many years' standing. I would therefore recommend, that when a case continued on the list more than three months, the order should be renewed; and thus a case continuing for a year would be paid for as four cases. If this is not done, the medical attendant on very small parishes, where the cases do not average more than four or five a year, will, if one or two of these should prove tedious, be very inadequately requited for his services.

I have calculated the attendance on six parishes in the Eastry Union for the last six months, according to the scale suggested by "Ruricola," and find that it amounts to rather more than the sum which the Union has agreed to pay; but that sum is so very low, that I do not think that his scale, even with the alteration I have suggested, would be too high. This, however, is a question which experience only can decide: the principle, I think, is unobjectionable, and I therefore hope that some of the Medical Associations will print his letter for general distribution.—I remain, sir,

Your obedient servant,

FREDERICK H. SANKEY.

Wingham, Feb. 15, 1836.

SELF-SUPPORTING MEDICAL INSTITUTION.

An institution of this kind has been established at Portsmouth, but we confess we cannot help having our doubts as to its stability, when we observe the only apparent source of its finances. The "Rules," a copy of which we have before us, open in this way:—"Every individual (without children) one penny per week. Every husband and wife (without children) one penny each per week. Every widow or widower, with one child, one halfpenny each per week. Every man and wife, with one child, one halfpenny each per week; and for every additional child one halfpenny, till the family shall amount, including parents, to six in number; after which, no additional charge will be made for children under 18 years of age." After stating several other regulations as to the parties eligible to the benefits of the institution, and the sums respectively which they must pay, it is added—"For the above small subscriptions, the undersigned engage to find every working hand residing in Portsmouth, Portsea, Southsea, and Landport (who cannot afford to pay for medical attendance in the regular way), as good advice as the town can produce." The *undersigned* are two physicians and four surgeons. *If* they can afford to bestow their labour for such emolument as may result from the subscriptions just mentioned, it is well; if, however, their object be to encourage habits of prudence and independence among the humbler classes, it is still better: for any expedient must be desirable which can rescue the poorer portion of the population from the tender mercies of the Poor-law authorities.

ROYAL INSTITUTION.

February 12, 1836.

Dr Grant on the Structure of Fishes.

It is highly gratifying to witness the interest which large and popular audiences now take in matters of natural history and comparative anatomy. The theatre this evening was filled with members and their friends, who all seemed delighted with what they saw and heard. The object of Dr. Grant's discourse was to show that every part of the structure of fishes was peculiarly adapted to the element in which these animals live, and move, and have their being. The lecturer began by describing the structure of the skeleton of fishes—that part which gives support to the whole system. He then proceeded to the muscles—the circulatory apparatus—the nervous system,—and lastly, he noticed the branchial arrangements, and the physiology of the swimming bladder, which he seems to consider as bearing some analogy to the lungs of warm-blooded animals. We very much admired the order and method adopted by Dr. Grant in this lecture, and the tact with which he availed himself of his numerous and highly-valuable illustrative drawings. In conclusion, he amused his hearers with a lively account of the importance of fishes in the scale of creation: he showed that, in addition to the abundant prey which the inhabitants of the deep make of all those beings below them living in the same element, by far the greatest number of animals belonging to earth and air, are

destined to be food for fishes; and he ended with the startling announcement, that perhaps the remains of the great Alexander might be traced to the same destination.

PROTEST OF THE STUDENTS OF LONDON UNIVERSITY,

AGAINST THE LATE PROCEEDINGS AT THE CROWN AND ANCHOR MEETING.

To the Editor of the Medical Gazette.

SIR,

We have great pleasure in forwarding to you the accompanying paper, signed by a large number of the medical students of the London University, who are desirous of disclaiming all connexion with the originators and proceedings of the meeting of medical students, held at the Crown and Anchor Tavern, on Monday the 18th of January.

We have the honour to be, sir,
Your very obedient servants,
ALFRED LEGGATT.
JOHN THOMAS DARVILL.

Feb. 17th.

"A report having been widely circulated, that the medical students of the London University were highly instrumental in causing and supporting the meeting of medical students, held at the Crown and Anchor Tavern, on Monday the 18th of January, we the undersigned students of medicine in the London University, do hereby disclaim all connexion with its originators and proceedings."

Signed by 197 *students.*

[The original document, with the autograph signatures attached, has been forwarded to us: we regret we have not room to give the names at length.—ED. GAZ.]

MORISON'S PILLS.

ANOTHER VERDICT OF MANSLAUGHTER.

A CORONER'S inquest was held this week, in the workhouse at Radcliff, on the body of Captain Mackenzie, aged 32, who died in consequence of large doses of Morison's pills, administered to him by a man of the name of Salmon, a "Hygeian agent." The proceedings occupied the better part of five days; much evidence on both sides being heard. The verdict was, "Manslaughter against Robert Salmon, for the improper and incautious administration of large doses of Morison's pills to the deceased, by which he caused his death." One of the Morisonian witnesses stated that he (witness) had swallowed 1000 of the pills in twenty days!

ROYAL BIRMINGHAM SCHOOL OF MEDICINE.

THE Birmingham school now rejoices in a *royal* title. We perceive that in accordance with a petition to the King, numerously signed, and with some noble names among the signatures, his Majesty has been pleased to become the Patron of the school. Sir Herbert Taylor's letter, signifying the King's acquiescence, is dated the 22nd ult.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Feb.* 16, 1836.

Abscess	1	Hooping Cough	2
Age and Debility	31	Inflammation	30
Apoplexy	8	Bowels & Stomach	5
Asthma	22	Brain	3
Childbirth	5	Lungs and Pleura	5
Consumption	69	Insanity	3
Convulsions	23	Liver, diseased	2
Croup	4	Measles	5
Dentition or Teething	8	Mortification	4
Dropsy	14	Small-pox	16
Dropsy on the Brain	10	Sore Throat and Quinsey	1
Dropsy on the Chest	1	Spasms	1
Erysipelas	2	Tumor	1
Fever	4	Unknown Causes	1
Fever, Scarlet	6		
Fever, Typhus	2		
Heart, diseased	6	Casualties	3

Decrease of Burials, as compared with the preceding week . . 113

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Feb. 1836.	THERMOMETER.		BAROMETER.	
Thursday . 11	from 30	to 40	29·91	to 30·23
Friday . . 12	27	47	30·04	30·03
Saturday . 13	21	41	30·25	30·20
Sunday . . 14	35	45	30·20	30·37
Monday . . 15	24	40	30·37	30·36
Tuesday . . 16	24	55	30·18	29·83
Wednesday 17	25	37	29·83	29·84

Prevailing winds, N.W. & S.W.

Except from the 11th to the 16th, generally cloudy, with frequent rain. A little snow on the evening of the 17th.

Rain fallen, ·3 of an inch.

CHARLES HENRY ADAMS.

NOTICES.

MR. PORTWINE ought to have more sense than to send us a letter utterly unfit for our pages: he has no right to indulge in coarse personalities towards a gentleman of whom he knows nothing, and who never once mentioned his name.

The letter *to Mr. Meade* is too long for publication, even if we were to permit our Journal to be made a medium of communication with that gentleman. We wish the writer had adopted some more brief form of exposure.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, FEBRUARY 27, 1836.

LECTURES ON MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

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LECTURE XXII.

IODINE.

General History.—Iodine was discovered in 1811 by M. Courtois, a saltpetre manufacturer at Paris. It was first described by Clement in 1813, but was afterwards more fully investigated by Davy and Guy Lussac. It was named Iodine, from ἰωδης, violet-coloured, on account of the colour of its vapour.

Native state.—It exists in both kingdoms of nature.

(*a*) *In the inorganized kingdom.*—Vauquelin met with iodide of silver in a mineral brought from Mexico, and Mentzel found iodine in an ore of zinc which contained cadmium. In sea water it has also been discovered, where it probably exists as an iodide of sodium or of magnesium. Many mineral waters likewise contain it. In the carbonated chalybeate of Bonnington it was detected by Mr. Copeland. About one grain of iodine was found by Dr. Daubeny in ten gallons of the water of Robbin's Well at Leamington, in Warwickshire. In the old well at Cheltenham the quantity was not more than one grain in sixty gallons. In a brine-spring at Nantwich, in Cheshire, there was about a grain of iodine in twelve gallons. In the sulphurous water of Castel Nuovo d'Asti, iodine was discovered by Cantu. In some of the mineral waters of Germany, Bavaria, and South America, it has also been detected. Fuchs found it in the rock salt of the Tyrol.

(*b*) *In the organized kingdom.*—Of *animals* containing iodine I may mention the genera *spongia*, *gorgonia*, *doris*, and *venus*: likewise *sepia*, the envelopes of the eggs of which, contain it. A very considerable number of *vegetables*, particularly those belonging to the family *algæ*, yield it. The following are some instances:—*Fucus vesiculosus*, *f. serratus*, and *f. nodosus*; (fig. 91 *a*, *b*, *c*.)

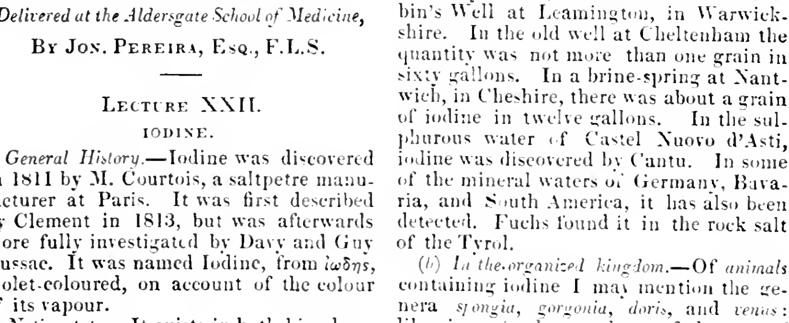

FIG. 91.

a, Fucus vesiculosus,
b, F. nodosus.
c, F. serratus.
d, Laminarla digitata.

laminaria saccharina, and *l. digitata*; (fig. 91, *a*) *halidrys siliquosa*; *chorda Filum*; *gelidium cartilagineum*; *halyseris polypodioides*; *phyllophora rubens*; *rhodomenia palmata*; *ulva Linza*; *porphyra umbilicalis*; *and padina Pavonia*: some of the marine confervæ—*gigartina helminthocorton*, *zostera marina*, &c.

Preparation.—The iodine and iodides of commerce are obtained from the *fucoideæ* (one of the orders of the family *algæ*). These, by combustion, yield an ash or cinder, commonly denominated *kelp*, which contains iodine. Davy states that French kelp is more productive than British; and Eklond, from experiments made at the Cape of Good Hope, concludes that the *laminaria buccinalis* yields more than any European algæ.

Kelp is a very heterogeneous substance. Its soluble parts are the chlorides of sodium and potassium, carbonate of soda, sulphates and nitrates of soda and potash, and the sulphuret and iodide of potassium or of sodium. The quantity of iodide, however, is very small in comparison with the other salts, and, therefore, the first object in the manufacture of iodine is to separate as much of these as possible. By repeated crystallizations we readily attain this, since the iodide being very soluble is left in the mother liquor, along with the sulphuret and a portion of the other salts.

This liquor is introduced into a stoneware still, sulphuric acid and the peroxide of manganese added, and heat applied. Iodine distils over, and after being washed with water, is dried between folds of bibulous paper.

The following is the *theory* of the process. —The mother liquor contains in solution a portion of all the soluble salts of kelp: when, therefore, the sulphuric acid is added, various gases are evolved, while the sulphates of potash and soda are left in the still. Thus the chlorides give out muriatic acid; the sulphurets, hydrosulphuric acid; the carbonates, carbonic acid; the nitrates, the nitric acid; and the iodides, the hydriodic acid. By the mutual action of the muriatic and nitric acids, we obtain chlorine and nitrous acid: by the action of the peroxide of manganese on the hydriodic acid, we obtain water, iodine, and the protoxide of manganese: the latter unites with sulphuric acid, while the iodine distils over. The mutual action of iodide of potassium, sulphuric acid, and peroxide of manganese, is shown by the following formula.

Reagents.	Results.
K I	I
$2\,\dddot{S}$	$\dot{K}\,\dddot{S}$
$\ddot{M}$	$\dot{M}\,\dddot{S}$

From this you observe, that one atom of iodide of potassium, two of sulphuric acid, and one of peroxide of manganese, yield one of iodine, one of the sulphate of potash, and one proto-sulphate of manganese.

Properties. — Iodine is a crystallizable solid, its primary form being a rhombic octahedron. It is usually met with in micaceous, soft, friable scales, having a greyish black colour, a metallic lustre, an acrid hot taste, and a disagreeable odour somewhat similar to that of chlorine. It fuses at about 225° F., and at 347° is volatilized, though the vapour rises along with that of water at 212°. Iodine vapour has a beautiful violet colour, and a great specific gravity, namely 8·716, according to Dumas. Iodine requires 7000 times its weight of water to dissolve, but alcohol and æther are much better solvents for it. Its atomic weight is about 126.

Characteristic tests.—In the free state, iodine is distinguished from other bodies by its forming an intense blue colour with starch. So delicate is this test, that Stromeyer says, water which does not contain more than $\frac{1}{450000}$ of its weight of iodine, acquires a perceptibly blue tinge on the addition of starch. This blue colour is destroyed by heat, and, therefore, in testing for iodine, the liquids employed should be cold: an excess of alkali also destroys it, by forming two salts, an iodate and an iodide, but by supersaturating with acid, the colour is restored.

When iodine is in combination (as with oxygen, or with bases), starch will not recognize it: for example, if a little starch be added to a solution of iodic acid, no change of colour is observed; but if some deoxidating substance be now employed (such as sulphurous acid or morphia) the blue colour is immediately produced. On a future occasion I shall show you the advantage to be taken of this in medico-legal inquiries. If the iodine be combined with a base (as potassium, or sodium), chlorine or nitric acid must be employed to remove the latter; and the iodine being then set free, will re-act on the starch. If you wish to detect iodine in the urine of a patient, the mere addition of starch will not suffice: you must employ also nitric acid, or chlorine, to remove the base with which the iodine is combined.

The blue compound of iodine and starch is usually designated the *iodide of starch*, but Raspail objects to the term; as grains of starch consist of two parts—an *external envelope*, within which is a *soluble gummy substance*. Now the iodine, says Raspail, attaches itself to the envelope only, giving it a blue colour, just as it

gives a yellow colour to other organic textures.

Purity. — We are told that iodine is much adulterated, but I doubt it. There are two properties which will, in most instances, determine its purity—namely, its solubility in alcohol, and, when heated, its giving out a violet vapour, and leaving no residuum. Coal, plumbago, peroxide of manganese, sand, and charcoal (all of which, it is said, have been found in iodine), would be in this way readily distinguished. But Dr. O'Shaughnessy states, that he met with one specimen so artfully adulterated, that the foreign ingredients were at the same time soluble in alcohol and volatilized by heat. A little imposition may be, and indeed is, practised by some dealers in iodine, by selling it in the moist state. An ounce, if very moist, may contain one drachm, or perhaps even a drachm and a half, of water. The easiest way of detecting this, is by compression between folds of blotting paper.

Physiological effects. — (*a*) *On vegetables.*— It has been supposed that the aqueous solution of chlorine promotes germination by indirectly yielding oxygen to the seed, and it was suspected that iodine acted in a similar manner. Cantu, professor of chemistry in the University of Turin, put this to the test of experiment, and inferred that seeds watered by a solution of iodine germinate more quickly than those watered by a solution of chlorine; iodine was detected in the plant in the state of hydriodic acid. Reasons, which I need not here detail, lead me to doubt whether either chlorine or iodine have this property of accelerating germination. Indeed, Vogel says that iodine, so far from quickening, actually checks or stops the process.

(*b*) *On animals.*—Hitherto no examination has been made of the effects of iodine on the different classes of animals, for, with the exception of man, dogs are the only animals on which it has been tried. On them its operation appears to be that of an irritant and caustic poison, though not of a very energetic kind. Magendie threw a drachm of the tincture into the veins without any apparent effect: 72 grains of solid iodine were applied by Orfila to a wound on the back of a dog; local inflammation, but no other inconvenience, resulted. One or two drachms administered by the stomach caused vomiting, and when this was prevented by tying the œsophagus, ulceration of the alimentary canal, and death, took place.

(*c*) *On the human subject.*—The effects on the human subject vary according to the dose. When administered in *small or medicinal doses,* we sometimes obtain the palliation, or even the removal of disease, without the least alteration in the functions of the body. Thus I have seen it taken daily for twelve months with evident advantage, in diminishing the size of a chronic tumor, without my being able to perceive any functional effect. Sometimes it increases the appetite, from which circumstance it has been denominated a *tonic.* But the long-continued use of it is said to bring on a slow kind of gastro enteritis; an effect which has been occasionally observed, though I believe it to be rare, and only met with when the remedy has been incautiously administered. The action of it on the organs of secretion does not appear to be constant; at least I can in no other way account for the contradictory statements made. Some writers (Jörg and Lugol, for example) ascribe to it a *diuretic* operation, while others (for instance, Dr. Coindet) have not observed this effect. Coindet, Sablairoles, Brera, and Magendie, call it *emmenagogue.* Dr. Manson, however, does not believe that it has any specific influence over the uterus, but ascribes its emmenagogue effects to the stimulant and tonic action which it exercises over the whole body. In one case, he tells us iodine occasioned so much sickness and disorder of stomach, that the menstrual discharge was suppressed altogether. The pulse is not usually affected, but in some instances becomes accelerated. *Salivation* has occasionally taken place: Lugol says he has seen it in several cases, the patients being generally males. Two instances have been recently mentioned in the Medical Gazette, one by Mr. Winslow, and the other by Dr. Ely, (in the latter the patient was a female). *Absorption of the mammæ* has also been observed. Three cases of this are reported in Hufeland's Journal; one of which may be mentioned. A healthy girl, 20 years of age, took tincture of iodine during six months for a bronchocele, of which she became cured; but the breasts were observed to diminish in size, and notwithstanding she ceased to take the remedy, the wasting continued, so that at the end of two years not a vestige of the mammæ remained. Sometimes the breasts waste, though the bronchocele is undiminished: Reichenau, in Rust's Magazine, relates the case of a female, aged 26, whose breasts began to sink after she had employed iodine for four months, and within four weeks they almost wholly disappeared, yet her goitre remained unaffected. *Wasting of the testicles* is another effect attributed to iodine. Some have ascribed an *aphrodisiac* operation to it, but Kolley, a physician at Breslau, who took large quantities for a bronchocele, denies that it had any such operation on him.

Let us now examine the *poisonous effects of iodine.* Occasionally during the medicinal employment of iodine, various

symptoms (called by Dr Coindet, *iodic*) have occurred, which, by endangering the patient's safety, rendered the suspension of the medicine absolutely necessary. Violent vomiting and purging, with fever, palpitation, rapid and extreme emaciation, cramps, and small and frequent pulse, occasionally with dry cough, are the symptoms described, and which constitute the state denominated *iodism.* This condition, however, must be a very rare occurrence, for it is now hardly ever met with, notwithstanding the frequency and the freedom with which the remedy is employed. I have seen iodine administered in some hundreds of cases, and never met with one in which either iodism or wasting of the mammæ, or of the testicle, occurred. Dr. Coindet says that during this state bronchoceles are sometimes rapidly diminished. Swallowed *in large doses,* iodine operates as a powerful irritant, or rather corrosive, poison, giving rise to symptoms similar to those produced by the concentrated mineral acids.

Modus operandi.—That iodine becomes absorbed, when employed either externally or internally, we have indisputable evidence, by its detection not only in the blood, but in the secretions. Cantu has discovered it in the urine, sweat, saliva, milk, and blood. In all cases it is found in the state of iodide, or hydriodate. Bennerscheidt examined the serum of the blood of a patient who had employed for some time iodine ointment; but he could not detect any trace of iodine. In the crassamentum, however, he obtained evidence of its existence, by the blue tint communicated to starch.

A curious circumstance is mentioned by Dr. C. Vogel, in the fourteenth volume of Rust's Magazine, of a lady, 28 years of age, of a yellow complexion, who, from the internal employment of the tincture of iodine, became suddenly brown, besides suffering other morbid symptoms. After some days the skin had the appearance of having been smoked!

I have already mentioned that iodine is a local irritant: in further proof of this I may mention the following facts. When rubbed on the skin it causes redness, with desquamation of the cuticle. Inhaling the vapour (diluted with air) excites cough, and increased secretion of bronchial mucus.

It seems to have a specific effect on the nervous system; for Lugol says he has observed headache, drowsiness, a kind of intoxication, and even stupor, produced by the use of ioduretted baths. Moreover, when an over-dose of iodine has been taken, or as a consequence of the accumulated effect of small doses, cramps occur. The sexual system has been said to be specifically affected by iodine, on the ground that this medicine is an emmenagogue and aphrodisiac, and that it sometimes occasions wasting of the testicles and mammæ. The rapid emaciation occasionally caused by iodine, as well as the disappearance of visceral and glandular enlargements, have given rise to an opinion that iodine stimulates the lymphatic vessels and glands. Lugol, on the other hand, asserts, that instead of producing emacia ion, it encourages the growth and increase of size. I have never seen either emaciation, or the reverse state, caused by its exhibition.

Uses. — (*a*) In the first place, concerning the employment of iodine in *bronchocele.* Of all remedies yet proposed for bronchocele, this has been by far the most successful. Indeed, judging only from the many cases published illustrative of its efficacy, we should almost infer it was a sovereign remedy. However, it is to be recollected that of those who have written on the use of iodine in this complaint, some only have published a numerical list of their successful and unsuccessful cases. Bayle, in his "*Bibliothèque de Therapeutique,*" has given a summary from the writings of Coster, Irmenger, Baup, Manson, and others, from which it appears that of 364 cases treated by iodine, 264 were cured. I much regret, however, that my own experience does not accord with this statement. I have more frequently seen iodine fail than succeed in bronchocele; and I know many others whose experience has been similar. Dr. Bardsley cured only nine, and relieved six, out of thirty cases. I do not, however, doubt but that others have been more successful. To what circumstance, then, ought we to attribute this variable result? I think probably to a variation in the nature of the disease; for under the common head of bronchocele, goître, or Derbyshire neck, are included very different conditions of the thyroid gland. In some cases this organ, though enlarged, appears to be perfectly healthy in its structure. In others tumefaction of the gland takes place suddenly, and may even disappear as suddenly, from which it has been inferred the enlargement depends on an accumulation of blood in the vessels, and effusion of serum into its tissue. Coindet mentions a goître which was developed excessively during the first pregnancy of a young female: twelve hours after her accouchement it had entirely disappeared. Now I can readily believe that iodine may be more beneficial in either of these kinds of bronchocele, than in those accompanied with new or accidental productions; for some bronchoceles have within them osseous, cartilaginous, fibrous, or fluid deposits; and in some cases the thyroid gland has assumed a schirrous condition.

Now in any of these instances iodine is less likely to be serviceable. Kolley, the physician already mentioned, who was himself cured of a large goitre of ten years' standing, says, that for iodine to be useful, the bronchocele should be recent, and not painful to the touch; the swelling confined to the thyroid gland, and not of a schirrus nature, nor containing any stony or other concretions; and that the general health be not disordered.

If the swelling be tender to the touch, and have other marks of inflammation, let the usual local antiphlogistic measures precede the employment of iodine. You may administer this agent either internally or externally. Commonly, however, both modes of exhibition are resorted to. The most effectual method of employing iodine externally is that called *endermic*, already described; namely, to apply an ioduretted ointment (usually containing iodide of potassium) to the cutis vera, the epidermis being previously removed by a blister. But the *epidermic*, or *iatroleptic* method, is more usually followed—that is, the ioduretted ointment is rubbed into the affected part, without the epidermis being previously removed.

(*b.*) *Scrofula* is another disease for which iodine has been extensively used. Dr. Coindet was, I believe, the first to direct public attention to the efficacy of this method of treatment. Subsequently, Baup, Gimelle, Kolley, Sablairoles, Benaben, Callaway, and others, published cases illustrative of its beneficial effects. Dr. Manson deserves the credit of having first tried it on an extensive scale. He treated upwards of eighty cases of scrofula and scrofulous ophthalmia by the internal exhibition of iodine, sometimes combined with its external employment; and in a large proportion of cases, where the use of the medicine was persevered in, the disease was either cured or ameliorated, the general health being also improved. Three memoirs on the effects of iodine in scrofula have been subsequently published by Lugol, physician to the Hôpital St. Louis, serving to confirm the opinions already entertained of its efficacy. From the first memoir it appears, that in seventeen months—namely, from August, 1827, to December, 1828 — 109 scrofulous patients were treated by iodine only; and that of these, 36 were completely cured, and 30 relieved; in 4 cases the treatment was ineffectual, and 39 cases were under treatment at the time of the report made by Serres, Magendie, and Dumeril, to the Academie Royale des Sciences. In his illustrative cases, we find glandular swellings, scrofulous ophthalmia, abscesses, ulcers, and diseases of the bones, were beneficially treated by it. Lugol employs iodine internally and externally: for internal administration, he prefers iodine dissolved in water by means of iodide of potassium, given either in the form of *drops*, or largely diluted, under the form of what he calls *ioduretted mineral water*, presently to be described. His external treatment is of two kinds; one for the purpose of obtaining local effects only, the other for procuring constitutional or general effects. His local external treatment consists in employing ointments or solutions of iodine: the *ointments* are made either with iodine and iodide of potassium, or with the protiodide of mercury; the *solutions* are of iodine and iodide of potassium in water; and according to their strength are denominated caustic, rubefacient, or stimulant: the rubefacient solution is employed in making cataplasms and local baths. His external general treatment consists in the employment of *ioduretted baths*. Of these different preparations more will be said hereafter.

(*c.*) As an *emmenagogue*, iodine has been found a powerful agent by Coindet, Brera, Sablairoles, Magendie, and others. I need hardly say, that as suppression or retention of the catamenia may arise from many, and even opposite causes, neither iodine nor any other remedy can be expected invariably to succeed. Magendie states, that a young lady to whom he administered it, and whose propriety of conduct he had no reason to doubt, miscarried after using it for three weeks.

(*d.*) In various *chronic diseases of the viscera*—such as enlargements of the liver, spleen, uterus, and testicles—iodine has been beneficially employed.

(*e.*) In *gonorrhœa* and *leucorrhœa*, when the inflammatory symptoms have subsided.

(*f.*) In the *venereal disease*, advantage has been obtained in the treatment of buboes, diseased testicles, &c.

(*g.*) *Inhalations of the vapour* of iodine have been employed in chronic pulmonary complaints—as phthisis and chronic bronchitis. In a former lecture I described the method of using them.

(*h.*) *In several forms of disease of the nervous system*, as chorea and paralysis, this remedy has been beneficially employed by Dr. Manson, and others.

(*i.*) Several cases of *cancerous* diseases are reported to have been relieved by the use of iodine.

(*j.*) *As an antidote in cases of poisoning* by strychnia, brucia, or veratria, Donne has proposed to administer the tincture of iodine or of bromine, since the iodides of these alkalies are much less active (so, at least, he says) than the alkalies themselves.

(*k.*) Notwithstanding the numerous cases thus treated by iodine, many others are recorded, but of which I shall only no-

tice the following:—In old non-united fractures, to promote the deposition of ossific matter, it has been used by Mr. Cross and Mr. Buchanan; in various chronic skin diseases it has been employed with benefit, more particularly those occurring in scrofulous subjects; in gout and chronic rheumatism, &c. &c.

Administration.—Iodine is rarely used alone, but generally in combination with the iodide of potassium: formulæ for the conjoint exhibition of these I shall give when I have spoken of the iodide: at present I shall confine myself to those preparations into which iodine alone enters.

(*a.*) *Tincture of iodine.*—This is a simple solution of iodine in rectified spirit, and may be made of various strengths. In the Dublin Pharmacopœia, the proportions are two scruples of iodine to one ounce of spirit. It is, however, an objectionable preparation; for, in the first place, by keeping, part of the iodine is deposited in a crystalline form, so that the strength is apt to vary; secondly, it undergoes decomposition, especially when exposed to solar light; the iodine abstracts hydrogen from the spirit, and forms hydriodic acid, which, acting on some spirit, forms a little hydriodic æther. These are not the only objections: when added to water, the iodine is deposited in a solid state, and may thus irritate the stomach. It is used both externally and internally: externally it may be mixed with the soap liniment, and internally it is exhibited in doses of from five or six drops to half a drachm. Each drachm contains five grains of iodine.

(*b.*) *Ointment of iodine.*—This is composed, according to the Dublin Pharmacopœia, of a scruple of iodine to an ounce of lard. If this be too irritating, the quantity of lard must be increased. The colour of this compound is brown, but, by keeping, it becomes paler; and hence should always be made when wanted.

Antidotes.—In the event of poisoning by iodine, or its tincture, the first object is to evacuate the poison. For this purpose, assist the vomitings by the copious use of demulcent liquids—for example, milk, eggs beat up with water, barley water, &c. As starch and iodine unite to form a blue compound, Buchner thinks it probable that the action of the iodine on the animal organism is weakened by this combination; and he therefore advises the exhibition of starch or flour, boiled in water, to the consistence of a thin paste. Magnesia is also recommended. Opiates have been found useful. Of course you must combat the gastro-enteritis by the usual means.

Iodide of Potassium.

Synonymes.—This salt is sometimes called *ioduret of potassium*, and in commerce is usually denominated *hydriodate of potash*.

Preparation.—Various methods of procuring it have been proposed. I believe Dr. Turner's is the best; it is as follows:—Add to a hot solution of pure potash as much iodine as the liquid will dissolve, by which means a reddish-brown fluid is obtained. Then pass sulphuretted hydrogen through the liquid, until it becomes colourless. Apply a gentle heat, to expel any excess of sulphuretted hydrogen; filter to get rid of the free sulphur, and exactly neutralize the free acid present, with potash; then crystallize.

The *theory* of the process is this: when iodine is added to potash, two salts are generated; five atoms of potash being decomposed, the five of potassium contained therein unite with five of iodine, to form five of the iodide of potassium; while the five of oxygen of the decomposed potash, combining with one of iodine, generate one of iodic acid, which, with one of potash, forms iodate of potash. These changes are thus shewn:—

Reagents.	Results.
$6\ \dot{K}$	$5\ K\ I$
$6\ I$	$\dot{K}\ \overset{\cdot\cdot\cdot\cdot\cdot}{I}$

To decompose the iodate, sulphuretted hydrogen is passed through the solution; the oxygen of the iodate, with the hydrogen of the sulphuretted hydrogen, forms water; sulphur is deposited, and iodide of potassium is left in solution.

Reagents.	Results.
$\dot{K}\ \overset{\cdot\cdot\cdot\cdot\cdot}{I}$	$K\ I$
$6\ S\ H$	$6\ \dot{H}$
	$6\ S$

Dr. Turner has subsequently somewhat modified this process, though I think for commercial purposes that just described is the best. His present plan is the following:—"Add iodine to a hot solution of pure potassa until the alkali be neutralized, when iodide of potassium and iodate of potash are generated; evaporate to dryness, and expose the dry mass, in a platinum crucible, to a red heat, in order to decompose the iodate. The fused mass is then dissolved out by water, and crystallized." The objection to this process is, that if too much heat be employed, part of the iodide will be volatilized; and if too little, part of the iodate will not be decomposed.

Properties.—The fundamental form of the crystals of this salt is a cube; and it is usually met with either in this form or in

that of an octahedron. The salt is white and opaque, but by slow crystallization may be obtained perfectly transparent. It contains no water of crystallization, but some water mechanically lodged between the plates of the crystals, so that it decrepitates when heated. It is fusible, volatile, and very soluble in water. At 60°, 100 parts of water will dissolve about 140 parts of the salt: it is also readily soluble in alcohol. The taste of this salt is acrid and bitter.

Composition.

1 atom Iodine	126
1 atom Potassium	40
1 atom Iodide of Potassium	166

Characteristic tests.—The following tests will prove that this is an iodide, or hydriodate salt:—(*a*) With a solution of corrosive sublimate it occasions a vermillion-red precipitate of the biniodide of mercury, which is very soluble in excess of the iodide of potassium. (*b*) With the sugar of lead it forms a yellow precipitate of the iodide of lead. (*c*) With the nitrate of silver, a yellow precipitate of the iodide of silver. (*d*) With the protonitrate of mercury or calomel, a greenish-yellow precipitate of the protiodide of mercury. (*e*) On the addition of starch and a few drops of either nitric acid, or of a solution of chlorine, the blue iodide of starch is formed. (*f*) Chloride of platinum produces the iodide of platinum, which is of a brownish-red colour.

To prove that the base of the salt is potassium, or potash, the following tests may be employed:—(*a*) Perchloric acid occasions a white precipitate of perchlorate of potash, while the liquid becomes yellowish-brown. (*b*) An excess of a strong solution of tartaric acid also occasions a white precipitate of the bitartrate of potash. (*c*) Carbazotic acid forms yellow needle-like crystals of carbazotate of potash. (*d*) Soak a pack-thread in the solution of the iodide; then dip the wetted end into melted tallow, and apply it to the exterior cone of the flame of a candle: a pale violet tinge is given to the flame.

Adulteration and impurity.—Iodide of potassium is rarely met with in commerce quite pure, for we may generally detect minute portions of the carbonate of potash. In the year 1829, I published in the Medical and Physical Journal an analysis of a commercial salt, which contained 77 per cent. of the carbonate. Others also have detected the same impurity. Dr. Christison found 74·5 per cent., and Dr. O'Shaughnessy 64 per cent. It is reported that this very impure salt is furnished by a Scotch manufacturing company. The following anecdote will shew that the pure is more active than the impure salt. Soon after I had discovered the gross adulteration just mentioned, I was told by a surgeon that he had prescribed the iodide of potassium in very large doses, without any obvious effect; and that he suspected the salt employed was that which I had described. I suggested that he should call at the druggist's where the prescription was made up, and test the salt. He did so, and found, as we suspected, it was adulterated. He requested a purer kind to be procured, with which, in future, Mrs. ——'s prescriptions should be prepared. The next day he was sent for in great haste to see his patient, who, it was believed, had been poisoned. The fact was, she had taken her usual dose of medicine, but made with the pure salt, and which had occasioned violent vomiting; though no serious effects resulted.

There are several modes of detecting the impurities: one is to expose the suspected salt to a full red heat, when the iodide will be volatilized, leaving the impurities. The solubility of the iodide in alcohol will serve to recognise the presence of those salts (as the carbonate of potash), which are not soluble in this menstruum.

The easiest method of detecting a carbonate in the iodide, is to place a small portion of the latter in a glass, and add lime-water. If the salt be pure, no change will be observed; but if any carbonate be present, a precipitate is produced. Any sulphate present may be detected by chloride of barium, which will occasion a white precipitate (sulphate of barytes) insoluble in nitric acid. To detect the muriates or chlorides, add nitrate of silver, collect the precipitate, and digest in ammonia; filter the ammoniacal liquid, and add nitric acid; if any chloride be present, a white precipitate will be produced. The theory of this latter test is this: nitrate of silver precipitates the iodides, the chlorides, and the carbonates; ammonia will dissolve any chloride or carbonate of silver, but not the iodide. On the addition of nitric acid to the ammoniacal solution, the chloride is thrown down, while the carbonate is converted into the nitrate of silver.

Dr. Christison has described a commercial kind of iodide, which has an excess of iodine, and, therefore, whose colour is yellowish, and odour strong. I have met with this; indeed, here is a specimen which I purchased of a chemist in this city.

Physiological effects.—Devergie has shewn that this salt is an irritant and corrosive poison, like iodine. Four grains in solu-

tion, thrown into the jugular vein of a dog, produced tetanus and death in a minute and a half. Two drachms, dissolved in water, killed a dog in three days, by causing inflammation of the stomach. The constitutional or remote effects are probably similar to those of iodine, and hence this salt may be employed in the same cases.

Modus operandi. — It becomes absorbed, and may be detected in the urine. A very curious fact also has been noticed; it may be found in the urine several days after it has been administered.

Uses.—It is employed in the same cases as iodine. In the treatment of rheumatism, and also of some of the secondary symptoms of syphilis (as eruptions, nodes, &c.), it has been found useful; in the latter complaints, especially when combined with sarsaparilla.

Dose from one to five grains. However, I have seen it given in much larger doses than this.

Administration.—Iodide of potassium may be employed alone or in combination with iodine.

1st. *Preparations of the iodide of potassium only:* (*a.*) *Solution.*—As this salt is soluble both in water and spirit, either liquid may be employed for dissolving it, and the strength may be varied at pleasure. Magendie's formula is thirty-six grains of the iodide dissolved in an ounce of water; and he says two or three ounces may be taken daily, without the least injury. To this solution you may add, when necessary, the tincture of foxglove. In scrofulous affections, a good mode of exhibiting the iodide is by solution in some bitter infusion.

(*b*) *Ointment of the iodide of potassium.*—In the Dublin Pharmacopœia this is prepared by rubbing together a scruple of the iodide and an ounce of lard; but you will sometimes find it useful to increase the strength to a drachm. This ointment undergoes change by keeping, acquiring a yellow colour; most probably by the separation of a little free iodine.

2. *Preparations of iodide of potassium with iodine.*—These are more active, energetic, and useful preparations than those containing either iodine, or the iodide solely.

(*a*) *Solution of the ioduretted iodide of potassium.*—Iodine readily dissolves in water, holding in solution the iodide of potassium, and this is, I believe, the best mode of exhibiting it internally. The strength may be varied at pleasure. I shall describe several preparations, some for external, others for internal use.

Ioduretted mineral water. — This is a favourite preparation of Lugol's: he uses it of three different degrees of strength.

	No. 1.	No. 2.	No. 3.
℞ Iodine..........	gr. $\frac{3}{4}$.	gr. j.	gr. $1\frac{1}{4}$.
Iodide potassium	gr. $1\frac{1}{2}$.	gr. ij.	gr. ij$\frac{1}{2}$.
Distilled water ..	℥viij.	℥ viij.	℥ viij.

The solutions are yellowish or orange-coloured, and quite transparent. When sweetened it is readily drunk by children; but it must be recollected sugar occasions in a few hours decomposition, and, therefore, it should only be sweetened when about to be drunk. From six to eight ounces of this solution are given daily.

Ioduretted drops.—By this I mean a strong solution of iodine and iodide of potassium in water, so that a few drops may be taken for a dose. Here is a formula for making the liquid denominated *solution of ioduretted hydriodate of potash,* or *Coindet's solution.* Dissolve 36 grains of iodide of potassium in an ounce of distilled water, and then add 10 grains of iodine. The dose of this is from 10 to 50, or 60 minims, three or four times a day.

Caustic, rubefacient, or stimulant solutions.—These I have already alluded to as being employed by Lugol.

	Stimulating Washes. No. 1.	2.	3.	*Rubefacient Solution.*	*Caustic Solution.*
Iodine	gr. ij.	gr. iij.	gr. iv.	ʒ iv.	℥ j.
Iodide of potassium	gr. iv.	gr. vj.	gr. viij.	℥ j.	℥ j.
Distilled water	lb. j.	lb. j.	lb. j.	℥ vj.	℥ ij.

Lugol uses the stimulating washes in scrofulous ulcers, ophthalmia, fistulous abscesses, &c. When the scrofulous surfaces require stronger excitement than usual, he employs the rubefacient solution. In tubercular tumors which have obstinately resisted all other means of treatment, the rubefacient solution may be applied in admixture with *cataplasms* of linseed meal. To prepare the mixture, the poultice is first made in the ordinary manner; and when moderately cool, a sufficient quantity of the rubefacient liquid is poured on it with a wooden measure. The caustic solution is used for touching the eye-lids and nasal fossæ, to repress excessive granulations, &c. &c.

Ioduretted baths.—I have already alluded to these as being employed by Lugol, in the treatment of scrofula. They are to be made in wooden vessels.

IODURETTED BATHS FOR CHILDREN.

Age.	*Water.*	*Iodine.*	*Iodide of Potassium*
	(Quarts.)	(Troy Grains.)	(Troy Grains)
4 to 7	36	30 to 36	60 to 72
7 .. 11	75	48..60..72	96..120..144
11 .. 14	125	72..96	144..192

IODURETTED BATHS FOR ADULTS.

Degree.	*Water.*	*Iodine.*	*Iodide of Potassium.*
	(Quarts.)	(Drachms Troy.)	(Drachms Troy.)
No. 1.	200	2 to 2½	4 to 5
No. 2.	240	2..2½..3	4..5..6
No. 3.	300	3..3½	6..7

(*b*) *Iodurretted ointment.*—This may be made by adding from 10 to 20 grains of iodine to the ointment of the ioduret of potassium already described.

Antidote.—If a case of poisoning by the iodide of potassium should occur, the treatment should be the same as for iodine, already described.

OBSERVATIONS

ON

LOCAL AND GENERAL BLOOD-LETTING.

By M. Lisfranc.

Application of Leeches to various parts of the Body—Enumeration of situations in which their application is inconvenient and dangerous—Remarks on General Bleeding.

When leeches are applied to the face, we incur the risk of producing œdema, if not erysipelas. Thus in ophthalmia, when they are applied too near the middle angle of the eye, they often cause erysipelas, though they may have been employed in great numbers. Again, ought we to employ leeches to the inner surface of the eye-lids, as practised by some oculists? I never do, because I have seen very violent inflammation, and even gangrene, result from it. I am aware, indeed, that such effects are not common, but it is sufficient that they be possible, to justify my condemning the practice. In inflammations about the throat, leeches are usually applied about the neck, where they produce cicatrices, which in women are unseemly. Experience has convinced me that in inflammations either of the larynx or pharynx, leeches, when applied over the mastoid apophyses, and along the roots of the hair, are just as efficacious, while the marks are hid; on the other hand, in children, and in women with very fine skins, leeches may divide superficial veins. Phlebitis may thence result, the more dangerous in proportion as the veins are nearer the heart; hæmorrhage may also take place, and if the part be touched with nitrate of silver, to arrest it, the cauterization may possibly cause phlebitis. It may also happen, that in the absence of a medical man, the persons about the patient may be enabled to stop the bleeding, while if the leeches be placed over the mastoid apophyses, pressure, applied by the most inexperienced and unskilful, can scarcely fail to arrest the hæmorrhage. Leeches are often ordered to the epigastrium in children who have abdominal inflammations, and often they are placed where the skin is most moveable, as just below the edges of the ribs; and this mobility, which depends upon the act of respiration, may give rise to a prolonged hæmorrhage. The leeches ought to be applied lower down, at some distance from the sternum or ribs.

It is also to be remembered (and this holds good in all diseases) that leeches yield but little blood when applied to parts largely supplied with fat. Thus in a very fat person, forty leeches applied to the abdomen in a case of peritonitis, might only increase the determination of blood to the peritoneum, instead of relieving it. Under such circumstances we must at least double the number of leeches, or even pressure, or general bleeding. Never apply leeches where there are many nerves, because this excites pain. Thus with respect to the forearm, they ought to be applied to the dorsal rather than the palmar surface. Never apply leeches within the vagina, for the external veins of the part communicate with the internal, and leeches applied externally bleed as well. When applied to the anus, do not place them too near the margin of the rectum, because the bites, irritated by the intestinal secretions, sometimes ulcerate, and are then very difficult to heal. Never apply leeches to the scrotum, nor on the skin of the penis, for in these situations they give great pain, and sometimes lead to gangrene, an accident by which the reputation of the surgeon cannot fail to suffer.

Never put leeches on the back of the hand or foot, on account of the great number of tendons there; the lower part of the forearm or the upper part of the leg are to be prepared. You must not select the *lower* part of the leg, because if there happen to be a varicose vein in that situation, the leech-bites might lead to incurable ulceration. I never apply leeches to the mamma, because the skin is very fine and sensitive; and I have ascertained that they succeed just as well when applied at a distance.

Let us now pass in review some cases of disease.—Ought leeches to be applied to an inflamed part? We are often directed to do so in erysipelas. But in erysipelas, with phlyctenæ, they often lead to gangrene, and they have also the disadvantage of being very painful, the sensibility of the inflamed surface being in a state of exaltation; besides which, experience has shown that when the leeches are applied beyond the border of the erysipelas, they afford equal relief. Do not apply leeches to œdematous parts, nor to parts which are ecchymosed, though this be but slight, unless you would incur the risk of lighting up gangrenous inflammation. In white swellings, when the skin is adherent to the subjacent textures, never apply leeches upon the tumor, because, as in œdema, the vitality of the part is diminished, and may lead to similar results. Never apply leeches upon a bubo, nor at a less distance than four inches, otherwise you will often have the bites to be converted into syphilitic ulcers. Without attempting to explain this fact, I maintain it; and if I be told that it happens but rarely, I reply that the possibility of the occurrence warrants the rule I have laid down. Do not apply leeches on a fractured limb, for the pressure of the necessary apparatus upon the bites may cause eschars, or greatly retard their healing, and you will thus be greatly embarrassed in applying the splints, &c. Even in hernia there is inconvenience in applying leeches to the part itself. Thus in using the taxis we may be incommoded by the blood, which continuing to ooze, causes the fingers to slip over the skin, or if the bleeding has ceased, the taxis may reproduce it; nay, the manipulation being rendered painful by the bites, may even increase the inflammation of the hernia. The leeches ought, therefore, to be placed beyond the sphere of the rupture; and this method has another advantage also, if an operation becomes necessary within a few hours, or even days. Finally, never put leeches on a schirrous breast, particularly if the schirrus be very near the surface, or occupies the skin, for you may have the bites converted into ulcers of scirrhous character, extremely painful, and increasing the progress of the malady. Place your leeches round the schirrus, and you have nothing to fear. Beware, for the reasons already given, of applying leeches to the neck of the uterus, when it is scirrhous.

When you have reflected on these principles, they will, as it were, become proper to you, and you will not have to learn them hereafter at the risk of your own reputation, or at the expense of your patients. But at a time when so much difference of opinion exists regarding depletion, allow me to make a few practical remarks on this important subject. I would put you on your guard against that spirit of system which has always been so injurious to our science.

In inflammations, general bleeding is the more advantageous in proportion as the inflammation is more directly under the influence of the circulation in the great vessels: it is on this account that it has generally succeeded so well in inflammation of parenchymatous textures. It is not so when the inflammation has its seat in the membranous tissues, especially those which are under the influence of the capillary system: here leeching is usually more efficacious. When, however, we wish to produce a *derivation*, general bleeding is almost always to be preferred. In this case it ought to be small, and practised at a distance from the inflamed part. The proofs by which this derivative principle of bleeding is demonstrated are numerous: I shall state a few of them. Uterine pains may have resisted all our narcotic remedies, and yield to the abstraction of a cupful of blood from the arm. A woman has a violent menorrhagia: we take a little blood by venesection, and the discharge ceases. This effect is almost constant; and it was this which led me to employ those small bleedings which succeed so well in subinflammations of the uterus. What farther proves this derivative effect is, that when the patients dislike the phlebotomy so much as to compel us to substitute leeching, we fail in the great majority of cases.

This derivative mode of bleeding has still more marked effects when it is practised in inflammation with effusion; for not only does it arrest the inflammation, but it empties the veins, and thus renders them greedy of fluids, and thus promotes the absorption of what has been poured out,—as has been proved by the brilliant experiments of M. Magendie.

When applied in large number, leeches unload the tissues, and act as antiphlogistics; but in small number, they produce congestion of the part to which they are applied. The proofs of this are very numerous. 1st. To restore the menstrual discharge, leeches are applied to the

ankles or to the thighs, but not in great number; six or eight at most are used, because it is well known that a larger number would not in general produce the desired effect. 2d. Leeches in small number are much more apt to cause erysipelas than when many of them are used. 3d. Applied in small number to a white swelling in the acute stage, they for the most part prove injurious; but when the disease is chronic, they frequently succeed. 4th. A few leeches aggravate a severe phlegmasia; a large number combat it successfully.

An important fact to be aware of is, that, other circumstances being alike, the action of leeches is not borne so well during health as during the presence of inflammation. This is matter of experience. It may thus be explained. During the state of health, nothing stimulates the vital properties above the degree which is required for the performance of the functions; but during the presence of phlegmasia, there is an unwonted stimulus, which exalts the vital properties, and makes them offer greater resistance to any thing tending to overcome them. When the inflammation subsides, this stimulus which acts upon the vital properties diminishes, and we then see that very small abstractions of blood weaken the patient very much,—a very important fact in a practical point of view.

Some very extraordinary phenomena take place in particular idiosyncrasies. Some patients cannot apply eight or ten leeches without being weak and exhausted during many days; others, on the contrary, bear large and repeated bleedings perfectly well. An individual, labouring under tetanus in this hospital, was bled nineteen times in as many days: the first bleeding amounted to three palettes; the others to one palette each. The patient, within the period specified, had also applied to him 740 leeches! But we could not push the depletion thus far without taking into consideration the state of the circulating system, and the various symptoms which indicated the absence of debility. It is proper to ascertain from patients how they have borne depletion on former occasions. Here is another precept for us to follow: we have recourse to a bleeding, and before we repeat it we must examine the pulse, the complexion, the muscular strength, &c., and thus by caution avoid exceeding what the patient can safely bear.

Depletions are also borne better or worse according to the seat of the disease. This point is of such fundamental importance, that it is necessary to enter into some detail regarding it. In inflammation from wounds, not implicating the viscera, we may proceed a great length in the abstraction of blood. Here there is no internal cause diminishing the vital energies; and bleeding has the effect of preventing or removing inflammation, when it is practised with sufficient energy: we thus often prevent suppuration and all its consequences. But there is a very important indication to observe, namely, that we discontinue to abstract blood whenever suppuration takes place: by neglecting this, we are exposed to a very great risk of causing the reabsorption of the pus, which is often followed by a speedy and fatal termination. I may remark, in passing, that it is also proper at the same time to discontinue the rigid diet, as this also favours absorption. In inflammation of the lungs, and particularly in wounds of these organs, it is a well-known fact that bleedings may be employed to a great extent, unless there be tubercles. Military surgeons have carried these depletions very far, but their works are too little read. In the army, I used, in cases of this kind, to practise first one large bleeding, and then, during the day, it might be ten different times, have taken two or three table-spoonsful of blood, thus arresting the difficulty of breathing when it has threatened to suffocate the patient. By this method almost all those cases, usually so fatal, recovered.

In inflammation of the brain or its membranes, venesection to a large extent has been recommended; but it is very necessary to distinguish the following case. In many persons bleeding augments the nervous disturbance, and in such cases it is necessary to renounce this remedy, lest we destroy altogether the equilibrium that exists between the sanguiferous and nervous systems. In inflammations of the abdominal viscera, you will do a great deal of harm if you carry your depletions as far as in thoracic or cerebral affections, and for this reason—these inflammations scarcely last for a few hours before the patients are brought into a state of great prostration: there is a peculiar influence exercised over the whole system, probably depending upon the abdomen being the seat of the digestive function. No inflammation so frequently assumes the adynamic form, as those which have their seat in the abdomen. Now, whenever an abdominal inflammation is of some days' standing, and proves obstinate, we must carry our bleedings to a much less extent than in the inflammation of other parts*.

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* The above, as well as the lecture by M. Lisfranc in our last number, has been taken from the *Gazette des Hôpitaux.*

OBSERVATIONS

ON

URINARY DEPOSITS.

By R. H. Brett, Esq. M.R.C.S.

[Continued from p. 800.]

Complex Organic Deposits.

Having noticed those deposits which are of a strictly organic nature, and which are to be found among the constituents of healthy urine, I shall now pass on to the consideration of the second division of the first class, and which is made up of those deposits consisting of an organic acid and alkaline, or earthy base, the constituents of which salts are to be found in healthy urine, although not under the same state of combination. This class of deposits differs materially from the free lithic acid just described, both physically as well as chemically. In the first place, they generally occur in much greater abundance than the free lithic acid, sometimes manifesting themselves very quickly,—as soon, indeed, as the urine cools; they do not, however, subside so completely or so speedily to the bottom of the containing vessel as the free lithic acid deposit does, so that the urine in many cases will remain turbid even after hours of repose, possessing an appearance not unlike thin pea-soup. It is to this class of deposits that the pink colouring matter attaches itself exclusively; so constant is this, that it may be assumed as a general rule, that wherever the pink colouring matter is found, there also the lithates will be found to exist. In many cases, however, the colour of these deposits is of a pale fawn, with or without a very slight pink tinge. The decidedly pink-coloured urinary deposits is by no means a rare occurrence, as stated by Dr. Prout; indeed, from the opportunities I have had of examining urinary sediments, during some three or four years, throughout the wards of a large hospital, I should affirm that they constitute by far the majority of urinary deposits. When this class of sediments is separated by filtration from the urine, the mass remaining on the filter assumes more or less of an argillaceous appearance, without being granular, as is observed with the free lithic acid deposit; it is quite smooth to the touch, and the particles composing it being in a state of exceeding fineness, their cohesion is more marked than in the lithic acid deposit. If the mass be washed for a considerable time on the filter, it will be found to lose materially in weight; whereas free lithic acid, as has been before noticed, is not similarly circumstanced. When suffered to dry upon the filter, the whole can be readily removed *en masse*, without the particles of which it is composed being separated. In a great number of the deposits of this class which I have examined, the particles have been found cemented together by means of a delicate filamentous structure, similar in appearance to very fine short hairs; these, by their interlacements, appear to form a sort of net-work, upon which the salts become deposited: by boiling in water, these organic salts are dissolved out, and the filamentous structure becomes more apparent as it is seen suspended through the aqueous fluid. I have once or twice observed the same capilliform substance in deposits consisting exclusively of the earthy phosphates. This circumstance has not been noticed by the majority of those who have treated of the urine or its sediments. M. Magendie, however, in a treatise published by him some years since, On the Causes, Symptoms, and Treatment of Gravel, remarks, cap. ii. p. 8:—"Le plus souvent les graviers sont isolés et sortent successivement. Dans un petit nombres de circonstances, je les ai vus former des espèces de grappes, étant attachés les uns aux antres par des poils." In the instances, however, which he relates, the deposit consisted of small calculi rather than a sediment, properly so called, the portions being most of them as large as a good-sized pin's head, and some of them even considerably larger. M. Magendie further observes, that he has occasionally found this delicate capillaceous structure mixed with phosphate of lime and magnesia, but does not allude to any instances of its occurrence in the organic saline deposits.

When urine containing this form of deposit suspended through it is subjected to heat, it becomes generally completely, though sometimes only partially clear, from the ready solubility of the lithates in the hot fluid; and this will be observed frequently even before

the boiling point is reached. No other deposit met with in the urine exhibits the same phenomenon. It must, however, be observed, that if the deposit be previously dried on a filter, its solubility both in the urine, as also in water, will be considerably diminished. A small quantity of free lithic acid is occasionally mixed up with this form of sediment, which will remain unacted upon by water when the dried deposit is boiled in that fluid, although readily dissolved, together with its accompanying lithates, when heat is applied to urine containing these ingredients in a state of suspension.

When this organic saline deposit is ignited, it blackens, and is in great measure dissipated, yielding an ash of a white colour, and generally of larger bulk than is observed in the case of the free lithic acid. This residue commonly effervesces with acids, and is alkaline in its reaction. Upon further examination it will be found to consist of carbonate of soda, with or without a little carbonate of lime and earthy phosphate. Both the carbonates of soda and lime result from the decomposition of organic salts during ignition, these salts being the urates of soda and lime. Neither of these salts, however, are always present, the deposit sometimes consisting entirely of urate of ammonia, with, perhaps, a trace of earthy phosphate. In those deposits, however, which occur in the urine of gouty subjects, the urate of soda will generally be found to exist in pretty considerable quantities. It not unfrequently happens that if ignition be performed in such a way as to allow of the collection of the products of sublimation, that a portion of muriate of ammonia will be obtained; and this is somewhat remarkable, when we bear in mind the ready solubility of that salt in water. A solution of caustic potass, or soda, is capable of dissolving this class of sediments, with an abundant evolution of ammonia.

From the above statement it is clear that the organic saline sediments are frequently made up of several ingredients, not consisting of one, but of several binary compounds. The following are the salts found by analysis :—

Urate of ammonia.
——— soda.
——— lime.
Muriate of ammonia.
Free lithic acid (in very small quantity.)
Earthy phosphates and alkaline sulphates (mere traces.)
Colouring matter.

This class of sediments has been represented as never acquiring the deep red, or reddish brown colour, of the uncombined lithic acid; it occasionally happens, however, that the lithate deposit is so slightly imbued with colouring matter, as to assume an appearance so closely resembling the earthy phosphates, that mere inspection will not suffice to distinguish between them. In such cases the application of heat to the urine through which the sediment is diffused, will be found sufficient to clear up the difficulty. If the deposit be of the phosphatic kind, no appreciable alteration will follow; if, however, the lithates be present, the turbidity will entirely disappear.

It is not a common circumstance to find, where the deposit of the lithates prevails, much mucus precipitated also; this is, however, occasionally found to be the case, and it happens in such instances that the ordinary character of the lithates is masked by their mixture with the mucus, so as to lead to the idea that the deposit was phosphatic. This was well marked in a specimen of urine which I examined some short time since. The patient from whom it was procured was labouring under symptoms of an affection of the bladder.

The urine was acid in its reaction, and the deposit which took place from it very abundant. A portion of this urine was allowed to remain at rest for some time, and the supernatant fluid, which was still very turbid, poured off from the bulky precipitate occupying the lower part of the vessel. The decanted fluid was left at rest, and the remaining portion, containing the bulky deposit, thrown upon a filter. The filtration went on slowly: the clear fluid which ultimately passed through was found to be highly albuminous; the residue on the filter, when dry, formed a thick varnish, which, when moistened with water, again assumed the pulpy semi-transparent character which it possessed before drying. It contained a minute portion of urate of ammonia, or uric acid, mixed up with it, but mainly consisted of mucus tinged by

the colouring matter of the urine. That portion of turbid urine which had been decanted, yielded, after some time, an abundant deposit of a pale salmon-colour, and which, upon examination, was found to consist chiefly of urate of ammonia, with a portion of pink colouring matter, and a little mucus. This specimen of urine was the more interesting because it was albuminous, a condition of that secretion very rarely met in conjunction with the deposition of the pink-coloured lithates.

Complex Inorganic Sediments.

I now come to speak of the last division of that class of deposits, the constituents of which exist in healthy urine, viz. the combination of an inorganic acid with earthy or alkaline bases. This form of sediment is much less frequently met with than the preceding. The urine from which it takes place is for the most part pale in colour, but not invariably so, as stated by Dr. Prout. I have examined more than one specimen of urine depositing the mixed phosphates, the colour of which was quite equal to that of healthy urine. In such cases, however, the deposits are not generally found to possess the ordinary white colour of the phosphates, but have an appearance in no respect different from that of the very pale lithates. The circumstance of the phosphatic deposits most frequently occurring of a white colour, seems to depend more upon a deficiency of colouring matter in the urine than upon any want of affinity between the earthy salt and the urinary colouring principle.

The sediments in question are generally found in smaller quantities than those consisting of the lithate; it sometimes happens, nevertheless, that the quantity is very considerable; and the appearance of the deposit will frequently be found to differ accordingly as it may occur in larger or smaller proportion. When in small quantity it usually subsides readily to the bottom of the containing vessel, is generally of a white colour, and entirely devoid of crystalline character. When collected upon a filter, washed, and dried, it becomes pulverulent, earthy in appearance, and soft to the feel, and is sometimes found mixed with small hair-like substances, analogous to those observed to cement in some instances the particles of the lithates together. This phenomenon, however, I have observed only when the phosphatic deposits have been coloured.

When, on the other hand, the bulk of the sediment is considerable, it will frequently assume a mucilaginous character; and in some cases is so tenacious as to be without difficulty lengthened out into threads when a stirring rod plunged into it is drawn up along the sides of the vessel. This might lead to the supposition that the deposited mass mainly consisted of mucus; but this is not the case, for when dried upon a filter the whole becomes soft and pulverulent. It must be remembered, however, that mucus is not at all unfrequently associated with the phosphate of lime deposit, and may be separated from the latter by the agency of diluted muriatic or nitric acid, which will dissolve out the earthy salt, leaving the organic matter unacted upon.

These are the physical characters of the phosphate of lime deposit; it remains now to be shewn in what way it may be distinguished from all other urinary sediments by its chemical habitudes.

Tests of the Phosphatic Deposits.

Whenever a white or pale fawn-coloured deposit takes place from the urine, insoluble in caustic potass or soda, and completely soluble in muriatic acid, or nearly so, without effervescence, that deposit is of the phosphatic kind. If, in addition to this, we find that the muriatic acid solution, when treated with just sufficient ammonia to render its reaction only very slightly acid, gives an abundant precipitate with oxalate of ammonia, and the clear fluid separated from the precipitate by filtration is not affected by the addition of caustic ammonia in excess, the salt is exclusively phosphate of lime. If a portion of the dry deposit in question be ignited, it blackens, and yields after a continuance of the heat a white ash, losing, however, very little in weight. This residue is not alkaline in its reaction, and does not effervesce with acids.

These characters, physical and chemical, will be found quite adequate to the distinction of the phosphate of lime from mucus, on the one hand, or the exceedingly pale lithates, on the other.

It has been stated by authors that the urine in this particular form of deposit is always found alkaline; and this alkalescency has been considered as essential to the production of the precipitate; for it is argued that the free phosphoric acid, which held the earthy phosphate in solution, being neutralized, the latter becomes thrown down. That this statement is correct in many, if not in the majority of cases, appears highly probable, more especially when we take into notice the extreme proneness of this description of urine to undergo decomposition, by which large proportions of carbonate of ammonia are formed; but still instances do occur in which, although a deposit of earthy phosphates is formed, the urine is itself, nevertheless, acid in its reaction. This I have observed in more than one instance, especially where the phosphatic salt has been coloured like the palest lithates; also where the precipitate has chiefly consisted of the triple phosphate.

In these cases it is manifest that the deposition of the phosphatic salts cannot depend upon the neutralization of the acid which acted as their solvent, for otherwise there would be found an alkaline, and not an acid, condition of the urine. The following question suggested itself. Is carbonic acid gas the solvent in such cases, and is it by the evolution of this gaseous matter that the earthy salts are deposited? There has, it is true, been much discrepancy of opinion among experimentalists, concerning the existence or non-existence of this acid in healthy urine. The pneumatic experiments of Dr. Marcet would lead us to decide the point in the affirmative; but then the results of the experiments of Berzelius and Woehler, great authorities, seem to negative the assertion. The experiments of Proust and Brande are in favour of its existence, and Dr. Prout considers it highly probable. I shall now proceed to detail some experiments which I have instituted on this subject. I have endeavoured, as it will be seen, to avoid all those sources of fallacy which have been objected to that mode of investigating the matter which I have adopted.

1. About four ounces of healthy urine just passed, and reddening litmus paper, were placed in a glass flask having a long neck. A tube, bent twice at right angles, was then inserted, the free end dipping into lime-water. After a short time, and before active ebullition had commenced, a copious turbidity was produced in the lime-water, gaseous matter continually passing over.

It has been objected to this experiment, that carbonate of ammonia is formed during the process of boiling, and, being carried over, renders the lime-water turbid. The following experiment was then made, in order to test the validity of this objection:—

2. The same quantity of recently-voided healthy urine was treated as in the first experiment; but the free end of the conducting tube, instead of passing into lime-water, was dipped into pure water; in which fluid a portion of litmus and turmeric paper were immersed. After a short time, copious bubbles of gas came over, and after being allowed to pass into the water for more than a quarter of an hour, it was found that the litmus paper was slightly reddened, whereas the turmeric paper was not in the slightest degree altered. It was therefore clear, from this experiment, that no carbonate of ammonia could have passed over, otherwise the turmeric paper would have been reddened.

3. The gaseous matters, instead of being passed into lime-water, or pure water, was collected over mercury in two glass tubes; a piece of caustic potass was passed up into one of the tubes, and after some time it was found that nearly the whole of the gaseous matter had been absorbed. The other tube had lime-water poured into it: after agitation, the lime-water was rendered exceedingly turbid. This experiment was performed in conjunction with my friend Mr. Bird.

I think, then, it may be fairly concluded, from the result of these several experiments, that carbonic acid does exist in healthy urine. It now remains to be shewn by experiment that this acid is capable of acting as a solvent over the urinary earthy phosphatic salts. I have observed myself, and it has often been noticed by others, that, upon applying heat to certain specimens of urine, a deposit has ensued; which, upon examination, has turned out to be phosphate of lime. This has been said to result from the urea undergoing decomposition; and in those cases where the urine is already strongly alkaline, and gives other evidences of a great proneness to undergo decomposition, this explanation may hold good; having, how-

ever, in several instances found the urine yielding the earthy phosphate by boiling, acid both before and after ebullition, I was led to conclude that such a deposit could not result from the decomposition of urea or other organic matter contained in the secretion, unless, indeed, it be supposed that only just enough urea or animal matter had undergone decomposition to yield sufficient ammoniacal salt to neutralize the free phosphoric acid, and no more; a circumstance not in the slightest degree probable, seeing that the ebullition had been continued for some time even after the precipitate had come down.

Is carbonic acid capable of dissolving phosphate of lime by being passed through that salt suspended in water? A very diluted solution of muriate of lime was rendered turbid by the addition of an excess of a solution of phosphate of soda; carbonic acid gas was then passed through this mixture for some time: the whole of the phosphate of lime was ultimately dissolved, the fluid becoming quite clear. The fluid just reddened litmus paper. Upon boiling, the phosphate of lime again came down, the fluid no longer possessing an acid reaction. The precipitate which took place from this fluid was collected, and found to be completely soluble in diluted muriatic acid, without effervescence. It is therefore evident that carbonic acid is capable of dissolving recently precipitated phosphate of lime, and that the evolution of the gas again causes the salt to fall. The same experiments were then tried with the ammoniaco-magnesian phosphate with precisely similar results. The following experiments were then made upon different specimens of urine:—

(A.) A portion of healthy urine, just voided, was rendered slightly alkaline by caustic potass; a deposit took place of the phosphate of lime; a current of carbonic acid was then passed through the mixture; the phosphate of lime was dissolved; the application of heat again caused it to come down: a drop of muriatic acid entirely dissolved it.

(B.) About 4 oz. of urine, obtained from a patient with chorea, was filtered, in order to separate some of the pale lithates which had subsided: the clear fluid was placed in a flask supplied with a conducting tube, as in the experiments before mentioned; the gaseous matters were driven over by heat into lime-water; the urine gave off copious bubbles of gas, which rendered the lime-water turbid, and at the same time the clear urine became turbid, and ultimately let fall a copious deposit, which, when collected, was found to be phosphate of lime. This urine was acid both before and after ebullition.

(C.) Another portion of the same urine was treated in the same way, excepting that the gaseous matters were conducted into pure water, into which litmus and turmeric paper were placed. The gas evolved slightly reddened the former, but did not at all affect the latter.

(D.) Some of this urine, which had been rendered turbid by boiling, was allowed to cool, and then treated with a current of carbonic acid. The whole was rendered clear, becoming again turbid by boiling.

(E.) Some of this urine, rendered turbid by boiling, was filtered. Equal bulks of the filtered fluid and fresh healthy urine were treated with precisely the same proportions of caustic ammonia: the precipitate which ensued from the diseased was afterwards found to be more considerable than that from the healthy urine.

These experiments have been repeated upon several specimens of urine, of a similar character to that just spoken of; the results, with very little differences in the quantity of phosphate of lime precipitated by ammonia, were the same. We may therefore conclude that certain specimens of urine yielding the pale lithates, contain a considerable quantity of phosphate of lime which is held in solution by carbonic acid, and is thrown down by heat, in consequence of the evolution of that gas, and not from the formation of carbonate of ammonia resulting from the decomposition of urea or other animal matter. It may also be concluded, that, in some cases, the quantity of phosphate of lime which remains in solution after the application of heat, is considerably greater than we meet with in healthy urine; and there can be little doubt but that the total quantity of phosphate of lime contained in urine of this description is considerably greater than what we meet with in an equal bulk of healthy urine.

It now remained to institute some experiments upon urine actually depositing the phosphates upon standing. The urine experimented upon was obtained from a patient who voided it

in my presence, at an early hour in the morning; the same individual having passed urine the previous night, which was found exceedingly turbid from the phosphates the next morning, and also in a slight degree alkaline. The morning urine was of specific gravity 1020, tolerably clear, containing in suspension a mucous cloud only: after standing twenty-four hours, the mucous cloud deposited, the supernatant fluid remaining clear; small transparent glistening crystals of the triple phosphate appeared here and there diffused through the urine, some adhering to the sides of the glass vessel and others floating on the surface of the fluid. This urine, be it observed, was distinctly acid when passed, remaining so for twenty-four hours, or more.

1. A portion of this urine, about nine hours after it had been passed, was heated in a flask; the gaseous matters being collected over mercury in two glass tubes, caustic potass was passed up into one, by which means considerable absorption ensued; to the other a little lime-water was added; considerable turbidity was produced by agitation.

2. This urine exerted an acid reaction even after boiling for nearly an hour and a half, and the gaseous matter which came off reddened litmus paper.

3. Two equal bulks of this urine, before any seperation of triple phosphate had taken place, were placed side by side, and through one a current of carbonic acid was passed. After standing for twenty-four hours, a deposition of the phosphatic salt was observed in that specimen which had not been treated with carbonic acid gas, but none in the other.

4. This urine was boiled directly after it was passed; an abundant phosphatic deposit ensued, the fluid still exerting an acid reaction.

5. This urine, twenty-four hours after it had been passed, and after a deposition had taken place, gave off carbonic acid by heat; the gaseous matters, conducted into water, did not redden turmeric paper.

These facts, then, relative to the presence of carbonic acid in urine, and the solvent power it possesses over the earthy phosphates, being established, serve to explain the otherwise anomalous circumstance of the phosphate of lime being occasionally precipitated from the urine by the mere agency of heat, without the necessary decomposition of any organic matter capable of yielding an ammoniacal salt; and also invalidates the statement that an alkaline condition of the urine is always co-existent with the deposition of the phosphatic salt. It is, however, maintained, that even in those cases where the urine depositing the phosphates remains acid for some time, that all the phosphatic deposition which may ultimately take place is depending solely upon the evolution of carbonic acid; for urine of this description is prone to undergo decomposition, by which means ammonia is formed, which will, by neutralizing all the free acids existing in the urine, cause a further precipitate of the phosphates; for all these latter salts, it must be remembered, cannot be separated from such urine by mere boiling; for after that process has been completed, the clear filtered urine will always yield a larger or smaller proportion of the earthy salts by the addition of ammonia; but so long as the urine remains acid, there can be no doubt but that the deposit results from the escape of carbonic acid. Whether the carbonic acid, in these cases, is separated from the blood in the kidneys, or is secreted in the bladder or urinary canals, is a matter of uncertainty. I should incline, however, to the latter opinion, from analogous reasoning with respect to the known functions of the stomach and intestinal tube; for it is an acknowledged fact that carbonic acid is secreted in those cavities: thus Chevreul found, in the stomach of a man just executed, 14 per cent. of carbonic acid, besides other gaseous matters. We may therefore, I think, fairly conclude, that in cases of the phosphatic deposits, the urine may be either alkaline or acid, most frequently perhaps in the former condition even when just voided; that when the secretion is alkaline from the first, it is highly probable that great proneness to decomposition exists, so that a very short delay of the secretion in the bladder will suffice for producing this change in it; that whenever the urine in such cases is turbid at the moment of its emission, it will be found alkaline. In those cases where the phosphates are thrown down, the urine remaining acid, it will generally be found clear when passed; or if opa-

lescent, from suspended mucus, carbonic acid in considerable quantities will, I believe, be always found to be present; that so long as the urine continues acid, the deposit is entirely owing to the escape of carbonic acid; and although such urine ultimately becomes alkaline, by which means a further quantity of the phosphates become separated, still that it is much less prone to undergo decomposition than in the cases where it is found alkaline from the first. It is not improbable either, that in these particular cases the phosphatic salts are secreted in the kidney, without a corresponding proportion of free phosphoric acid to hold them in solution; that they pass suspended in the urine into the bladder, and there become dissolved by the carbonic acid secreted in the cavity of that viscus.

The inferences I have drawn from the foregoing experiments, seem to be borne out, to a certain extent, by the effects which have been observed to result from the administration of certain mineral waters in cases of phosphatic disease. Thus Magendie, in his work on Calculous Disorders, before cited, speaks of the advantage accruing to individuals suffering from the *gravelle blanche*, as he denominates it, from the employment of the waters of Seltz, Contrexeville, Bains, and Victry. Now all these waters contain considerable quantities of carbonic acid, besides certain alkaline and earthy carbonates: according to Berzelius, the quantity of carbonic acid in the Seltz water is equal to its own volume. Now, as neither the carbonated or bi-carbonated alkalies exert any solvent action over phosphate of lime, and as free carbonic acid has been proved, by direct experiment, capable of dissolving it, I conceive that the beneficial results which have been obtained from the use of the waters in question were mainly attributable to the agency of the large quantity of carbonic acid gas contained in them. It would not therefore be injudicious, perhaps, to employ water highly charged with carbonic acid, in those cases of the phosphatic deposit where the mineral acids had either failed in their action, or their administration was not deemed advisable.

Ammoniaco-magnesian Phosphate.

I shall now conclude what I have to say respecting those deposits the constituents of which are to be found in healthy urine, by a brief notice of that combination of an inorganic acid with an earthy and alkaline base, called the ammoniaco-magnesian phosphate, and a few words on the carbonate of lime deposit. The triple compound of phosphoric acid, magnesia, and ammonia, very rarely occurs as a pure uncombined sediment, nor is it ever found *per se* in urinary calculi. Doubtless in all cases where the sediment possesses a marked crystalline form, it mainly consists of the triple salt, to which, indeed, it owes its crystalline structure; but at the same time larger or smaller quantities of phosphate of lime will be found to be present, upon applying the proper tests for detecting the presence of that salt. Depositions of the triple phosphate, the particles of which are sufficiently finely divided to constitute what is commonly understood by the term urinary sediment, are by no means common. Magendie observes, "Je n'ai jamais rencontré cette espèce de gravelle que sous l'etat de graviers plus ou moins volumineux; dans certains cas lisse à leur surface, et approchant de la forme d'olive ou de pistache." This peculiar salt most commonly separates from the urine in the form of a thin film, which floats at first upon the surface of the urine, but which gradually increasing in density by fresh additions to its bulk, falls to the bottom of the fluid, and is not unfrequently found adhering to the sides of the containing vessel in the form of small white transparent glistening crystals, which are never found tinged with the colouring matter of the urine, which circumstance would serve to distinguish them from the crystalline deposit of lithic acid, which, as I have already had occasion to observe, is always more or less imbued with the reddish brown colouring matter of the urine; neither does this form of sediment ever become associated with the pink colouring matter of the urine. It sometimes happens that deposits mainly consisting of the triple salt, do not appear to possess any crystalline structure until after they have been collected on a filter and dried, when the crystalline character is readily recognized, especially by the aid of a lens.

If the delicate pellicle which forms on the surface of urine yielding deposits of the triple salt be placed on a filter, washed, and dried, the quantity is found

to be so inconsiderable that you can scarcely, and in some cases cannot at all, observe its characteristic crystalline structure, in consequence of its having been imbibed by the tissue of the bibulous paper: the better plan in such a case is to employ acetic, or very diluted muriatic acid, for the last washings, which should be afterwards examined as follows:—

Evaporate the acidulous solution of the triple phosphate to a few drops; place the fluid on a glass plate, in the field of a microscope of ordinary power; take a glass rod charged with a drop or two of a solution of caustic ammonia, and immerse the end in the acid fluid. After a short time numerous crystals of the triple phosphate will be observed to deposit, observing somewhat of a stellated arrangement, each crystal being a prism: this is exceedingly characteristic of the salt in question. This microscopic mode of proving the existence of the triple phosphate was, I believe, first introduced by Wollaston. Dr. Yelloly has also employed it very successfully in the analysis of calculi containing portions of the triple salt. The crystalline film just spoken of will, I believe, invariably be found to consist of the pure ammoniaco-magnesian phosphate, without any admixture with phosphate of lime. Not so, however, the deposit found at the bottom of the containing vessel; this, I believe, always contains a small proportion of phosphate of lime; for when the muriatic acid solution of it is barely neutralized with ammonia, oxalate of ammonia always causes a turbidity, indicating the presence of lime.

Those remarks which have already been made relative to the action of carbonic acid on phosphate of lime, will apply equally well to the triple salt just considered, as determined by experiment.

Carbonate of Lime.

I believe there are no cases on record in which the carbonate of lime has been found, in the human subject at least, assuming the character of a pulverulent amorphous deposit; it has been observed, and that only in a very few instances, in the form of small calculous concretions, of a convenient form and size to admit of their egress by the urinary channels. Urine that has been long kept, and undergone more or less of decomposition, has been observed by Proust, the Spanish chemist, to yield small crystals of carbonate of lime, which were found adhering to the surface of the containing vessels; they most probably resulted from the decomposition of urate of lime, which exists in healthy urine. I once had an opportunity of examining a urinary human calculus, of the size of a small hen's egg, the external crust of which consisted of carbonate of lime. This salt is readily enough distinguished from all other urinary saline compounds, by its white colour, solubility in weak acids, such as the acetic or diluted muriatic acid, with effervescence; and its copious precipitation from its acid solutions by means of oxalate of ammonia, the acid fluid being previously neutralized by ammonia.

[To be continued.]

CASE OF ANEURISMAL TUMOR IN THE ORBIT,

FOR WHICH THE COMMON CAROTID ARTERY WAS TIED.

To the Editor of the Medical Gazette.

Sir,

I should feel obliged by the insertion of the following case in your journal: if not too long, I think you will consider it sufficiently interesting. For the sake of brevity, having omitted almost all details of treatment, (as I am not aware that it differed from that usually pursued in similar cases,) and having confined myself to an account of only the principal occurrences, I am not aware that the history could be advantageously curtailed.

I have the honour to be, sir,
Your obedient servant,
George Busk,
Surgeon to the Seamen's Hospital.

S. H. S. Dreadnought,
Feb. 19, 1836.

Richard Simmons, aged 20, a seaman, was admitted July 13, 1835, labouring under the usual symptoms of concussion of the brain, with very considerable hæmorrhage from the right ear, and a small wound behind the left. It was stated that he had received a severe blow on the right side of the

head, from the gaff of the vessel to which he belonged, by which he was rendered immediately insensible; and he was nearly so on admission, but could with difficulty be roused. At first he was very pale and cold, but in an hour or two rallied. The hæmorrhage from the right ear continued all night. The following day he was quite sensible, but appeared dull, not complaining of any pain. Pupils natural.

On the 15th he was still dull, and completely deaf in the right ear; the eye-lids and integuments around the left orbit were swollen, apparently from serous effusion; they were not discoloured or painful. The pupil of the left eye was dilated and fixed; vision, however, was unimpaired, but he was unable to move the globe of the eye in any direction, and had slight paralysis of the facial muscles on the left side.

On the 18th, some increase of inflammatory symptoms required attention, but was readily subdued. The pupil was observed to have become irregular, the irregularity consisting in an elongation downwards, and vision was not quite clear; there was occasionally a very partial rotatory movement of the globe, as if caused by the apparently involuntary action of the superior oblique muscle. He had some headache, confined principally to the left side. On the 21st his mouth became slightly affected by mercury. On the 24th, the headache was very trifling, but the left side of the face had become numb, and felt very "uncomfortable," and he complained of great dryness in the mouth, although his tongue was quite moist; there was also considerable purulent discharge from the right ear. On the 25th, the integuments of the left side of the face and head extending to the vertex, were extremely tender to the touch, but without any morbid appearance. The paralysis continued unaltered. On the 28th, the conjunctiva of the left eye was much inflamed and swollen from œdematous effusion; and on the 31st, purulent matter was deposited between the laminæ of the cornea, at the lower part, with nebular opacity, but the eye was much less painful, and headache gone. His health was now in most respects restored, and functions properly performed.

He began soon to complain of curious noises in the right ear, from which the discharge continued, and he was very deaf. The eye was not painful, but the onyx was increased. In the beginning of September the paralysis and loss of sensibility were complete. The anterior laminæ of the cornea had given way, and a deep ulcer occupied the site of the abscess, of a healthy character. No change of importance took place until about the 20th of November, when he became affected with small-pox, and was sent to the Small-pox Hospital, whence he returned on the 1st of December. On his re-admission, the state of the face was unaltered; the eye generally was prominent, and much inflamed; the ulcer on the cornea was very large, and in an irritable state. The severity of these symptoms was soon pretty well subdued; and attempts were made, by counter-irritation in every form, and other means, to remove the palsy of the face, but without effect. Sensibility in some measure returned, preceded by anomalous painful feelings in the affected integuments. He continued in this state, without any change in symptoms or appearance, except that the eye seemed to protrude rather more from the orbit than had been previously observed. The ulcer on the cornea filled up. His general health being quite restored, and medical treatment not appearing to have any effect on the local complaint, he was desirous and recommended to go to his friends in the country, and trust to the efforts of nature.

However, on the 1st of February, on making examination of the eye, I felt on pressing the globe a distinct pulsation; and farther found, deeply situated in the upper and inner part of the orbit, a firm pulsating tumor, which appeared about half an inch in its transverse and longest diameter: it was situated between the levator of the eyelid and the bone, and did not show itself externally, but when the eyelid was raised it caused some projection of the loose conjunctiva.

The pulsation of the tumor was accompanied with a very distinct whirr, which could also be felt on pressing the parts in its immediate neighbourhood. Through the stethoscope, a very loud aneurismal whizzing sound was communicated, which could also be heard on applying the instrument over the inner canthus of the other eye, and on

the left side of the frontal bone, as high as the roots of the hair, and nearly as far back as the ear.

He has very loud noises in the head, in the right ear, resembling the sound of church-bells, and in the left like the breaking of waves on the sea-shore; he complains more of these incessant noises than of anything else. The eye feels hot and uneasy, but otherwise he has no pain.

As pressure on the left common carotid put a stop to the pulsation and sounds of the aneurism, and to the noises in his head, it appeared to me that a ligature on that vessel presented hopes, and perhaps the only hope, of affording him relief, or even permanent cure. As a preparatory step, and he being very well able to afford the loss, 20 ounces of blood were taken in the evening, and the next day, Feb. 2nd, the left common carobid was tied: immediately on tightening the ligature, the pulsation and sounds of the aneurism ceased, as also the internal noises. In the evening, four hours after the operation, obscure pulsation could be felt in the tumor, which, however, was not large. The whizzing sound could also be plainly heard with the stethoscope, and over as large an extent. There was no pulsation in the temporal artery; the internal noises were at intervals nearly as loud as before the artery was tied, and at others, nearly absent; he felt great pain on swallowing; pulse 110. He took ℥ss. of liq. opii sedativus, and a wetted cloth was applied to the forehead and eye. On the 3rd, in the morning, the pulsation was very obscure, and the sounds much diminished; the interval noises were also much less. He had had no sleep, and complained much of pain on deglutition and on coughing, and of severe pain in the left hypochondrium. He has also a troublesome cough, which he has had for some time; pulse 120, sharpish. He was bled to 16 oz., with immediate relief, and in the evening was much easier in all respects. On the 4th, no remains of the tumor could be felt, and all pulsation was gone from the orbits, nor could any sound be heard with the stethoscope. The internal noises are quite absent, and his hearing somewhat improved; pulse 100, soft; skin moist, and tongue clean. The pain had left the left hypochondrium, but he felt some in the right, when coughing. The wound was dressed, and the sutures removed; union had taken place to a great extent. In the evening the cough was occasionally severe, with mucous expectoration, and pain referred to the diaphragm; pulse 120, soft; skin moist; bowels not opened. He had some calomel and compound ext. of colocynth, and a mixture with sulphate of magnesia and tincture of digitalis, and a linctus for the cough.

5th. He felt very comfortable; has had several stools; countenance pale; pulse 100; eye less prominent.

6th. Feels weak, but is quite free from pain; cough nearly gone. The conjunctiva much less vascular than before the operation, and the cornea is clearer; some grumous blood was discharged from the left nostril in the night; pulse 90.

7th. Pulse 80; the lips of the wound are opened, but it is filled with healthy granulations; sleeps well; appetite too good.

11th. He sat up several hours, and on the 15th the ligature came away.

18th. The wound is all but cicatrized; he is quite free from pain or uneasiness, and feels only hungry and weak; he is not yet allowed meat; pulse 70, soft. No remains of the aneurism can be detected; the eye has returned wholly to its natural level. The upper half of the cornea is quite clear, the lower occupied by a dense leucoma, to the centre of which runs a large red vessel from the conjunctiva; that membrane is hardly vascular, and vision is good through the clear part of the cornea, when the lid is elevated; the pupil natural.

The left side of the face is quite paralyzed, but sensibility is perfect, except on the left side of the nose, where he feels pricking pains when touched. He has no power of motion whatever over the globe of the eye, and is still very deaf, and more so in the left ear than in the right, but has no noise in the head. His intellects have never been affected. Pulsation is very distinct behind the lower portion of the sterno-cleido-mastoid muscle, probably in the subclavian.

If any change of consequence takes place while the man is under my care, I will take the liberty of communicating it through your publication.

CASE OF RUPTURE OF THE UTERUS,

FOLLOWED BY COMPLETE RECOVERY.

To the Editor of the Medical Gazette.

Sir,

I beg leave to lay the following case before the profession, through the medium of your journal.

I was sent for to Mrs. Rook, aged 28, a strong healthy woman, in labour of her third child, on the 1st November, 1834, at seven A.M. I had gone to a distance a short time previously, and did not return till nine o'clock. I found her in very severe labour, the breech presenting at the external orifice. The membranes had given way at the time I was sent for, and the labour was described as having been severe from six o'clock. The breech was expelled in a few minutes; the pains all the while most violent, and continued until the child had passed as far as the umbilicus, when they suddenly ceased, and the patient became pale, and vomited several times. Considering these symptoms as merely the effects of exhaustion, I desired her to have some spirits and water; which in some degree revived her. After waiting several minutes for a return of the pains, the child became convulsed. Afraid it might be lost, I determined to force the delivery. I had not proceeded long when the propulsive action returned, but now so weak that I was compelled to use all the force I could consistently with the safety of the child. When the delivery was accomplished the woman was quite exhausted, which I attributed to the severity of the labour. Having ascertained that there was no unusual discharge, I began to use the necessary means for the restoration of the child, in which I might be occupied about ten minutes. When I laid hold of the cord I felt a twitching, which induced me to think there was another child; and on applying my hand to the abdomen for the purpose of ascertaining, I was confirmed in the belief, by finding the epigastric region enormously distended. On pressing firmly, in order to satisfy myself, I was somewhat surprised to find the tumor give way suddenly under my hand. Now convinced there was no child, I began to suspect the existence of hour-glass contraction. The twitching still continued in the cord, and as the discharge was considerable, and the woman becoming faint, I gave her sixty drops of laudanum, preparatory to introducing the hand. I had considerable difficulty in overcoming the contraction, and it was not till upwards of a quarter of an hour had expired that this was effected. The uterus was quite flaccid beyond the point of contraction. I found the placenta in the right lateral, and inclining to the anterior, region of the uterus, firmly adherent. Tracing it to the fundus, what was my horror on getting among the intestines! My very blood ran cold, and I felt as if I had been guilty of murder. On directing the finger forward, I felt the peritoneal lining of the abdomen. When withdrawing my hand, I pushed the intestines up clear of the lacerated edges of the uterus, and then endeavoured to separate the placenta by scratching it with the nail. Finding this in vain, I resolved to break it down, and bring as much of it away as possible, that the uterus might be allowed to collapse. I succeeded in removing about a third of it, when the woman became so faint as to oblige me to desist from all further attempts. The discharge being considerable, I determined, as a forlorn hope, to plug the vagina, so as to prevent the external discharge, and thereby encourage the formation of a coagulum: fearful, at the same time, that I was only avoiding one evil to incur another—its escape into the abdomen.

Cordials were now freely administered, as the patient was apparently in a sinking state, and I informed her attendants of the nature of the case, telling them I had no hope of her recovery. She was seized with faintings several times during the day. On the following morning I removed the plug, and in the course of the day I requested Mr. Blackburn to see the case. He urged me to endeavour to remove the remainder of the placenta: in the evening I removed one or two small pieces with the finger, which were lying loose in the upper part of the vagina; but she complained so much of tenderness that I did not venture to introduce the hand. The abdomen was somewhat tender and swollen, so that she could no longer bear the bandage, which was accordingly removed.

On the evening of the 3d (Mr. Blackburn being still anxious for the removal of the placenta), I requested him to make the attempt. He introduced the hand into the vagina, and laid hold of what he thought was a part of the membranes: on pulling it down, it was followed by a portion of the intestine. After having examined it, the membrane which was gangrenous was cut off, and the intestine returned as speedily as possible. We now gave the woman up for lost, not conceiving there was the slightest possibility of her recovery.

She continued very low from the time of delivery. Pulse from 120 to 150, small, thready, and compressible. On the fourth day she was seized with vomiting of a dark-coloured fluid, resembling coffee-grounds. Abdomen much distended, and tender on pressure; notwithstanding which, the wine, and occasionally brandy, was continued, as the patient appeared to be sinking. The opiates were omitted and castor-oil administered, the bowels not having been moved from the first. We now concluded that there must be strangulation of the intestine. Dr. Carson was called in at this time, at the request of the husband: he cordially agreed in the treatment pursued, and advised its continuance. The bowels were freely moved in the evening, but the vomiting did not cease until early the following morning—about sixteen hours from its commencement. She went on to improve for three days—treatment, wine, beef-tea, soups, and opiates at bed-time—when the vomiting again returned, and lasted for ten hours, though not so severe as on the former occasion, and the bowels were severely purged; for which opiates were given. After this she improved rapidly, and began to take a little food; the pulse becoming stronger and less frequent, and the bowels regular.

From the second day there was a dark grumous discharge from the vagina, extremely offensive, with portions of membrane occasionally: it afterwards became whitish, and of the consistence of cream, and continued till the 17th December. She was able to leave her bed in three weeks, and went on improving without any untoward symptom.

I have frequently seen this patient since, and though she is not so stout as formerly, yet she enjoys the most perfect health. She has not since menstruated, but she is still suckling the child.

I am not certain how far I was justified in breaking down the placenta; yet it must have had the effect of causing its separation in a shorter period than otherwise would have happened: but, on the other hand, I was thereby incurring the risk of increasing the hæmorrhage.

The patient was a woman of a most robust constitution, and to this alone must her recovery be attributed. She entertained the most sanguine hopes of recovery herself, and, in spite of every remonstrance, she persevered in suckling the child, although there was little or no secretion of milk until about a week after delivery.

I cannot exactly say what was the extent of the rupture in the uterus: so far as I was able to judge, I should state it to have been at the least eight or nine inches.

For about two months previous to her confinement, she was affected with a pain in the right iliac region, which was occasioned by a fall. I am inclined to think that the excitement consequent on the injury may have occasioned so intimate a union between the substances of the uterus and placenta, as to prevent the free contraction of the uterus in the part occupied by the placenta; so that one part being weaker, and contracting more, than the other, occasioned the rupture. This is at least probable, from the circumstance that the one side of the rent was bounded by the margin of the placenta.—I am, sir,

Your most obedient servant,

John Stuart Currie,
Surgeon.

Edgehill, Liverpool,
Feb. 7, 1836.

CHEMICAL ANALYSIS OF HOMŒOPATHIC POWDER.

To the Editor of the Medical Gazette.

Sir,

I do not pretend to fathom the philosophic depths of the homœopathic system of healing, but I can deal with its facts. I lately got possession, on the point of a pen-knife, of a small portion of one of its mysterious powders, which contained altogether as much as would lie on a shilling, and was to be the sole medi-

cine for four days; and I submitted this quantity to a microscopic investigation. The result at which I arrived may be judged of by others, when they know the several processes of my analysis. My stock was not great, and you may readily conceive, that for a variety of experiments, I had to husband it carefully.

In the first place, to a very minute portion of it, I exhibited ni ric acid; the whole of the mass gradually broke down and dissolved into a colourless mucilage. To this I applied ammonia; and the substance being spread very thinly over a plate of glass, on spontaneous evaporation it crystallized readily in long flat prisms. I was led at once to conjecture from these two results, that the substance was no active mineral, but that it was probably of the nature of sugar, and that I had obtained in the first place oxalic acid, and in the second the prisms of the oxalate of ammonia.

I tried several other processes, which need not be detailed: but on adding a portion of it to a drop of bichromate of potash, I found that a part of it dissolved into a colourless fluid, in which the bichromate floated in small flat drops, each of these drops containing, floating within it, a number of the oval molecules of starch, and that these in evaporation entirely prevented the otherwise ready crystallization of the bichromate.

I then spread a portion of the powder on a glass, and breathed on it. The moisture immediately swelled the oval particles, and it assumed all the character of starch wetted with warm water, the particles, however, being occasionally agglutinated by another amorphous substance. On applying iodine, the particles assumed a dark colour.

Again, I dissolved some of the powder in sulphuric acid, which occurred with a slight evolution of gas. On adding hydriodate of potash, the iodine was disengaged, and thrown down, and the molecules appeared of a dark iodine tinge.

I then took a portion of the moistened powder, and added nitric acid, and watching the action narrowly, I observed that it swelled still larger, and burst successively the starch molecules, the same as it does in common starch, only that the substance of these appeared more delicate, and made less resistance. Ammonia had the same effect.

Lastly, I held a small quantity on a glass over a spirit-lamp. It melted at a moderate heat, turned transparent, and yellow, and then brown, and smelt a sort of mixed smell of burnt flour and sugar.

From all these facts I am disposed to conclude that this powerful specific, which is to be administered in infinite small doses, is a compound of sugar and starch, obtained from some delicate vegetables. Had there been any active mineral principle, it must have appeared; and if this is the case, then the mystery of homœopathy does not lie in the medical treatment, but in some recondite arcana yet to be discovered.

I am, &c.

E. C.

[The party, a distinguished surgeon, by whom the above was sent to us, is satisfied that the accuracy of the writer may be depended upon.—Ed. Gaz.]

ON THE PUNISHMENT OF THE GUILLOTINE.

Various opinions have been entertained, nor has controversy yet ceased, touching the question—whether a person decapitated by the guillotine suffers pain for any length of time? In his work, "*Sur l'Incertitude des Signes de la Mort*," M. Julia de Fontanelle has given a summary of the arguments urged by those who support the affirmative side, and adds his own impressions on the subject, to the effect that there *is* decidedly pain subsequent to the act of decollation. When the Convention, in 1792, adopted the guillotine in lieu of the gallows, and were moved to the change perhaps principally by the convenience it offered for inflicting death rapidly, the motives of those who recommended it to their adoption, were humane. Guillotin,* Cabanis, Petit, and other

* Dr. Guillotin died in 1814; he never ceased to lament that his name had become inseparably connected with an instrument of so many horrors. He had perhaps the more reason to regret this, inasmuch as, it is said, nobody ever was despatched by the machine proprosed by him: the *Guillotine*, properly so called, had its edge placed horizontally. It was Louis who suggested its actual and most efficient form, with the edge oblique, whence the *Louisine* would be its more appropriate appellation: but it was too late—the people had already got hold of its first name.

eminent physiologists of the day, bore testimony to the value of the instrument, as one that was well fitted for putting the victim at once out of pain. But Sue, Soemmering, and Mojon, among others, maintained the reverse.

Professor Sue reasoned chiefly from analogy. He struck off the heads of fowls, sheep, and calves, and observed that there were in every instance marked indications of sensibility. In a turkey which he decapitated, the movements of the head continued for a minute and a half; the mandibles and the pupil displayed strong action, and the eyelids winked. The body, after remaining motionless for a minute, rose, stood in the natural position for a minute and a half, walked, shook its wings, put up its foot to scratch its neck, then fell into convulsions and died. All this occupied six minutes, nor did the heart even then totally cease to beat.

In the head of a decollated calf, there was no cessation of movement of the eye-lids, pupils, ears, nostrils, muscles of the face and lips, for above six minutes; and the body moved for seven. The whole expression of the animal indicated intense suffering.

M. Mojon, of Genoa, made some experiments at Paris in the year 1804, in conjunction with MM. Guillotin, Nauche, and Aldini: they obtained the heads of some persons who had just been guillotined, and the following facts were observed:—That for a quarter of an hour the eyes were sensible to strong light; the eye-lids, on being raised, were spontaneously and hastily closed again. That the tongue, on being pricked with a needle, was immediately drawn into the mouth again, and the features betrayed signs of pain; and lastly, that the ear also was sensible to sounds. M. Julia de Fontanelle adds, in confirmation of this last circumstance, that he recollects having seen the head of a man that had been guillotined, on being called by name, *Tillier*, or *Detillier*, turn its eyes to the quarter whence the voice proceeded; nor does he think the story so very improbable, that the head of Charlotte Corday blushed with indignation when the executioner handled it roughly on holding it up to the crowd!

M. Castel, also, in an able paper *On Sensibility*, in one of the journals, says he has no hesitation in believing that the head, immediately after decollation, still possesses the sense of sight and of hearing, as well as the perception of pain.

What M. Julia de Fontanelle states as his final opinion on the subject, is couched in these terms:—"Decapitation is a most cruel mode of death, inasmuch as both head and body suffer *incomprehensible* pain for some time after the blow; the head more particularly, as it is more pre-eminently the seat of pain, paying dearly in this respect, as Petit says, for its prerogative of lodging the great organ of feeling. If every violent change of the organic functions is painful, *à fortiori* the separation of the head from the trunk must be so. It is a dreadful punishment; the circumstances are terrible. * * * * * In short, death by the guillotine is one of the most appalling, cruel, and torturing methods of taking away life: the feeling of pain, I am persuaded, continues for a considerable time, nor is sensation completely extinct as long as the vital heat remains."

On the other hand, some able remarks have been recently made by M. Dubois d'Amiens, which throw discredit on the observations and tenets of his predecessors—as already quoted. He shows the fallacy of some of the physiological arguments adduced by MM. Mojon, Sue, and Fontenelle, and maintains stoutly that sensation in the individual is annihilated on the fall of the knife. "It was noticed," he says, "by M. Mojon, that the blood does not immediately forsake the vessels of the head on decapitation,—a fact, by the way, not much known to painters, who always depict the head as streaming with blood when newly cut off from the body. For the first seven, eight, or ten minutes, there is no flow of blood from the cerebral vessels; but after that period the parts become relaxed, and hæmorrhage ensues. There may be convulsive movements during this time in the muscles of the face and eyes; and these movements may be influenced by stimulants; but it is wrong to infer that these convulsions betoken pain. Such reasoning is deceptive: the circumstances which take place in certain diseases, and in experiments on living animals, prove that it is far from trustworthy. In convulsions, for example, it is well known that the more violent

the paroxysm, the less the pain. The patients, however they may be exhausted afterwards—however they may suffer from lacerations of the tongue, or bruises received in the fit—have felt no pain: the centre of sensation was neither conscious of the internal acts of the organism, nor of the barbarous treatment but too often employed in these attacks. *Clonic* convulsions, it is well known, besides being wholly uncontrolled by the powers of volition, take place without any perception on the part of the individual; while *tonic* convulsions, and those that occur in tetanus, and from poisoning with nux vomica, upas tienté, the bites of rabid animals or rattle-snakes, are undoubtedly painful, but it is because the sensitive centre is uninjured. There is, in short, no analogy between these phenomena and those which are observed on the removal of the head from the trunk.

"The assertion that pain must be felt when the muscles of the face are seen to be convulsed, is no more physiologically true, than to assert that there is no pain when the features are destitute of expression, and the muscles all in a state of repose; for experience shows that the fact is just the reverse. There are disorders in which the frame is wholly unmoved, and yet the pain felt is agonizing beyond conception. We are told by M. Orfila, that when he laboured under cholera, he was for twelve hours pulseless, voiceless, motionless, blue, and icy cold; yet all this time he was a prey to the most dreadful torture: he felt as if pain darted out from every point of the spinal marrow, towards the surface, and then became concentrated in the region of the stomach.

"In the punishment of the guillotine, the instantaneous division of the integuments, the muscles, the vessels, and every part surrounding the spinal marrow, must prevent the occurrence of pain, the whole being done with such rapidity. But the division of the spinal marrow itself—is this attended with pain? The fact seems to be, that it occasions at the instant such a disturbance (*perturbation*) in the sensitive centre, that the functions of the latter cease immediately.

"It may happen, however,—at least there are some probabilities in favour of the supposition,—that the brain does feel for a few instants, but its sensations can only be those of violent disturbance. The brain continues to live as long as it is visited by arterial blood: when that fluid ceases to circulate through it, death ensues—but not instantaneously—particularly as we have seen that the heads of the guillotined retain their blood for several minutes. But is it reasonable to suppose that a head, under such circumstances, should be sensible of painful affections? An energetic stimulant will no doubt augment the intensity of a convulsive movement: a muscle taken from the body may be excited by certain stimuli; yet all this by no means warrants us in believing that the decapitated feel pain."

On these remarks of M. Dubois d'Amiens, which were first published in M. J. de Fontenelle's work, the author makes a few observations by way of reply. He objects to the argument which assumes the unconsciousness of epileptics during a paroxysm. "Though the patients," says M. Julia de Fontenelle, "may assure us they felt nothing, are we to credit the fact? or should we not rather conclude that the sensations they experienced left them no power of recollection—no traces in their memory? Take the instance of somnambulists: they reply to your questions during sleep; yet, on awaking, not only remember nothing, but positively deny that they had a conversation with any one. May not also that exhaustion, which M. Dubois admits, be the effect of overstrained sensation in consequence of the pain suffered during the paroxysm? The want of memory is no proof that there is no suffering during convulsions.

"With regard to the expression of the features, which M. Dubois considers as merely the effects of convulsive action of the muscles, how happens it that *pain alone* is invariably indicated, and that that expression is heightened in proportion to the stimulus applied. *The head of a guillotined man was never observed to look merry, to smile, or even to remain placid.*

"Soemmering, Mojon, Sue, and others, who believe that the heads of persons decapitated by the guillotine are sensible for several minutes, found that belief not only on the fact of the expression of pain so distinctly marked on the face, but on that of the sudden closing of the eyes when turned to the rays of

the sun, a circumstance which has often been noticed. It may be said that this is the mere effect of the stimulus of light on the muscles of the eye; but if so, why is not the same phenomenon observed in all other cases after death? Because in the guillotined the luminous rays stimulate the retina *still alive*, and this excites the reaction of the eyelids; but nothing of the sort can happen to *dead* eyes.

"In conclusion," says M. de Fontanelle, "it is generally admitted that life is the result of organization; that the brain is the centre of sensation; that the head of a guillotined person maintains for some minutes its proper condition and structure—that is to say, all the elements and conditions that belong to it while alive: why, then, should we deny it, during this short space of its organic integrity, the sensitive faculty which is the attribute of that state*?"

REPORT OF A PAPER
ON THE DEATHS
OF SOME
EMINENT PHILOSOPHERS OF MODERN TIMES.

Delivered at the College of Physicians, Feb. 22,

BY SIR H. HALFORD, BART.

AFTER some introductory observations, in the course of which he alluded to the great number of persons distinguished for their intellectual acquirements who had borne testimony to the truth of christianity, the learned author of the paper proceeded to remark, that he might have selected his examples from amid a host of divines, but preferred the spontaneous declarations of other philosophers, whose evidence might, perhaps, be received with greater readiness, as their opinions were not open to the suspicion of being in any degree biassed by a sense of professional duty. He then continued—

LORD BACON, in the indulgence of his fancy, conceived a notion, whilst taking an airing in the winter, that snow would preserve animal matter from corruption; and bought a hen with which he might make the experiment immediately. As soon as the fowl had been disentrailed, he filled it with snow, and deposited it in a large snow-ball. By this operation he was chilled, but nevertheless continued his airing, until he became so ill as to be obliged to stop at the house of Lord Arundel, at Highgate, where he desired to be put to bed. He died of an inflammation of the lungs there, in a week afterwards, in the 66th year of his age.

It may be presumed that the state of his mind gave the disease a great advantage over him. The degradation which he had suffered by the King and Parliament, I dare not say unjustly, though I cannot conceive that his morality was of that Spartan kind, which made the crime consist in allowing it to be detected; or of that Venetian character, in later days, whose best conscience was, according to the poet, not to leave undone—but to keep unknown. But the humiliation had broken his spirit; and although his punishment had been remitted by the same authorities, no doubt that such dilapidation of frame as care and sleepless nights were sure to give rise to, facilitated that dissolution which might not, otherwise, have taken place for years.

To the learning and philosophy of Lord Bacon his posterity has done ample justice. The reasoning by induction, first and exclusively propounded by him as applicable to natural philosophy, is now the only mode of reasoning held to be legitimate in physics; and his writings abound so much in imagery and good sense, as well as knowledge, that it is difficult to lay down the book when once one has taken it up. His disquisitions upon ecclesiastical polity are so luminous and just, and his proposals for simplifying the law so intelligible and practical, that if we may judge by what has lately been done by the wisdom of Parliament, they must have furnished the model for its recent improvements. But his piety is poured forth in such strains of simplicity and beauty, that I must quote one specimen of it, from an address which he was accustomed to utter in his devotions:—

"I have delighted in the brightness of thy sanctuary; thy creatures have been my books, but thy scriptures much more. I have sought thee in the courts,

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* Recherches Medico-legales sur l'Incertitude des Signes de la Mort, les Dangers des Inhumations precipitées, &c. Par M. Julia de Fontanelle, Professeur de Chimie Médicale, &c. Paris.

fields, and gardens, but I have found thee in thy temples."

To Lord Bacon's genius succeeded a kindred spirit, the ornament and glory of his age, the Honourable Robert Boyle, who was born on the day on which Lord Bacon died. He was of a very delicate habit, and so pale and thin as made it appear wonderful to his friends of the Royal Society, then lately established, that he was able to occupy himself, so laboriously as he did, in making the numerous experiments which were required in his investigations. Nevertheless he lived to 65 years of age, and died exhausted and worn out by natural decay, rather than by any notable well-characterized disease; though it is not improbable, from such details as have reached us, that it was the climacteric malady which destroyed him.

With some of Mr. Boyle's works we are all acquainted. Boerhave, who ought to be authority with physicians, said, which of Mr. Boyle's writings shall I recommend to you? All of them: to him we owe the secrets of fire, air, water, animals, vegetables, fossils; so that from his works may be deduced the whole system of natural knowledge. He did, indeed,

"Look through nature, up to nature's God."

And to the accomplishments of the scholar and philosopher he added the most exalted piety, and the purest sanctity of manners; and the end and aim of all his inquiries into nature, was to do honour to its great Maker.

Sir Isaac Newton was born so puny and sickly a child, that his mother thought he could not live many days; yet his life was protracted to 84 years.

Does this appear to any of you marvellous? Let him recollect that it is probable extraordinary care was thrown around his diminutive helplessness; that this close attention to all the imperfections of his tender frame would be continued to the period of its complete development; that such habitual watchfulness over all its movements would, at length, render his life more secure than that of a robust habit, which might, by negligence, be surprised into danger and death. The uncertainty of human life of which we all complain, is rendered more uncertain by our own improvidence and inattention, and by a misuse of our strength, which, under certain indispositions, allies itself with disease, and aids it in the destruction of the vital spirit. This is most observable in inflammations, and in apoplexies, and in other ills also where the natural abundance of blood, if circulated quietly, constitutes health; but in the slightest excess and hurry, a dangerous plethora. Sir Isaac died at last of the stone. This evil did not manifest itself until a very short time before his death. He had taken a house at Kensington, in hopes of remedying a slight embarrassment in his breathing; and having occasion to come to town, to attend a council of the Royal Society, he suffered torments the next day, which never ceased till they had destroyed him. Dr. Mead and Mr. Cheselden, who were called to him, were of opinion that the stone had been imbedded in the substance of the bladder, and was moved from its quiet position by the jolting of the carriage. Whatever had occasioned his distress, Sir Isaac never betrayed an impatient feeling, but was entirely resigned to the will of the Almighty, and sought and found comfort, not in his philosophy—not in the fame of his optical experiments, and of the demonstration of the planetary orbits from the principle of gravity—for he knew that

"——— nec quicquam tibi prodest,
Aërias tentasse domos, animoque rotundum
Percurrisse polum, morituro!"—Hor.

no; but in contemplating the benevolence and mercy of God, and in a humble hope of the intercession of His Son.

It is recorded in his epitaph, that he asserted in his philosophy the majesty of God, and exhibited in his conduct the simplicity of the gospel; and a philosopher of high credit of modern day has remarked, that it is one of the proudest triumphs of the Christian faith, that he, who, among all the individuals of his species, possessed the highest intellectual powers, was not only a learned and profound divine, but a firm believer in the great doctrines of religion.

I should be glad to assist in refuting the allegation made originally, I believe, by Huygens, a foreigner, upon the authority of a letter from a young man at Cambridge, and lately repeated by a most respectable periodical publication, of Sir Isaac Newton's having been insane. It was said that he had suffered a severe mental emotion by the

loss of his papers, containing calculations which it had cost him the labour of many years to make. A candle had been left incautiously upon the table on which these papers lay, and a favourite little spaniel having overturned the candle, had set fire to them and burnt them; and this occasioned a temporary loss of his reason.

I confess I am not satisfied that, whatever degree of disappointment and vexation such an accident might occasion, the result amounted to insanity. It is the business of those who make such a charge, to substantiate it by proof. Accordingly, a letter of Sir Isaac's to Mr. Locke has been called up amongst the arguments in proof the derangement of the author's mind. The letter manifests, indeed, a great deal of irritation—such as intense thought upon an abstruse subject, long continued, without the intervention and refreshment of sleep, might occasion; but a subsequent one, written a fortnight afterwards, apologizes for the rudeness and discourtesy of the former, and refers it to his not having slept for an hour together any night, and for five successive nights not a wink. Mr. Locke's reply to this does not convey the slightest suspicion that he entertained such a notion; and it is written with so tender and unaffected a veneration for the good as well as great qualities of the excellent person to whom it is addressed, as demonstrates at once the conscious integrity of the writer, and the superiority of his mind to the irritation of little passions. On Mr. Locke's construction, therefore, of Sir Isaac's letter, and on the view which so good a judge of mind as Mr. Locke took of the state of Sir Isaac's faculties, I rest the decision of this question.

What I have now said of Mr. Locke, may, perhaps, incline you to desire to hear more of this great man. Besides, he was one of you; for he took his degree of M.B at Christ Church, Oxford, when he had been a Westminter student, and owed his first introduction to the world to having administered to the health of Lord Ashley, afterwards Earl of Shaftesbury; who had come to Oxford for the purpose of consulting Dr. Thomas, an eminent physician there. The Doctor being called out of town, requested Mr. Locke to attend his Lordship till his return. This was the foundation of an intimacy between Mr. Locke and his patient ever afterwards.

I may remind you, moreover, that by the kindness of the late Lord King, a paper was read here, in the year 1829, containing the details of a case of tic douloureux, in the person of the Countess of Northumberland, at Paris, treated by Mr. Locke; and we have also the testimony of Sydenham to Mr. Locke's medical knowledge, who says, "Nosti prætereà quam huic meæ methodo suffragantem habeam, qui intimius per omnia perspexerat. utrique nostrum conjunctissimum Dominum Joannem Locke, quo quidem viro, sive ingenio judicioque acri et subacto, sive etiam antiquis, hoc est, optimis moribus, vix superiorem quemquam inter eos qui nunc sunt homines repertum iri confido, paucissimos certè pares."

Locke, then, was a physician; and who amongst you does not feel a pleasure in remembering that the honourable profession to which he has attached himself, was the profession to which Mr. Locke applied the powers of his great mind?—and why should we not all take pride in the observation of that eminent scholar and statesman, the late Lord Grenville, that, "from the very first dawn of reviving letters to the present moment, there never has been a period, in this country, when the masters of medicine amongst us have not made manifest the happy influence of that pursuit on the cultivation of all the other branches of philosophy."

Mr. Locke's health was always delicate, and he was subject to attacks of asthma, which sometimes compelled him to go abroad in search of a less fickle atmosphere than that of his own country. He lived, however, to 73 years of age, and died on the 28th of October, 1704, at the house of Sir Francis Masham, at Oates, in Surrey, where he had been domesticated any time within the last 14 years of his life. He was perfectly aware, it seems, that his days were numbered, and was well prepared for the awful moment of separation from this world. We have, from the authority of Lady Masham herself, the best account of the last hours of his life. Having desired that he might be remembered at evening prayers, she asked if he had any objection to the domestics of the family attending the service in his chamber? To which he replied that he

had none. When prayers were over, he gave some orders with great serenity of mind, and an occasion offering of speaking of the goodness of God, he especially extolled the love which God shewed to man, in justifying him by faith in Jesus Christ. He returned him thanks in particular for having called him to the knowledge of that divine Saviour. He exhorted all about him to read the Holy Scriptures attentively, and to apply themselves sincerely to the practice of all their duties, adding expressly, that by these means they would be more happy in this world, and secure to themselves the possession of eternal felicity hereafter. He passed the whole night without sleep, and desired next day to be carried into his closet; where, after dozing a little, he ceased to breathe about 3 in the afternoon, without any indication of pain or suffering.

Mr. Addison died at the age of 47, of dropsy, brought on probably by a disease of the liver. The habits of life of the higher orders of society in Mr. Addison's time were less cautious, and less compatible with health than they are at present. In proof of this, we may notice the greater comparative longevity at the beginning of this century, than was found at the commencement of the last. The Northampton Tables, which were published in the middle of the 18th century, give only 4 as the average number of those who had arrived at 100 years of age and upwards, out of a million. Whereas the population returns of 1821 and 1832 give 26 as the average number of those who had reached 100 and upwards, out of a million. Habitual suppers, and more wine drank after dinner, and after supper, contributed something to the formation of diseases which shortened life; and where these potations were indulged in by literary men, who took no exercise, it is probable that their effect was still more pernicious. Pope has given us a detail of Addison's familiar day, by which it appears that he studied all morning, then dined at a tavern, and went afterwards to Button's. From the coffee-house he went again to the tavern, where he often sat late, and drank too much wine.

But these nocturnal symposia, however protracted and unwholesome, did not prevent nor retard those effusions of graceful humour which the next morning's study produced; nor was there reason, we hope, for remorse, which is sure to accompany the slightest admixture of depravity with an inveterate habit. Accordingly, when the hour of his dissolution approached, he sent for his son-in-law, the Earl of Warwick, that he might see in what peace a Christian could die!

As a describer of life and manners, Dr. Johnson observes that Addison must be allowed to stand first of the foremost rank; and it is his peculiar merit to have made his wit subservient to virtue. All the enchantment of fancy, and all the cogency of argument are employed to recommend to the reader his real interest—the care of pleasing the Author of his being.

Sir William Jones, after a protracted evening walk in an unwholesome quarter in the neighbourhood of Calcutta, was seized with a shivering fit, which was followed with fever, and by symptoms of an abscess in the liver, a common disease in Bengal, and died on the 9th day of his illness, in the 47th year of his age.

It is difficult to withhold an expression of one's wonder at the extent of knowledge he had acquired in languages, arts, and sciences, in the course of so short a life. His acquaintance with Grecian literature was extensive and profound, and in the modern dialects of Europe, French, Italian, Spanish, Portuguese, and German, he was thoroughly conversant. The language of Constantinople also was familiar to him, and of the Chinese character and tongue he had learned enough to enable him to translate an Ode of Confucius. His skill in the idioms of India, Persia, and Arabia, has perhaps never been surpassed by any European; and his compositions on oriental subjects display a taste which we seldom find in the writings of those who had preceded him in these tracts of literature.

In his eighth anniversary discourse to the Asiatic Society, he remarks, that theological inquiries are no part of the subject he was then discussing; but he could not refrain from adding, that the collection of tracts which, from their excellency, we call "the Scriptures," contain, independently of a divine origin, more true sublimity, more exquisite beauty, purer morality, more important history, and finer strains both of poetry

and eloquence, than could be collected within the same compass from all other books that were ever composed in any age or in any idiom. The two parts of which the Scriptures consist are connected by a chain of compositions which bear no resemblance, in form or style, to any that can be produced from the stores of Grecian, Indian, Persian, or even Arabian learning: the antiquity of these compositions no man doubts, and the unstrained application of them to events long subsequent to their publication, is a solid ground of belief that they were genuine productions, and consequently inspired.

Of Dr. Johnson it will be much more difficult to say too little than too much. The very interesting book of his life is to be found upon every gentleman's table, and with his works many of you are better acquainted, perhaps, than I am. He died of dropsy, brought on by repeated asthmatic attacks, which had annoyed him many years, and had often driven him from the pure air of Streatham, where he spent a good deal of time, by the friendship of Mr. Thrale, into the solitude and closeness of Bolt-court, Fleet-street, in which he found his respiration more easy than any where else. So much for the caprice of asthma, of which my experience has furnished me with many instances.

I remember a gentleman, subject to fits of asthma, who built himself a house in an elevated beautiful situation in Surrey, and whilst it was building, lived in a cottage in a valley beneath it. The first night he attempted to sleep in his new residence, he suffered so much from distress in his breathing, that he returned to the cottage, intending to make a second experiment, under better auspices he hoped, when he should have recovered from his late suffering and alarm. The same experiment was made again and again, with the same unhappy consequences, until at length he was obliged to abandon his new abode entirely, and to dispose of it.

Another example occurs in my recollection, in the person of a patient who consulted Sir George Baker, as well as myself, upon an asthma, which had distressed him grievously. We advised him to travel, as it was in his power to do so, in search of an air that would suit him, and wherever he should find it, there to fix his residence, for some time at least. In the course of his travels, he arrived at Lyme, in Dorsetshire, where he breathed with unusual comfort. After a sojournment of three weeks at Lyme, he was able to walk up a considerable hill out of town, at a quick pace.

Dr. Johnson was born of a melancholic temperament, and of a scrofulous habit. Such a constitution of mind and body would render a man prone to act upon impulses, and to disclose one of the characteristic symptoms of insanity; particularly if he did not entertain a strong religious principle, which might be ready to interpose between the purpose and the deed, and to arrest violence, until reason, in abeyance for a while, should recover its proper authority and sway. Dr. Johnson had this religious principle in its most lively vigour, and a power of reasoning also beyond that of most men of his time. From dejection of spirit, he found relief in society; and it was as happy for his associates and for the world at large, as for himself, that he sought it there, for his powers of conversation were extraordinary, and furnished inexhaustible instruction to those who listened to him. And there is scarcely a writer, whose profession was not divinity, that has so frequently testified his belief of the sacred writings, has appealed to them with such unbounded submission, or mentioned them with such unvaried reverence.

Thus have I laid before you an account of the deaths of some of the most eminent philosophers of the two last centuries in this country; and it cannot have escaped your observation, that in giving also their religious sentiments, I had it in view to bring to your recollection such their invaluable testimony to the truth of the Gospel. I know that in matters of eternal concern, the authority of the highest human opinions has no claim to be admitted as a *sufficient* ground of belief. It is every man's duty to weigh well, and to consider for himself, the reasons of his faith. But it cannot fail to encourage and to confirm his own conclusions, to know that these, the best, the wisest, the most learned of mankind, who devoted much of their time to the study of the Holy Scriptures, arrived at the same results. Bishop Horsley has observed, that the man of science and speculation, the

more his knowledge enlarges, loses his attachment to a principle to which the barbarian adheres—that of measuring the probability of strange facts by his own experience.

And shall physicians want these subjects of speculation, to encourage their hopes, and to enlarge their faith in the promises of the Gospel? Might not Mr. Locke have been led to his lofty contemplations, which ended in so solemn a conviction of divine truth, by those early studies of the nature of "man's small universe," which were to prepare him for our profession? And did not their early converse with the awful circumstances attending the last scene of human life, suggest to Sydenham, to Boerhave, to Heberden, and to Baillie (what, blessed be God! it has suggested to myself), not the hope only, but the confident expectation, of another and a better world, of which they have testified to us and to posterity.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

A Key to Structural, Physiological, and Systematic Botany: for the use of Classes. By JOHN LINDLEY, Ph. D., F.R.S., &c. 1835.

Remarks on the Geographical Distribution of British Plants: chiefly in connexion with latitude, elevation, and climate. By HEWETT C. WATSON. 1835.

The New Botanist's Guide to the Localities of the Rarer Plants of Great Britain. By HEWETT C. WATSON. 1835.

WE are induced to group these works together, not so much for their referring to the same science, but because they stand towards each other in the relation of the starting-point and the goal. The views unfolded, and the language of signs employed in the works of Mr. Watson, can neither be understood nor appreciated, except by the student who has had the benefit of being educated according to the principles explained in the elementary work of Dr. Lindley. Of this, therefore, we shall first speak. It consists of new editions of two productions of the same zealous and accomplished botanist, by which he has already greatly contributed to elevate the attainments of the rising generation of botanists—viz. "The First Principles of Botany," and the "Nixus Plantarium," now presented in an English dress. As two editions of the former have received the seal of approbation from the most enlightened teachers and diligent pupils, it is not necessary to say more of it than that the alterations in it tend to bring it up to the level of the science of the day, as understood by the most distinguished botanists; and that with such aphorisms or texts to discourse from, it must evince great inability on the part of the teacher, or carelessness and incapacity on the part of the student, if the number of scientific and philosophical botanists be not greatly augmented by its farther use. Of the Nixus more might be said, if it were requisite here, or our limits permitted us to discuss the principles on which it is constructed. But we cannot do more than remark that, however great the agreement of most botanists may be with respect to the principles or rules which determine the boundaries of the different natural groups, very great diversity of opinion exists among the most distinguished masters of the science, with regard to the co-ordination of these groups, as must be clear to any one who will compare the Nixus with Professor von Martin's "Conspectus Regni Vegetabilis," published about the same time, having appeared at Nürnburg in 1835. We are far from objecting, however, to such a volume of arrangement being submitted to the student, if it be understood as provisional, and not definitive. It will then constitute a very admirable subject for the exercise of their powers of observation and reasoning. We must introduce another cautionary remark, and that is, to warn the student against committing to heart the succinct and very excellent characters of the different tribes, and supposing that when asked the diagnostic marks of any tribe, if he can repeat these short sentences trippingly on the tongue, that is all that is necessary. If ever he lose sight of the great principle, which is one of the

leading characteristics of the natural method, that the presence of these peculiarities of structure stated in the definition, are indications of the co-existence of certain other peculiarities, he will scarcely be entitled to rank higher than those self-styled botanists, who are perfectly satisfied with themselves when they can state respecting a plant, that it belongs to the pentandria, monogynia, or triandria, trigynia, &c.

The errors we have perceived, or supposed we have perceived, are so trifling, that if the book were not likely to pass into the hands of thousands of beginners, we should not mention them. First, in § 5 we do not exactly understand what is meant. "Plants consist of a membranous transparent tissue, chemically composed of a *hygrometrical* (?) combination of oxygen, hydrogen, and carbon, to which nitrogen is occasionally superadded." 2nd. The reference (101) in § 89 must be incorrect: we presume it is intended to be (218). Lastly, in the Nixus, p. 46, as an example of the Anonaceæ, the *piper æthiopicum* is given: now either some other plant of the tribe should have been chosen, or its real botanical name, *uvara æthiopica*, been used, lest the tyro should be led to suppose that the genus Piper belonged to the Anonaceæ.

The higher the general character of any work for excellence, the more need of accuracy in every part of it.

Mr. Watson, by the publication of the second work on the list, may be said to have taken possession of a field of wide extent and great interest, scarcely any portion of which has been cultivated by preceding botanists. Indeed, the subject of botanical geography, as relates to the world at large, has received very little attention from British botanists, if we except Brown and Hooker; nor was the subject ever alluded to in any of our elementary works, till the publication of Dr. Lindley's Introduction to Botany, in 1832; though a small treatise, or rather sketch, on the subject, had been published by Mr. Barton, in 1827, with maps. No remarks upon the distribution of plants in Britain existed till Mr. Watson printed, for private circulation, in 1832, "Outlines of the Geographical Distribution of British Plants." Some notices on the subject, from the pen of Mr. Arnott, were incorporated with the description of Britain in the Encyclopædia of Geography; and some calculations of the relative numbers of species found in certain districts, occur in Dr. Greville's "Flora Edinensis," and Winch's "Flora of Northumberland and Durham," as well as Johnston's "Flora of Berwick-upon-Tweed." Professor Henslow has made some calculations on the subject, and one or two other observers, such as Dr. Boice (in his inaugural dissertation), and Mr. Macgillivray, have contributed to our knowledge upon these points. But after assigning every fair amount of merit to these different writers, still to Mr. Watson is due the praise of taking a comprehensive view of the distribution of plants in Britain, and investigating the causes which influence their distribution. Much of what he has embodied in his work has been obtained by his own observation and inspection of different parts of England and Scotland, by great personal exertion. But difficult as the first step is in such undertakings, Mr. Watson has taken so vast a stride as to render all the subsequent steps comparatively easy; yet he does not intend to remain content with what he has accomplished, but regards it only as the ground-work of a more extensive treatise; and he requests, in his preface, assistance from all who are capable of affording it. To contribute towards procuring information, we here repeat the titles of the subjects he deems desiderata for the prosecution of his object:—

"1. Altitudes of hills, lakes, and fixed objects.

2. Information on the climate of places, particularly with reference to the temperature and humidity.

3. Dates of the first flowering of any of the following *wild* plants, if carefully noted:—

Corylus Avellana	Ranunculus Ficaria
Prunus spinosa	Viola canina
Cratægus Oxyacantha	Oxalis Acetosella
Rosa canina	Veronica Chamædrys
Lonicera Periclymenum	Hyacinthus non-scriptus
Ulex nanus	Arum maculatum
Hedera Helix	Cardamine pratensis
Erica Tetralix	Lotus corniculatus
—— cinerea	Vicia Cracca
Calluna vulgaris	Digitalis purpurea
Cytisus scoparius	Linaria vulgaris
Ilex Aquifolium	Senecio Jacobæa.

4. The highest or lowest places at which any species has been observed,

whether in absolute height, in comparison with the appearance or cessation of other species, or in relation to the parts of particular mountains, as at the base, middle, or summit; provided such heights are not considerably within the limits assigned to the particular species in the present work.

5. Unpublished, or *recently confirmed* localities for the less common species; as also, localities near the boundary-lines of such as are not spread over the whole island.

6. Actual specimens in confirmation of the localities, heights, &c. will be most welcome, and be preserved in the view of making them public evidence hereafter. The name of the *donor*, and that of the *county*, should INVARIABLY be written on the ticket accompanying each specimen; also the name of the person supposed to have gathered the specimen, if not the donor himself. Neglect of this renders the specimens of little value.

7. Notices of changes in the habits or characters of plants, in connexion with differences of situation and season.

8. Information as to the success attending attempts to cultivate plants of milder or warmer climates, without the aid of artificial heat. Also, notices of the influence of elevation on the produce of fields and gardens.

9. Any corrections or amendments relating to the contents of this volume, as well as criticisms and suggestions for improvement, will be received with pleasure by the author, if allowed to make them public in case it should appear desirable to do so."

The first object may be accomplished more accurately, as well as more easily, than hitherto, by a new species of barometer, invented by John T. Cooper, Esq., which he proposes to call the "*Hydropneumatic*," which is more portable than those heretofore used.

As the subject of medical topography is now more likely to receive a greater share of professional attention than has previously been the case in Britain, we have entered into a very attentive examination of Mr. Watson's work, in order to ascertain how far it will be serviceable in making observations on vegetation and climate, as connected with health and disease. Though some popular ideas on the subject of this connexion exists, accurate data are very deficient in the different counties of England and Scotland; but we trust that our stock of these will annually be increased; and have very little fear upon this point with regard to England, since the attention of the profession has been so ably directed to it in the proposal of Dr. Conolly, submitted to the Provincial Association of Physicians and Surgeons. That is already beginning to produce good fruit, and to augment the amount of this, the great requisite is to interest many observers by convincing them of its importance. In an able paper in the Edin. Med. and Surgical Journal, for April 1821, entitled, "Sketch of a Plan for Memoirs on Medical Topography," occurs the following passage:—"The early appearance of many plants, and the slowness or rapidity of their vegetation, the torpidity, arrival and departure, of many quadrupeds, birds, and insects, will often mark more strongly than the indications of the glass the nature of a climate; and the judicious naturalist will avail himself of the circumstances in his topographical description. Strong indications of the healthfulness of a country may be drawn from its plants and animals; and the approach of unhealthy seasons has often been marked by the changes produced on them. In America, the common house-fly has disappeared, while musquitoes have been multiplied, and several new insects have been observed previous to some of their malignant epidemics; and at similar periods certain trees have emitted unusual smells, and the leaves of others have fallen prematurely, and the fruits have been of inferior size and quality; while, in some places, an unusual growth of vegetable productions (fungi) has preceded the most destructive scourges of mankind."—P. 167.

The truth of these remarks is best exemplified in tropical climates, or the warmer countries of Europe, as may be seen by reference to the various valuable papers on Medical Topography, in the Transactions of the Medical and Physical Society of Calcutta, and "Koreff, de regionibus Italiæ aere pernicioso contaminatis observationes," Berlin, 1817. But in every country, plants (even more than animals) are the most delicate and correct of all barometers and thermometers; the difference of date of flowering of the same species of plant, in different situations, affords

indications of the existence of causes of difference of climate, which cannot be ascertained by any instrument that man has hitherto contrived. Sir Edward Parry, in his Second Voyage, in 1822, p. 240, deemed it important to mention, and mark in Italics, that "the first flower of the *saxifraga oppositifolia* was brought on board as a matter of curiosity, by the botanists, on the 9th of June, *or one day later* than it made its appearance at Melville Island, in 1820." And why this importance attached to what many may think a very trifling difference? Because, in their situation, every criterion of the approach of warmer weather was interesting, to say nothing of the difference of the date of flowering of this plant being accompanied by other differences which materially affected their comforts and prospects of release from their icy captivity. At p. 241 he states—

"June 16th.—Having thus reported our own progress for the last week, I cannot omit saying something of that which nature had been making in the same interval. A few more flowers of the saxifraga had here and there been procured, but they were still curiosities; the more so as being almost the only ones that had yet made their appearance. Some water had now been obtained from the shore, by baling a gallon or two from each little pool, and carrying a cask about on a sledge, to be thus filled. At Melville Island, at the same period, the ravines were beginning to be dangerous to pass, and were actually impassable during the third week in June."

Mr. Watson, at page 37, gives a view of the date of flowering of different plants at different places in Britain, from which it appears that the same species flowers at Barnstaple thirty days earlier than at Strathpeffer, in Rosslin. If we take a more extended range than the length of Britain presents, we shall find a still greater difference: the almond tree (*Amygdalus communis*) flowers at Smyrna in the earliest half of February; in Germany, the latter half of April; and at Christiana, about the beginning of June. It is greatly to be desired that records were kept of the dates at which plants ripen their fruits and seeds, as well as of their dates of flowering. Indeed, it is worthy of remark, that the causes which determine the flowering of a plant are different in degree, if not in kind, from those which determine the ripening of its seeds. And we are the more desirous of directing attention to this point, as the only omission of importance which we have perceived in that part of Mr. Watson's book where he treats of the meteorological peculiarities of Britain, is in some degree connected with this subject. He has taken no notice of the difference of the intensity of solar light on the western and eastern sides of this island, though it affords the only plausible explanation of some very remarkable as well as important facts connected with vegetation. Mr. Barton, in the work before alluded to, states (p. 22), "*Wheat* demands a warmer climate than barley or oats. This grain is not found to succeed in the west of Scotland, the summer's sun being insufficient to ripen it." Even in England, the western side of the island appears better adapted to the growth of grass than of corn; and, accordingly, it may be observed, in every part of the kingdom, that corn is carried from east to west, while cattle are driven from west to east. All our principal corn counties are situated on the eastern side of the island, from the Lothians, in Scotland, to Kent, the south-eastern county of England." And again (p. 29), "At the further extremity of that long promontory, which, projecting into the Atlantic, forms at once the most southern and the most western point of England, neither the *apricot*, the *vine*, nor the *greengage plum*, produce ripe fruit, for want, as it should appear, of sufficiently powerful sun-beams."

Mr. Watson, p. 63, also states, "Wheat is cultivated only in the lower part of the region, (which Mr. Watson terms the *upland*) and in the highlands, chiefly along the eastern coast." P. 64. "On the east coast of Sutherland, the peach will ripen against walls with the aid of a glass sash, perhaps even without." Thus we see, that while the apricot does not ripen in Cornwall, the most south-western country of England, in lat. 50°, the peach ripens in Sutherlandshire, nearly the most north-eastern county of Scotland, in lat. 58°. This remarkable difference is ascribed to the greater number of clouds prevailing on the west coast, on which are placed the highest mountains, taking the whole length of the country, (Watson, p. 3—

17) and which attract the vapours from the Atlantic. The clouds intercept the direct rays of the sun, and only allow diffused light to fall on the plants and fruits. This difference is the more important to be noticed, since the observations of Dr. Daubeny, which establish the law, that the influence of lights on plants depends less on the heating than on the *illuminating* power of the rays.

There are various other important points touched upon in Mr. W.'s book which we had marked for comment, but the length to which this notice has already been extended compels us to confine ourselves to one, viz. the 5th section—"Remarks on the distribution of British plants over other countries." This section we deem of great interest and importance, but we can only direct our attention to some farther illustrative data, furnished by Professor Schouw, on the geographical distribution of trees on the Scandinavian Peninsula, which are printed in London's Magazine of Natural History, for February, 1836.

We feel assured we have said enough, though much less than the interesting nature of the book inclines us to do, to induce every one who attends either to botany as a pure science, or to the important applications of which it is capable for elucidating climate, and other important matters connected with the health of the community, to procure it for themselves, and study it with the attention it merits.

Mr. Watson has promised two additional volumes, in farther illustration of the subject; one of which is to contain plans, maps, and more extended tables. Till these appear, we beg to recommend Mr. Barton's Lecture on the "Geography of Plants," the maps of which are extremely useful. A map, which is also very useful, is to be found in Dr. Prout's Bridgewater Treatise; and one, but on too small a scale, in the article "Meteorology," in the Encyclopædia Metropolitana; but the article itself will be found worthy of perusal.

The third work on our list is part and parcel of that which we have above spoken of. In this view Mr. Watson regards it, and it is undoubtedly the true one, but many will use it as an independent work, being in reality a new botanist's guide to those who search for plants as isolated productions, and without reference to the locality in which they grow. Even for this last purpose, it is much more complete than the Botanist's Guide of Turner and Dillwyn, and is more commodious, being in one volume. In one respect only do we consider it inferior, and that is, in the omission of stations of the cryptogamic plants. Humboldt has remarked, that there is less diversity in the cryptogamic vegetation of different countries, which differ considerably in latitude and other circumstances, than the flowering plants. We conceive, therefore, that as bearing upon the subject of medical topography, the cryptogamic plants of a district are worthy of enumeration, as any considerable difference found among them would seem to point out the existence of some more than usually powerful cause, modifying the climate of the locality. Mr. Watson merits our best thanks, however, for what he has done; and we sincerely wish him every success in the farther prosecution of his labours, as well as solicit for him the assistance which he desires, and which we are sure he will receive from the liberal-minded botanists of Great Britain.

MEDICAL GAZETTE.

Saturday, February 27, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

THE *HYGEIAN* QUACKERY.

THE utter rooting out of all quackery from the world, must, we suppose, by this time, be given up as a hopeless enterprise. In spite of the boasted lights of science, and the diffusion of knowledge, which are said to be daily dispelling what remains of the darkness and ignorance bequeathed to us by our ancestors, matters (so far as we can see) look pretty much as they did in the times of old. When Pliny, some 1800 years ago, could write of his contemporaries, in respect to their predilection for mysteries in medicine,—*minus credunt, quæ ad salutem suam pertinent, si intelligunt*—and add, concerning the parties who were in the habit of taking

advantage of this wilful blindness, *itaque hercule in hac artium sola evenit, ut cuicunque medicum se professo statim credatur, cum sit periculum in nullo mendacio majus*,—the illustrious natural historian may be considered as not only stating what was the case in his own days, but for ages before him, and, with a sort of second sight, what is likely to remain a truth to the end of time.

Our belief is, that quackery has been permitted, in the great scale of natural and moral dispensation, to run its career with medicine, in order to serve as a spur, or a scourge, to quicken the activity of those who profess the legitimate healing art—to put their zeal and their faith to the test; much in the same manner as heresy and infidelity are allowed to figure in the same course with true religion.

But such a persuasion by no means leads to the principle that quackery should for a moment be tolerated, or that we should relax in our efforts to spurn and put beneath our feet the impudent impostors who infest society. And of all impostors, who can be looked upon as more thoroughly offensive or detestable, than those who, like the *Rev.* Robert Taylor, in the garb and disguise of a respected calling, abandon themselves to vile purposes, and for a little paltry notoriety, or lucre, prostitute the profession to which they belong? Such persons, however, are luckily rare, and the odour of their presence, when they do make their appearance, is generally too rank for ordinary nostrils long to endure.

The quackery that prevails at different times is of various and diversified character. Not long since we had it in a sort of *parvenu* shape; from the humblest origin it reared its head among the high places, and obtained a short-lived footing within the aristocratic circle. The late St. John Long, compared with the sort of people who now solicit public attention, and practise on public gullibility, was a respectable personage: he had some notion of the decencies to be observed in ministering to the cure of disease; and though he committed certain fatal blunders, he had not, we believe, a tithe of the mischief to atone for that has been perpetrated by his successors. The fellows who at present prey on the community, and who profess the *Morisonian* humbug, calculate so far on the credulity and love of mysticism prevalent among the middle and lower classes, as to serve up daily in the public prints manifestoes, addresses, and other puff documents of the coarsest texture. They must find it adapted to their purpose, or they would not persevere. They have even the presumption to affect to *theorize*—to *reason*—to support with *argument* the principles which they announce. Any thing more grossly clumsy, we believe was never before attempted to be thrust down the public throat—capacious as it is for swallowing the boluses of quacks. Down, however, the lumps of stupidity go, along with the lumps of pills—both alike suited to strong stomachs. It would be, indeed, only a ludicrous exhibition were it confined to such maws: but the weak in mind and the weak in body form a large proportion of the community, and to such this sort of "bolting" is but too often fatal.

That "*blood makes blood*" is one of the precious dicta of the "Hygeists:" we begin to believe the maxim ourselves, when we see that one homicide by the *hygeian* pills leads to another in quick succession: the blood already stamped on *the* pills—*the blood of repeated manslaughters*—is not enough to deter the unhappy dupes who hunger for them; and many of whom must pay the price of their depraved appetite with their lives. It is a curious instance of the infatuation which becomes occasion-

ally epidemic in large and *soi-disant* enlightened states: and to the philosopher who would estimate the amount of common sense prevailing in this country, and more particularly in this great metropolis at present, it must afford a valuable subject for meditation. We know not that any of our statisticians, by the way, have ever attempted calculations of this sort—the estimating the amount of rational principles prevailing in communities at different periods—but we suggest that the history of the rise and progress of quackery in the world, if honestly recorded, would afford a large portion of the required data. But to come to the point which induced these observations.

Our readers have, doubtless, read in the public prints of last week, (we merely had room to mention the result ourselves), some account of the inquest held at Ratcliff workhouse, on a new victim of the Morison quackery. The verdict pronounced by the jury in that case was one arrived at not hastily, but after a patient hearing of much evidence for several days; and nothing could be clearer, from the circumstances deposed by most of the witnesses, than that the verdict was a just one—that manslaughter of an aggravated nature had been committed by the administration of the Morison quack pills. But the most remarkable feature in the inquest—that sort of judicial investigation being in itself now by no means novel in reference to the Morison quackery,—the most striking circumstance, we say, attending this inquest, was the presence of certain individuals, calling themselves regularly educated medical men, who came forward as the humble disciples of the *Hygeist*, to defend the propriety of administering certain medicines, of the composition of which they professed to know nothing. There was a Dr. Lynch, an Edinburgh graduate, who took the lead on this occasion, in advocating what he calls Hygeiaism, and who was very chary in announcing himself as the paid lecturer of Morison: he was, indeed, touchy in the extreme when interrogated on this point. He has, however, since that, we perceive, advertised himself in the newspapers in connexion with his patron, appending his signature, along with that of Morison, to a tissue of the most stupid and beastly trash. He has also given notice of the continuation of his course of lectures at Exeter Hall, to which, like the preceding ones, the public are invited *gratis;* the subject of the lectures being "Hygeian Medicine," alias, Morison's pills. Now here is the *professional* gentleman whom Morison has retained as his great apostle and advocate, and of whose conjunction the *Hygeist* is abundantly proud; referring all parties who yearn for the vegetable pills to the Doctor, and showing his gratitude, no doubt, in a hundred ways that the public are not aware of. We have no wish to stamp the professional character of any medical man with ignominy; but if, as we have said, quackery be to medicine what avowed infidelity is to religion, we leave Dr. Lynch to guess to whom, in modern times, we must compare *him;* for he has, in the theological profession, a pretty exact and faithful counterpart*.

Of the other medical man, who figured in Morison's defence, (a Mr. Tothill or Tuthill of Exeter), the sort of evidence he gave will serve as a specimen. He knew nothing, and cared nothing, about the ingredients of Morison's pills; but he was full of gratitude for the benefit they had done himself, when he laboured under (we believe)

* *Dr.* Lynch, at the inquest, informed the public that he had formerly lectured at Mr. Grainger's school, in the Borough; but Mr. Grainger has since explained, that the *Dr.* never gave more than three or four lectures there. The subject was morbid anatomy—not Hygeian quackery; nor did Mr. G. at that time suspect the *Dr.* to be in any respect a "Hygeist."

rheumatism. He had since then thrown ordinary physic to the dogs, and taken to the Hygeian: he had become the great Hygeian agent in South Devon, and was deemed of so much importance by his principal as to be brought up to London to give his testimony at the inquest. The opinion elicited from him, on cross-examination, with reference to the efficacy of *the* pills, was peculiar; at least we never heard the like expressed of any medicinal substance equally *harmless*. An old distich repeated, not always very trippingly on the tongue, especially when repeated rapidly, concerning tobacco,

> "—— If you be well, it will make you sick—
> —— 'twill make you well, if you be sick,"—

comes pretty near Mr. Tuthill's affirmation, or oath rather, about Morison's pills:—" 'Pon my soul," says the respectable witness, " if you were to take a dozen boxes of them, they would only make you sick; and if you are sick, you have only to take more of them and get well again!"

Such are the samples of *medical* men who have taken up " hygeiaism," and who no doubt expect to make it a profitable thing: we would beg leave to offer them a little advice, however — and that is, to take care lest they find it ere long a dangerous speculation. The coroner begins to come very often into somewhat awkward contact with the " agents," and verdicts of " manslaughter," followed by fine and imprisonment, are not so very unfrequent. It may not be pleasant, neither, to exchange Exeter, or Exeter Hall, for one of his Majesty's gaols. These hints we throw out to the new hygeists, as proper for them to chew the cud upon. To *them* we say nothing of the position in which they place themselves in the eyes of that profession to which they claim to belong; they have taken their measures deliberately, and, of course, are determined to abide by them. But to the profession itself we have a word or two to offer.

Quackery stalks abroad in more gross and disgusting form than it has ever before assumed; it bids defiance to the regular practitioner; it denounces him as the slave of ignorance and prejudice; and while it associates with it a few unworthy individuals, who boast to belong to the medical corporations, it flies in the teeth of those very bodies corporate, from which all well-educated medical men derive their privileges to practise.

Are professional men to look on with apathy under this provocation? Are those lawless vagabonds to be suffered to cajole the public—to tamper with, and often to destroy, the lives of his Majesty's subjects—and to insult the regularly-educated practitioner at the same time? Forbid it, prudence! forbid it, common sense! The proper course, then, is manifest: let every man in the profession exert himself within his sphere to expose the folly—the wickedness, of quackery: let him put away his contempt for charlatanism, for it has reared itself, through his sufferance, to a height not to be despised: let him attack it in its strong holds, so that if it be not extinguished in the contest, it may at least receive a check salutary to itself, and conducive to the public good.

CONVERSAZIONI AT THE COLLEGE OF PHYSICIANS.

THE meetings at the College of Physicians commenced for the season on Monday evening, on which occasion the room was, as usual, crowded to overflowing. The learned President occupied the chair, and was supported by the two Archbishops, the First Lord of the Treasury, and several other of his Majesty's Ministers,—by many dignitaries of the Church and of the Law, as well as by an immense assemblage of distinguished persons.

Sir Henry Halford read a paper on the Deaths of some Eminent Philosophers of Modern Times, which was received with much approbation: it formed a continuation of the series of essays on analogous subjects which the learned author has each succeeding year so skilfully adapted to the audience assembled on these occasions. (See a full report of the paper at p. 859, *ante*).

We cannot, however, allow the present opportunity to pass without remarking, that the members of the College ought either to support these meetings in a more efficient manner, or to discontinue them. That the assembling within its walls, at stated periods, of so large a number of the most eminent men in the kingdom, both as to rank and reputation, has a tendency to raise the College itself in public estimation, no one can reasonably doubt; — their presence is a tribute to the profession of physic, the influence of which becomes extended to its members. We should have expected to learn that all the Fellows gave to these meetings their most cordial support, and that the difficulty was not in procuring papers, but in selecting from the abundance of them. Such, however, was not the case last season; and it is on this account that we venture to express our astonishment that an opportunity so favourable of supporting the general interests of their body, should be so utterly disregarded; and that those who invite the scientific and literary public to become their guests should take so little trouble about the intellectual part of their entertaintment. At present the very existence of the meetings depends wholly and exclusively on the exertions of Sir Henry Halford, whose success in diversifying the subjects of his papers, and giving *eclât* to these conversazioni, has been very remarkable; but with his Presidency, unless there be some great change, the meetings must at once cease, and probably for ever.

REMUNERATION OF MEDICAL WITNESSES.

The principle of remunerating medical witnesses was acknowledged last session both by Lords and Commons; and the clause of the County Coroners' Bill for fixing the amount to be paid at from 1*l.* to 2*l.* was carried, and would have become the law of the land, but for the unexpected impediment of some circumstance connected with the forms of parliament. It was understood at the time that the bill was to be reintroduced, and there is, we believe, no doubt of it passing during the present season. Meantime, however, the *honourable* member for Finsbury, anxious, if possible, to gain some credit, has taken this part of the matter out of the hands of Mr. Cripps, the first mover, and is about to bring in, as a measure of his own, and as if it had been his own original design, that provision of the Coroners' Bill which secures a due remuneration to the medical witness, and which parliament last session declared its intention to afford. This is a shabby trick towards those to whom the medical profession is really indebted for first bringing the matter forward; but as it is of little consequence through whose medium the boon is conferred, we most earnestly recommend our brethren to petition in favour of the proposed measure. Such interference on their part is now rendered absolutely necessary, because the circumstance of this fragment of the bill having been taken up by Mr. Wakley, brings its passing into a degree of jeopardy which no one could have anticipated, and which it will require all the influence of medical men with their parliamentary friends to counteract.

THE LATE DR. JOHN CHEYNE.

We regret to announce the death of Dr. Cheyne, formerly of Dublin, and Physician-General to the Forces in Ireland. He was well known to the profession by his various practical dissertations, particularly those on Hydrocephalus, Croup, and other Diseases of Children; by his works on the Pathology of the Pharynx and Bronchia, and on Apoplexy and Lethargy. He also published, conjointly with Dr. Barker, an account of the Epidemical Fever in Ireland; and contributed various papers to the Dublin Hospital Reports, and to the Cyclopædia of Practical Medicine.

Dr. J. Cheyne was born on the 3d February, 1777. He was originally of Leith. After having been some years Surgeon in the Artillery Corps, he returned to Leith, where he obtained considerable practice. In the year 1795 he took the degree of M.D. at Edin-

burgh. Having married an Irish lady, he was induced about the year 1809, to settle in Dublin, where he obtained the appointment of Professor of Medicine, and became Physician to the Meath Hospital. The confidence of the profession in his experience and excellent judgment, with his delicate and honourable conduct, drew him into the highest and extensive practice, and obliged him to resign his public duties, except those belonging to the important post of Physician-General, to which he was appointed in the year 1820. For many years his professional receipts were very large: but his health broke under the fatigue. When in a state of great debility, a domestic calamity occurred, which seemed quite to overwhelm him: one of his sons was accidentally shot while on an excursion in the country; and unfortunately, as Dr. Cheyne hurried from town to his assistance, the postillion rode over and killed a child. These distressing circumstances, we have reason to believe, hastened his retirement from Dublin; his departure from which metropolis drew forth public expressions of regret from all the medical bodies. He brought his family with him to England, and bought a property in Buckinghamshire, where he lived in retirement during the last few years. He died, after an illness of some months, on the 31st January, 1836.

Dr. Cheyne was of the most amiable disposition, joined to a pleasant humour. He felt himself sinking, without losing his natural cheerfulness, and died in the confident hope that he was entering on the better part of his existence.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY,

Tuesday, Feb. 23, 1836.

HENRY EARLE, ESQ. F.R.S. IN THE CHAIR.

THE first paper read this evening was from the pen of Mr. Buller, detailing the particulars of a case of *Dislocation and Fracture of the Lumbar Vertebræ*. A preparation of the bones was exhibited.

At the request of the President, a paper on nearly the same subject, by Mr. R. A. Stafford, was read: it gave an account of a case of *Dislocation and Fracture (?) of the eleventh and twelfth Dorsal Vertebræ*.

It appears that, in August last, the patient, a man 31 years of age, was occupied in cleaning the windows of a first-floor; when, feeling himself become giddy and about to fall, he made a spring backwards, to clear the iron railings of the area beneath: he fell upon his buttocks, and was for a short period senseless. A crowd collected, and Mr. Stafford happening to be in an opposite house, he immediately ran across to the man's assistance. By this time those about him had raised him from the ground, supporting him on his feet. He was extremely pallid, and complained of severe pain in his back, but he had walked one or two steps. He was immediately carried into the house and laid upon a bed. Mr. Stafford examined him. His body was bent forward, and slightly twisted towards the right side. At the eleventh dorsal vertebra the spine projected, and between the spinous process of that bone and the spinous process of the twelfth dorsal vertebra there was a chasm, into which the three first fingers could be placed with ease, being in measurement full two inches in length. The superior parts of the vertebral column projected over the inferior; so that the lower part of the body was carried a little forwards, and the spine was not in the same line as it is in its normal form. There was no paralysis or numbness in the lower extremities, and no loss of motion, excepting the weakness which would naturally ensue from such an accident. The bladder and rectum, as will presently be seen, possessed their full powers, and no other symptom existed to denote so severe an injury to the spine, excepting the pain in the back.

As the slightest movement of the body might have displaced the vertebral column still more (for it was already torn), and thus wounded the medulla, the man was carefully laid upon a litter and carried to the St. Marylebone Infirmary. He then was cautiously placed upon his back on a double-inclined plane bed, at its lowest angle. He could not, however, at first bear this position long, from the extreme severity of the pain of the injured part. He therefore was occasionally moved on his right side, which was done with the greatest care. He was cupped on the injured part as often as the symptoms required the loss of blood, purged, and kept strictly on the antiphlogistic regimen; taking diaphoretics during the febrile action. When the violence of the pain had subsided, which was in about four or five days, he was placed entirely on his back and kept perfectly quiet. The chasm between the eleventh and twelfth dorsal vertebræ gradually lessened, the pain subsided, and the patient in three months left the Infirmary quite well, except the deformity, and being weak in the back.

During the whole treatment, no unto-

ward symptom happened: the bladder and rectum performed their functions naturally, and no paralysis whatever occurred.

It would (as Mr. Stafford observed) be extremely difficult, without a dissection of the spine, to account for the displacement of the vertebræ in this case. The author endeavoured to explain the occurrence in this way:—That in consequence of the sudden jerk given to the back, in endeavouring to clear the palisades (for the man acknowledges that he made an effort, even during his descent, to do so), that the action of the muscles tore asunder the intervertebral substance between the eleventh and twelfth dorsal vertebræ, fracturing or dislocating, at the same time, the articular joints; and hence the gaping or chasm between the two spinous processes. Fracture of the body of the vertebra, with dislocation or fracture of the articular joints, might also have produced the same phenomenon.

The most remarkable feature, perhaps, in the case, is, that the medulla received no injury, and that there were no symptoms at any time to show that there was either pressure upon or any lesion of the cord, or its membranes.

Mr. Earle exhibited the fractured vertebræ to which he had called attention at the last meeting. They had since been sawn longitudinally, and now left no room to doubt that their conformation was owing to a complicated fracture, and not disease of the bones. The spinal cord, at one place, must have been compressed into the calibre of a goose-quill.

A desultory conversation now arose on the subject of injuries of the spine; towards the conclusion of which, the President suggested that it would be desirable to have a paper drawn up containing the joint experience of several of the members; and he thought that this might be best effected in the manner of the old French Academy—by a commission selected from among the Fellows of the Society.

The reading of a paper *On an anormal Structure of the Valves of the Heart*, by Dr. Kingston, was commenced; but, in consequence of the lateness of the hour, it was discontinued—to be renewed at the next evening meeting.

CLINICAL LECTURE

ON

TIC DOULOUREUX;

Delivered at the Middlesex Hospital School,

By Sir Charles Bell.

Before leaving this hospital, I mean to give you some clinical remarks; a practice which I have pursued for one-and-twenty years: it was my earliest duty, and it shall be my last, to the pupils of this hospital.

There is an indescribable pleasure in reflecting on the successful treatment of diseases attended with pain amounting to agony. On Thursday last re appeared a patient (Charles Delafield) in whom some of you were much interested during the early part of last summer. He presented himself a miserable object; his head surrounded with a night-cap and rolls of flannel, which almost hid a face pale and wasted with incessant pain. Seeing him so proper an object of the charity, I gave him a letter, and wished him to come into the house. He expressed himself grateful, but he dared not; for he could not bear the restraint even of lying in bed, and had no relief from pain but in continual work in his business, which was that of a carpenter. His complaint was *tic douloureux*, and of that most severe kind which fixes in the centre of the cheek: it came like a flash of lightning upon him. I exhausted my little store of remedies, and still he returned, not weekly but daily, a miserable object—a study for the painter, if he desired to design "the last man,"—a man despairing.

After some weeks of attendance, one morning (whilst I was surrounded with the out-patients) this man, not waiting his turn, burst through the crowd, calling out he was cured! This, no doubt, he did from his confidence in the interest young and old had taken in his sufferings. I knew not what I had given him, but on looking to his card, I found the following:—

℞ Ol. Tiglii. (Croton) gtt. j.; Mas. Pil. Colocynth. Co. ʒj. Misce et fiant. pil. xii. Mitte. Pil. Galban. Comp. xii. One of the purgative pills and two of the gum pills to be taken on going to bed.

The pills operated quickly, and rather violently, upon him; but he continued them; the pain leaving him, and a remarkable change taking place for the better in his countenance, no doubt from his obtaining sleep as well as freedom from pain.

Before I go further, I shall recal your attention to the pathology of this complaint, and venture to repeat what I formerly stated to you. It has appeared to me surprising that authors have omitted to found on the anatomy of the nerves, which leads so directly to the satisfactory explanation of the symptoms in this disease. The sympathetic nerve we have seen to be a whole system of nerves spreading every where, possessed neither of sensibility nor power over the voluntary muscles; it is nevertheless acknowledged to

have important offices in controlling and combining the whole economy of the system, and to have its centre in the abdominal viscera. The very circumstance of its affording no phenomena like other nerves, should lead us to conjecture that, as this system resembles in structure the nerves of sensibility and motion, it must have powerful, though secret, influences.

I was careful to point out to you that the connexions of this system, or (if you will) of this nerve, are universal; but that the habits or mode of demonstrating it, leads us to pay more attention to the branches which extend into the head, though neither larger, nor probably more important, than those which extend into the plexus of the axilla, or into the sacro-ischiatic nerve.

Are we to admit or to deny this influence of deranged bowels—of visceral irritation—in producing external pains, local paralysis, or partial spasms? No man who attends to disease can deny the existence of this influence. Taking this as admitted, the line of connexion is clearly laid down in the anatomy.

Nor can we deny, I think, the effect of the confluence and mixing of internal nerves with such as go to parts external and exquisitely sensible; and that, through this connexion, external pains become significant of internal disease, or more commonly of irritation and disordered function.

One step further in this inquiry. The fifth nerve is the most exquisitely sensitive of all the nerves of the frame: the sensibilities it bestows are enjoyed in a higher degree than those produced through any other nerve of the system. It is also the seat of most severe pain.

Impressed with these facts, the moment that we see the map of the relations of the sympathetic nerve with the second division of the fifth, by a large and direct branch, and lesser connexions of the same nerve with all the branches of the fifth, we surely need look no further in explanation of the frequent and intimate dependence of a painful affection of the face upon the state of the digestive organs.

It is rather remarkable that Mr. Abernethy, who did so much to direct the attention of the profession to the influence of the stomach and bowels on local affections, should have abandoned his ground on the occasion of the triumph of the principle. I allude to that passage of his work when he writes, "I shall only say, that to me tic douloureux appears, in general, to be as much a constitutional affection as gout, or rheumatism; and that constitutional treatment is that which seems most likely to be of advantage in this as well as in nervous affections generally."

Most certainly the mere exhibition of blue pill and the bitter draught (though they will alleviate) will not cure the painful affection of the great nerve of the face. But consider the length of the intestinal canal: above all consider how strangely distinct portions of that canal are affected by different medicines. Does not this imply a distinction in portions of the tube, which may, in their disturbed condition, affect remote parts, and with various effects? This, I confess, has long been my opinion; and that although the common means of relieving a headache, or a megrim or clavus, may fail in this, yet that we ought not to despair of finding a purgative which, peculiar in its properties and effects, may reach the seat of this irritation, and may consequently influence the tic douloureux; and what more likely than the croton, in proper combination? I was acting under this conviction when I prescribed the croton oil.

But let us return to the result of experience. Since the period when Delafield appeared suddenly among us, like him who drew Priam's curtain, I have had four cases of pure *tic*, cured by the same means.

The first instance was in the lady of the Rev. R. H., of Kent. She sent for me to the hotel; she described her sufferings, the nature of which I shall presently narrate. I prescribed for her, and took the precaution of recommending her to shew the prescription to her medical friend in the country, and to make him a party to the proceeding. After some time she returned to town, with the most animated expressions of the benefit she had received; and on Friday last I saw this lady's husband, who spoke only of the fear of the return of the disease.

I shall state a case almost within your own sphere of knowledge, since the lady was attended by Mr. Wood*, late house-surgeon, and who shews his regard for the old hospital by being occasionally with us:—

Mrs. W. came to Gordon's Hotel, to be under my care and Mr. Wood's. She looked miserably, being continually in dread of pain. The attack begins by a deep and agonizing pain on the right side of the nose or cheek; it then ranges to the back of the head and neck, returns through the roof of the mouth, and fixes in the place first affected (the canine tooth and lateral incisor tooth are exquisitely sensitive); and this is its course invariably. "The pain is periodical; it has not varied, in the time of its return, five minutes for eleven weeks. It attacks her at one o'clock in the morning, whether she be asleep or awake. Last

* Now practising in Bolton-Street.

Sunday night she sat up till one o'clock, to see if she could prevent it; but it came at the appointed time. It continues from one till eight, when it gradually abates; during all this time she cannot remain in bed, but walks about the room. The attack recommences at half-past ten in the morning."

In conjunction with Mr. Wood, I gave this great sufferer the combination of pills I have mentioned to you; and the report after three days was to the effect, that "the pain does not come on till three hours after the usual time of attack, which time she has gained for refreshing sleep."

She went for a week to the neighbourhood of Windsor; returned in a few days; resumed her pills, the ol. tiglii being increased to the sixth of a drop in each pill. She had also a comforting draught, with infusion of gentian and of cloves, with tincture of orange peel and spirit. ammon. aromat; and on my second visit after her return, I had the pleasure of seeing her in great spirits, and entirely free from pain.

On the 17th of this month I took this note of a lady's suffering from tic douloureux:—"She acknowledges to have bad health; she has weak digestion, flatulence, no acidity, but torpid bowels constitutionally, which obliges her to take a little pill. She thinks her complaint commenced from cold, caught in driving in an open carriage; it has continued fourteen weeks; it is a violent pain on the cheek-bone, in the eye, and in the temple. The pain is so violent and so sudden, that she compares it to what might be the effect of a blow with a hammer on the eye; it comes on whilst she is sitting at dinner; the eye pours out water. The pain usually continues from one till five in the afternoon. She is well at night, and passes the hours of sleep tranquilly; she has swallowed pounds of iron; has used veratria externally."

I hope to report favourably of this patient; having recommended her to continue under her excellent physician, with the hints as to medicine which I have given you. I need hardly add, that before these patients came to me, they had tried bark, steel, ammonia, arsenic, belladonna, and iodine, and all the usual remedies to correct digestion. The belladonna and blue pill was the medicine which the late Dr. Warren depended on. The external application which the last patient found to be most beneficial, was laudanum and obacco infusion.

Some have appeared to me to confound the disease of the nerve with the true tic; and I would preserve the name, to distinguish it from various other painful affections of the face, necessarily seated in the fifth, which is the only nerve that bestows sensation here. We have had in the hospital, patients suffering from excruciating pains of the face, from disease of the parts surrounding the nerve; and no doubt similar pains arise from disease of the bone; but you will have no difficulty in distinguishing the true tic, by the suddenness of its attack, by the perfect intermission, and more especially by the circumstance of the sensibility of the parts, and the action of the muscles, being unimpaired; whereas in those painful affections which arise from the actual presence of disease, there is numbness of the parts supplied by the branch of the fifth nerve which is affected; or the nature of the complaint is evinced by the inflammation spreading to the third nerve, and affecting the eye-lid.

I must not dismiss this subject without adverting to the opinion expressed in a work which is in your hands—the Lectures of Sir Astley Cooper, by Mr. Tyrrell. I allude to the passage where he describes the operation of dividing the branches of the fifth nerve; and not these only, but the portio dura of the seventh nerve, which is described as the most frequent seat of this disease. I wish Sir Astley Cooper had looked to this passage, and given it the guard of a note: nobody is better aware of the effect of the division of the portio dura, since I formerly told you that, directly on the publication of my first paper on the nerves, he furnished me with examples of the effect of cutting it across.

But the division of the branches of the fifth, though it has been practised by every surgeon of eminence in the last half century, is not to be copied. It produces only the effect of a severe impression; the advantage is temporary, if any, and the root of the evil is not reached.

The complaint the most likely to be mistaken for the true tic douloureux, is that pain felt on the superficial branch of the fifth consequent on disease of the bone or sockets of the teeth influencing an internal branch of that nerve. But this can be ascertained by careful observation.

On the whole, that disease which is marked by the sudden and violent pain—as, when a patient, in speaking to you, starts as if lightning had struck him, and applies his finger to his face and to the branches of the fifth pair of nerves, the infra-orbitary or superciliary holes, or angle of the eye, and when he remains thus holding his head, speechless, and rocking on his seat—such a pain will not be mistaken for the disease in the nerve: it is the true *tic*, as I must presume depending on remote irritation.

To sum up, I feel authorized to say,

that the tic douloureux is of that class of pains where the irritation of internal parts affects an external and sensitive nerve; where the disease is not actually seated in the nerve, but results from a remote irritation. Nor is it the consequence of disease embracing the trunk of the nerve affected; as in the patients from whom these preparations before me were taken. I feel equal confidence that the source of the disease is in the abdominal viscera—not arising from disease otherwise formidable, but rather from disordered function, which I apprehend to be the reason why patients suffer for a long succession of years unsubdued in strength, unless by watchfulness and the exhibition of poisonous medicines which are in vogue.

I have stated to you that I have had five cases in succession of painful affections of the face, varying considerably in their character: some where the pain shoots suddenly, and for a short period, and coming irregularly: others strictly periodical, and these cured by the very simple means I have told you; the sixth may be a failure. But these are sufficient to countenance the opinion I have offered; and that you are still to seek the means of relief by correcting the morbid condition of the viscera.

LECTURES

ON THE

DISEASES OF THE NERVOUS SYSTEM.

By M. Andral.

As published in the *Gazette des Hôpitaux*, with the approval of the learned Professor.

HÆMORRHAGE OF THE NERVOUS CENTRES.

Effects produced by Cerebral Hæmorrhage on Digestion—Circulation—Respiration—Secretion—Generation—Various Complications of Symptoms—Certain Rare Affection—Duration and Progress of Cerebral Hæmorrhage—Treatment of the Attack, and of the Symptoms which remain after—Prophylaxis.

We have next to pass in review the various affections which are produced in the functions of nutritive life, by hæmorrhage of the nervous centres.

Digestion presents no marked change except in complicated cases. I ought, however, to remark that there is constipation of greater or less obstinacy; but I already pointed out this while speaking of the muscles of the rectum, the contractility of which is diminished, or altogether abolished. The mucous membrane of the alimentary canal also becomes less sensible, and it is astonishing to witness how doses of purgatives are now borne, such as would under other circumstances have been inadmissible.

Even the *Circulation* offers nothing which is precise or constant, so that the state of the pulse admits of no general description. Sometimes, at the moment of the hæmorrhage occurring, it is strong and vibrating; at others, it is small, weak, and compressible. These characters of the pulse require great attention in reference to the treatment. Some remarkable circumstances have been observed with respect to the circulating system, and great importance has been attached to the condition of the vessels in the face, as indicating the serous or sanguineous form of the effusion. But this assumption does not hold good, for all those who have hæmorrhage of the brain have not redness or flushing of the face; on the contrary, some of them have the countenance remarkably pale. In sanguineous apoplexy, a certain degree of suffusion of the eye has been frequently observed; and, indeed, hæmorrhage into the brain has frequently been preceded by bleeding from the mucous membranes.

Respiration is not very notably affected where the cerebral hæmorrhage has been trifling: but this does not hold good with respect to the mesocephalon, however inconsiderable the hæmorrhage. The slightest lesion of this, or of the *tuber annulare*, produces an immediate and direct modification of the function of respiration. I do not find that any specific observations have been made with regard to the influence in these respects, of hæmorrhage into the cerebellum. I have said, that if the bleeding into the hemispheres be trifling, no modification of breathing is produced, but if it be considerable, this function may be impaired to as great an extent as in effusion into the mesocephalon or medulla oblongata.

Secretion—requires no comment, unless, indeed, it be that the urine is not voided with the usual facility, owing to the paralysis of the bladder, as I have already stated.

With regard to the *Generative organs*, it has long been remarked that erection takes place in certain hæmorrhages. No account was taken of this phenomenon, and it was confounded with what happens in strangulation; it was attributed to asphyxia and turgescence of the venous system, till Gall came, and placed the organs of generation under the influence of the cerebellum. Proceeding upon this idea, some persons have endeavoured to shew that erection always took place when the cerebellum was the seat of the effusion. What number of cases are there in support of this opinion? Seven instances have been published, in which erections were present

simultaneously with hæmorrhage of the cerebellum; but let me add, that such erections have also been met with in other cases, where there was no such hæmorrhage. In the seven cases alluded to it was the middle lobe which was affected. Six of the patients belonged to M. Serres, who has recorded them either in his Anatomy of the Brain, or in Magendie's Journal de Physiologie. The other case, related by M. Guyot, also presented hæmorrhage of the middle lobe. One of those related by M. Serres was very remarkable, inasmuch as a woman, 70 years of age, who had hæmorrhage of the middle lobe of the cerebellum, had the menses return at this advanced period of life; and at the time of her death had the uterus filled with blood, and the ovaries injected.

The preceding cases are unique, and for myself I have not found a single instance in which any affection of the genital organs has been coincident with hæmorrhage of the lateral lobes of the cerebellum. But frequently, when the middle lobe is diseased, the medulla oblongata in which it lies participates in the change in such manner as to render it very difficult to say from what point the symptoms originate. On the other hand, whilst there is paralysis as to sensation and motion in the limbs and other parts, the erection alluded to must be regarded as of a nature quite opposed to palsy. Besides, this erection is produced without any lesion of the cerebellum—as for instance, by certain inflammations of the spinal cord; and M. Segalas, by touching the spinal marrow at a particular point with a needle, was able to excite erections, and even emissions. Certain accidental productions, such as softening, &c. have sometimes produced some change about the genital organs; but at present I speak exclusively of hæmorrhage.

Complication of symptoms. — I shall here conclude what I have to say of the symptoms which usually attend hæmorrhage of the nervous centres. But these symptoms do not always present themselves with the marked characters I have described; they may also be complicated with other phenomena, or be replaced by symptoms altogether new, depending either upon affections of the cerebral mass, or of other parts, the lesions of which then become mixed up with those of the nervous system.

As to those complications which are connected with the nervous centres, they may present themselves in affections of the intellect or motion. Thus in some persons there are cramps, which are not to be regarded merely as produced by the effusion, but by the inflammation which surrounds it; at other times convulsions are observed, which come on at intervals, and affect only the paralyzed limbs. Sometimes, one side of the body being paralyzed, the other is a prey to convulsions; but these convulsions do not depend upon the effusion—they arise from the inflammation of the meninges in its neighbourhood. Are we to regard these convulsions as always depending on inflammation? When we bleed patients for apoplexy, we sometimes see frightful convulsions come on while the blood is flowing, so that we are obliged to tie up the arm again. In such cases the bleeding cannot have induced inflammation, and the convulsions are to be attributed to the too sudden abstraction of blood.

Certain other more rare effects. — I have two or three times seen individuals who, at a longer or shorter period before, had been affected with cerebral hæmorrhage, and who had ever since suffered from a singular necessity of walking directly forwards, this being occasionally interrupted by an imperative necessity of walking backwards,—the impulse in either case being inevitable. Either of the above modifications of motion may be present without the other. How are these curious facts to be explained? Magendie, in the course of his experiments, by cutting the part of the hemispheres behind the corpora striata, or by cutting these parts themselves, produced in various animals an irresistible necessity of moving forwards; if, on the other hand, he removed the cerebellum, the animals always ran back, as if seeking to avoid something which frightened them. Is there any thing analogous to these artificial lesions in the brains of apoplectic patients who present the same phenomena? Have lesions been found corresponding to these aberrations of motion? I do not think that any such cases, in man, are well authenticated, unless in one instance, wherein lesion of the cerebellum was found in a patient who always had a great tendency to walk backwards. As to the rest, this disposition to walk either backwards or forwards is extremely rare.

Sometimes the hæmorrhage has been found in one of the peduncles of the cerebellum. It is well known that, according to the experiments of the same professor, if those parts be cut in one of the lower animals, it begins to turn round and round till it be completely exhausted. Accordingly M. Serres found on the dead body of a patient who had shewn this tendency to wheel round and round, a hæmorrhage into the right peduncle: this case, so far as I know, is unique.

I have also said that there were some unwonted phenomena to be mentioned in reference to the intellect; that is to say, some which are wanting in the great majority of cases. Among these is a violent

delirium, which shews the presence of an inflammation of the brain or its meninges; and this may take place either on the first appearance of the hæmorrhage, or some days after the attack. A certain degree of dulness, or somnolence to a greater extent than I described under the general symptoms, may also be occasionally met with.

If we now turn our attention beyond the nervous centres, we shall find various acute or chronic inflammations taking place simultaneously with these hæmorrhages; and such inflammations present a character altogether peculiar, dependent upon the cerebral lesion. The most conspicuous circumstance is the tendency to adynamia, resulting from the compression, by which the vitality of the individual is altered. In such cases, sloughs form with great readiness on the points where the body rests for any considerable time.

Duration of hæmorrhage of the nervous centres.—This will vary according to the seat of the lesion. It rarely proves fatal at once; there is always some interval between the effusion and death—such as a quarter of an hour, one, two, three, four, five hours, &c. More usually the interval is yet longer; and when you see a person fall dead, as if shot, do not infer that it is a case of apoplexy, for this kind of death does not occur even from hæmorrhage of the mesocephalon: your suspicions ought rather to be directed to rupture of the heart, or great vessels. Sometimes, in such cases, nothing is met with, and then some vague hypothesis is created; but it is generally made in bad faith, or by persons who have not investigated the case sufficiently.

Progress.—Frequently, when it has once commenced, such a case makes constant progress one way or the other. Sometimes, from the attack till death, there is a succession of changes; the relapses are frequent, and it rarely happens that an individual who has once had an apoplectic stroke fails to die of the disease. The prognosis is therefore formidable in itself, and from the frequency and facility of relapses. As to the rest, the complaint presents great varieties, according to, 1st, its seat, which may be either in the mesocephalon, in the hemispheres, the cerebellum, or spinal marrow. 2dly, The nature of the symptoms: thus it may be with or without loss of intelligence; with or without various complications. 3dly, The intensity of the symptoms, which may be distinguished into weak, medium, and strong; and this old division is one which it is convenient to retain. 4thly, Upon the complications with other diseased states, which may take place either in the substance of the brain, or in the membranes, or in other organs.

Treatment.—The method of treatment may be distinguished into that to be adopted during the attacks, into that directed against the accidents which follow it, and into that intended to prevent a relapse.

1. During the attack the essential part of the treatment consists in the abstraction of blood, which ought to be had recourse to abundantly, both generally and locally, but particularly the former. From these we obtain a variety of effects: they present an obstacle to the continuance of the hæmorrhage, and they remove the congestions which have a tendency to take place all over the brain, and which, rather than the hæmorrhage itself, are the cause of death; they prevent inflammations, which otherwise become developed in a manner more or less speedy and violent. Besides all this, bleeding favours the re-absorption of the blood which has been effused. Some recommend opening the veins of the arm; others those of the neck; others those of the feet. Bleeding from these last is often difficult, and at all events it is usually easier to obtain blood, and to as great an extent as we please, from the arm. Magendie and some others recommended puncturing the occipital arteries. What is of most importance is to abstract a sufficient quantity; and we must not fear to take a pound: the English go even to the extent of two pounds at once.

But is it always necessary to have recourse to the abstraction of blood? I have found in my practice that it is so, and that two, three, or even four bleedings, are requisite; but if the symptoms do not then yield, and the patient becomes weak, the coma will only be increased by farther evacuation. I have seen some cases where nothing had been done, beyond applying a few leeches; and others, where only a clyster was administered; and yet the patients, after lying three days in a state of coma, recovered, all the symptoms disappearing, except the paralysis. This does not show that we ought not to bleed, but only that the state of coma may pass off without this remedy, and that bleeding, carried too far, may be productive of harm. The same holds good with respect to the inflammations of other organs, in all of which there is a point, beyond which bleeding fails to be of any service. In the pathological, as in the physiological condition, there is a certain period necessary to bring either a fatal or favourable termination. A certain degree of power is required by nature for accomplishing the breaking up and absorption of the clot: bleeding, practised judiciously, favours

this, but when carried too far, impedes it. If there be any one disease more than another in which nature makes an astonishing effort at reparation, it is in hæmorrhage of the brain. Bleeding from an artery has frequently been tried in these cases, and the effects have been much praised, especially in the temporal artery; but the facts are not sufficiently numerous, and I much prefer opening a vein. An American physician has practised arteriotomy at the wrist, in apoplexy, with success, but this does not seem to deserve imitation. Under particular circumstances, leeches may be applied to the thighs, the groins, or the anus. The head ought to be kept uncovered, and moistened occasionally with cold water, but the application of ice requires great caution.

It is necessary to act on the skin and the alimentary canal by medicines and clysters; but emetics, which were long erroneously regarded as useful, must be avoided.

Treatment of the sequelæ of apoplexy.—Have we the means of directly combating the paralysis? Certainly not, inasmuch as it is but a symptom. However, there is a period, long after the attack, when we think that all the clot has been absorbed, we may directly stimulate the paralytic limbs, or the nerves going to them; and what leads me to say so is, that cases have been met with in which individuals have died paralytic, long after the apoplectic seizure, in whom no lesion whatever has been found in the brain; or if any change, at all events this has been exceedingly slight. Under such circumstances the paralysis was doubtless perpetuated, merely because the patients had lost the habit of moving the limbs, and had made no effort to regain it. Electricity and nux vomica, employed too soon, may lead to a return of the hæmorrhage, and so cause the immediate death of the patient.

In old palsies it has been customary to try mineral waters of various kinds, and in various ways. Baths of all sorts are also used.

In those patients who shew a tendency to the adynamic form of disease, I find it useful to give them habitually some light tonic, as bitters, or steel,—directing my attention rather to the general system than to the head in particular. The means I have just mentioned restore to the system the degree of energy required for carrying on the absorption.

The *preservative* means consist in abstracting blood from time to time from those who exhibit signs of cerebral congestion. Without this condition the bleedings would be useless. The same remark also applies to various kinds of disease.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

February 18, 1836.

William Parker Hoare.
Arthur Noverre.
Charles Pickering.
John Dommett Bishop, Calne, Wilts.
James Robb, Ockbrook.
Thomas Prichard, Anglesea.
Thomas Norris, Wigan.
Henry Whittaker Baker, Ludlow.
George Smith, Bridlington.
Edward Headlam Greenhow, Tynemouth.

February 25.

Samuel Lamb, Stourport.
Samuel Nicholson, Manchester.
Thomas Young, F.L.S., Taunton.
C. H. M. Mules, Ilminster.
William Bingley Lamb, Wakefield.
John Field, Tong.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Feb.* 23, 1836.

Age and Debility	31	Hooping Cough	1
Apoplexy	7	Inflammation	15
Asthma	16	Bowels & Stomach	2
Cancer	4	Brain	4
Childbirth	3	Lungs and Pleura	4
Consumption	41	Insanity	2
Convulsions	24	Liver, diseased	1
Croup	1	Measles	4
Dentition or Teething	8	Mortification	1
Dropsy	17	Rheumatism	2
Dropsy on the Brain	10	Small-pox	7
Dropsy on the Chest	1	Stone & Gravel	1
Epilepsy	1	Thrush	1
Erysipelas	2	Tumor	1
Fever	3	Venereal	1
Fever, Scarlet	3	Unknown Causes	1
Gout	1		
Heart, diseased	3	Casualties	8

Decrease of Burials, as compared with the preceding week . . 66

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Feb. 1836.	THERMOMETER.		BAROMETER.	
Thursday . 18	from 31	to 40	29 96	to 30 14
Friday . . 19	26	37	30 18	30·24
Saturday . 20	16	36	30·25	30 34
Sunday . . 21	13	39	30·.6	30·11
Monday . . 22	29	43	29·94	29 69
Tuesday . . 23	28	45	29 63	29 56
Wednesday 24	21	44	29 40	29 16

Prevailing winds, N. & S.W.

Except the 18th and two following days, generally cloudy; a little rain on the 22d and evening of the 24th.

Rain fallen, ·125 of an inch.

CHARLES HENRY ADAMS.

NOTICES.—We have been obliged temporarily to postpone Mr. Portwine's letter, and several other communications, for want of space.

At p. 803, in Mr. Ure's paper, *for* "heals with a cicatrice," *read* "heals with a faint linear cicatrice."

WILSON & SON, Printers, 57, Skinner-St. London

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 5, 1836.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XXIII.

OF SULPHUR.

History, &c.—This substance must have been known at a very early period, though Stahl is the first who gave a definite value to the word *sulphur;* for the ancients used it to signify the general cause of combustibility, and, therefore, every combustible was said to contain it. By some this word is derived from *sal*, and *πυρ, fire;* by others, from *solum* and *πυρ*. Our common term, brimstone, seems to be a corruption of *brin* or *brenstone*, or, as Piers Ploughman writes it, *brynston*, and literally means *burning stone*.

Native state.—It is abundantly met with in nature, principally in the inorganic, but also in the organic kingdom. *Native*, or *virgin sulphur*, is found uncombined with any considerable portion of other ingredients, and is divided by mineralogists into *common* and *volcanic*. The first exists in the oldest rocks, namely, the inferior stratified ones (as mica slate), and the unstratified ones (as granite.) Volcanic sulphur abounds in the neighbourhood of volcanoes, of which it is one of the products. In the neighbourhood of Vesuvius there is a hill which has long been famous as a repository for this substance; I allude to Solfatara, denominated by the ancients *Forum Vulcani*, or the Court of Vulcan. It is a kind of half-extinct volcano, from which are constantly evolving sulphuretted hydrogen, muriatic acid, and aqueous vapour. (Fig. 92).

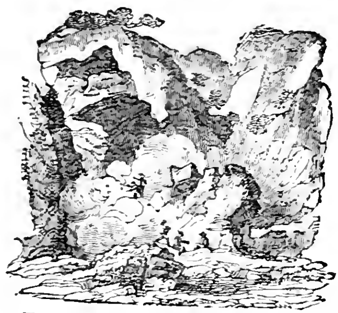

FIG. 92.—*Warm Spring of Solfatara.*

Sulphur is also found native in a state of combination. Thus sulphurous acid gas exists in considerable quantities near Ætna and Vesuvius; and sulphuric acid is found in the free state, as well as in combination, forming sulphates. Sulphuretted hydrogen, or hydrosulphuric acid, is developed from sulphureous waters, and from the soil in volcanic countries. Lastly, pyrites, or metallic sulphurets, are met with in great abundance: I need only instance the sulphurets of lead, copper, and iron.

Sulphur likewise exists in the organized kingdom; but I shall only cite garlic and eggs, as containing it.

Preparation.—It may be obtained in two ways,—by the decomposition of the native sulphurets, or by the purification of native sulphur.

(*a*) *Purification of native sulphur.*—To free native sulphur from its impurities, it is distilled in earthen pots (fig. 93, 1 1), arranged in two rows, in a large furnace; the top of the pot, which serves for the introduction of the sulphur, and for the removal of the residuum, being kept closed during the operation. The upper and lateral part of

each pot communicates with an inclined tube of about two inches diameter, and

Fig. 93.

four long. When the fire is lighted in the furnace, the sulphur fuses and sublimes, and passes through this tube into another pot (2), placed on the outside of the furnace, and perforated near its bottom to allow the melted sulphur to flow into a pail (3) containing water, where it congeals, and forms the rough or crude sulphur (*sulphur crudum*). This crude sulphur is further purified by distillation in large cast-iron vessels, and it then forms the *refined sulphur* of commerce. When fused, and cast into wooden moulds moistened by water, it constitutes the *stick*, *roll*, or *cane sulphur* (*sulphur in baculis*) of the shops.

For medicinal use it is reduced to a pulverent, or rather granular form, by sublimation from a large iron retort or pot into a chamber, or sulphur house. In this state it forms the *flowers of sulphur* (*flores sulphuris*), or *sublimed sulphur* (*sulphur sublimatum*). As a little acid is formed in the process, it requires washing, and then becomes *washed sulphur*, or *sulphur lotum* of the Pharmacopœia.

(*b*) *Decomposition of the metallic sulphurets.*—In some places sulphur is obtained by roasting or distilling the sulphurets of iron or copper. At the Paris mines in Anglesea, it is prepared by roasting copper pyrites; and the sulphur which is volatilized is collected in chambers, connected with the domes of the furnaces by means of horizontal flues. In Saxony and Bohemia, sulphur is obtained by distilling iron pyrites (*bisulphuret of iron*) in earthen tubes, placed in a long furnace or gallery. About one-fourth of the sulphur is disengaged by the heat, and passes into a receiver, where it is condensed in water; the residue is the sesqui-sulphuret of iron. The rough sulphur thus obtained is purified by fusion, which is sometimes twice or thrice repeated; the lighter impurities which rise to the top are skimmed off, and the liquid sulphur poured from the heavier sediment which falls to the bottom. The dregs remaining after the purification are called horse brimstone, or dregs of sulphur (*sulphur vivum*; *sulphur caballinum*; *sulphur griseum*.) This sulphur may be further purified by distillation, as mentioned in the last process.

Properties.—Pure sulphur is brittle, crystallizable, and, at common temperatures, solid; of a yellowish-green colour, without smell, and with a very weak and almost imperceptible taste. Its specific gravity is about 1·99. It is a bad conductor of electricity, and, therefore, by friction becomes powerfully electric. It is a bad conductor of heat, and when grasped in the warm hand crackles, and sometimes breaks to pieces. It is fusible and volatilizable; but the points at which these phenomena occur are variously stated. When heated to about 300°, in contact with atmospheric air, it takes fire and burns with a pale blue flame, and at the same time emits a large quantity of fumes having a peculiar suffocating odour (*sulphurous acid.*)

Sulphur is said to be a *dimorphous* substance; that is, the primary form of the crystals of native sulphur, or of those deposited from the solution of this substance in bisulphuret of carbon, is a rhombic octahedron; while that of the crystals obtained by the fusion and slow cooling of sulphur, is an oblique rhombic prism. Now these two forms are incompatible, and cannot be derived the one from the other: the first belongs to the *rectangular* or *rhombic right prismatic system*; the second to the *rectangular* or *rhombic oblique prismatic system*.

When sulphur is heated to 340°, it becomes viscid; and by increasing the heat the viscidity increases, until the temperature arrives at between 400° and 500°. If while in this state it be suddenly cooled, as by throwing it into water, it remains quite soft, so that it may be drawn out into threads. The cause of this change, which seems to be merely physical, is not understood. The atomic weight of sulphur is 16.

Characteristic tests.—Sulphur is easily distinguished from other bodies, by its colour, its fusibility, and its volatility, and by its burning with a blue flame, and the evolution of sulphurous acid gas, the odour of which can be easily recognised.

Impurities.—Rough sulphur is always mixed with variable quantities of foreign substances. Vauquelin distilled 200 grains, and obtained a residuum of 0·82, composed of silica, carbonate of lime, iron, bituminous charcoal, alumina, and magnesia, but the proportion of earthy matters is generally more considerable. Sulphur obtained from pyrites sometimes contains orpiment, (*sesqui sulphuret of arsenicum.*) The purity of any specimen is determinable by dissolving it in oil of turpentine, which does not act on the impurities.

Physiological effects.—In doses of one or

two drachms, sulphur acts as a very mild purgative, without exciting the vascular system, or occasioning griping. Given in smaller but repeated doses, it is said to operate as a stimulating diaphoretic. That it becomes absorbed is shown by the smell of sulphuretted hydrogen, which it communicates to the sweat, urine, and milk, and by silver articles in the pockets of patients, becoming blackened. Various ill consequences have been attributed to the continued employment of it. Thus it has been said to cause inflammation of the bowels; and in confirmation of this we are told, that in the Veterinary School at Lyons, it has been found that a pound of sulphur given to a horse brought on intestinal inflammation, and death. I know not what the effects on horses may be, but I believe it to be a very safe remedy for the human subject, and I say this from a somewhat extensive experience of its effects.

Uses.—Sulphur is employed both internally and externally.

(*a*) *Internally*—It is given for various purposes. *In affections of the rectum*, as stricture, hæmorrhoids, and prolapsus, it is a valuable agent as a mild purgative. I have frequently employed it when patients had great disgust for castor oil. In order to promote its purgative effect, it will be sometimes necessary to conjoin magnesia or the bitartrate of potash. *In chronic cutaneous diseases*, more especially prurigo, impetigo, and scabies, the internal use of sulphur is sometimes attended with great benefit. There are various other cases in which it has been recommended, such as chronic catarrh, asthma, gout, rheumatism, worms, &c.; but I think it is unnecessary to do more than allude to them. Some years ago it was proposed as a preventive against measles, though I believe the only real prophylactic is isolation.

(*b*) *Externally*—Sulphur is a most valuable remedy in various skin diseases, more especially seabies; and notwithstanding many substitutes have been proposed for it in the latter disease, none are so generally successful. Those who ascribe this disease to the presence of a small insect, the *acarus scabei*, explain the efficacy of sulphur frictions, by supposing that sulphur kills the little parasite. But before adopting hypotheses of this kind, first let the fact be proved that the itch is *caused* by an insect. It is unquestionably true, that in many persons affected with this disease, parasitic insects of peculiar characters have been found; but we have yet to learn whether these were causes, effects, or mere accompaniments of the cutaneous affection. Rayer says it is indubitable that the number of insects bear no proportion to that of the vesicles, and that it is rare to discover these insects on the abdomen and groin, although the eruption of scabies is here very common: moreover, he says, scabies is known to continue when no more acari are to be discovered.

Sulphur is also useful in various other chronic skin diseases, as porrigo.

Baths of the vapor of sulphur have been employed, according to Rayer, with advantage. Do not confound these with baths of the vapor of burning sulphur, that is, baths of sulphurous acid gas, which I shall notice hereafter.

Administration.—Internally it is usually given in the form of an electuary, or suspended in milk, in doses of from one to three or four drachms. The *lac sulphuris* is a more pleasant preparation for internal use than the sublimed sulphur. Externally sulphur is employed in the form of ointment. In the London Pharmacopœia are two sulphur ointments. The *unguentum sulphuris* is composed of three ounces of sublimed sulphur, and half a pound of lard. The other is the *unguentum sulphuris compositum;* it contains besides sulphur and lard, powder of white hellebore-root, nitre, and soft soap; but it is a very irritant application, and consequently is inapplicable in some cases. The great objection to sulphur frictions is their disgusting odour. The late Dr. Bateman recommends the following formula, as obviating "both the smell and sordid appearance of sulphur ointment:"

℞ Potassæ Subcarbonatis, ʒss.; Aquæ Rosæ, ℥j.; Hydrargyri Sulphureti Rubri, ʒj.; Olei Essent. Bergamot. ʒss.; Sulphuris Sublimati, Adipis Suillæ, aa. ℥ix. Misce secundum artem.

(*a*) *Precipitated Sulphur.*

History and synonymes.—This substance (called also *lac sulphuris*, milk of sulphur, and *hydrate of sulphur*) was known to Geber, the Arabian chemist.

Preparation.—Boil 1 part of sublimed sulphur with 2 parts of slacked lime in 8 parts of water. Filter, and add muriatic acid: sulphur is precipitated.

Theory.—The explanation of this process is very complicated. When sulphur and lime are boiled together, two salts are formed, namely, a bisulphuret of calcium, and hyposulphite of lime. The following formula will explain this part of the change:—

Re-agents.	Results.
6 S	2 (Ca + 2 S)
3 $\dot{Ca}$	$\dot{Ca}$ + (2 S + 2 O)

So that you see 6 atoms of sulphur react on 3 of lime, and generate 2 of the bisul-

phuret of calcium, and 1 of the hyposulphite of lime. On the addition of muriatic acid, both these salts are decomposed. By the action of 2 atoms of muriatic acid on 2 atoms of the bisulphuret of calcium, we obtain 2 atoms chloride of calcium, 2 of sulphur precipitated, and 2 of sulphuretted hydrogen.

Re-agents.	Results.
$2\,(Ca + 2\,S)$	$2\,Ca\,Chl$
$2\,H\,Chl$	$2\,S$
	$2\,S\,H$

1 atom of muriatic acid acting on 1 of the hyposulphite of lime, produces 1 atom of chloride of calcium, 1 of water, and 1 of hyposulphurous acid; which latter is immediately decomposed into 1 of precipitated sulphur and 1 of sulphurous acid.

Re-agents.	Results.
$\dot{Ca} + (2\,S + 2\,O)$	$Ca\,Chl$
$H\,Chl$	$\dot{H}$
	$2\,S + 2\,O \begin{cases} \ddot{S} \\ S \end{cases}$

Now the sulphuretted hydrogen (developed by the action of the muriatic acid on the bisulphuret of calcium), reacts on the sulphurous acid (formed by the spontaneous decomposition of the hyposulphurous acid), and forms water and sulphur.

Re-agents.	Results.
$\ddot{S}$	$2\,\dot{H}$
$2\,S\,H$	$2\,S$

However, from the smell, it is evident a portion of the sulphuretted hydrogen escapes unchanged.

Properties.—Precipitated sulphur agrees in most of its properties with sublimed sulphur, but is much whiter, and is in a finely pulverent form. Berzelius says, that when melted, it gives out a little sulphuretted hydrogen; and on cooling, resumes the yellow colour it had before boiling it with the alkali.

Composition.—It is composed of sulphur, with a little water; and hence it is frequently termed *hydrate of sulphur*. According to Bucholz, however, when well dried, it contains hardly a trace of water, and, therefore, any present under ordinary circumstances must be regarded as hygroscopic; so that the term hydrate is hardly applicable to it.

Impurities.—If sulphuric be substituted for muriatic acid, in the manufacture of this substance, sulphate of lime will be formed, and mixed with the precipitated sulphur. The impurity may be detected by volatilizing the sulphur in a crucible, or by solution in oil of turpentine or liquor potassæ.

The *effects*, *uses*, and *doses*, are the same as the common sublimed sulphur already noticed.

(*b*) *Sulphuretted Oil.*

History.— Sulphur is soluble in both volatile and fixed oils, and formulæ are still found in some continental works for the preparation of pharmaceutical compounds of them. The present is the only solution of sulphur contained in the London Pharmacopœia. It was formerly denominated *balsam of sulphur*.

Preparation.—It is prepared by boiling together two ounces of washed sulphur and a pint of olive oil. It cannot be regarded as a mere solution of sulphur in oil, since the odour of sulphuretted hydrogen which the compound possesses proves that the oil has undergone partial decomposition; in fact, the heat to which the oil is raised, to boil it, causes a chemical change.

Properties.—It is a dark reddish-brown viscid substance, having an extremely unpleasant odour.

Effects.—Locally it acts as an acrid irritating substance. Its remote action is that of a stimulant, causing thirst and febrile disorders. It has been supposed to possess expectorant and diaphoretic properties.

Uses.—It has been used as a local application to foul ulcers. For internal use, it is resorted to in pulmonary affections. Its unpleasant taste and smell in most cases preclude its employment.

The *dose* is from 10 to 40 or 50 drops.

Carbon.

History.—The term carbon, (from *carbo*, coal), was first employed by Morveau, Lavoisier, and Berthollet, to designate the pure matter of charcoal. To Lavoisier we are indebted for proving that by combustion in oxygen gas, the diamond and charcoal furnish the same product—namely, carbonic acid.

Native state.—Carbon is copiously met with in nature. When pure and crystallized, it forms the diamond: combined with oxygen, it constitutes carbonic acid, which is found in the atmosphere, in certain mineral waters, and in combination with bases (as lime): it occurs combined with hydrogen, either as carburetted hydrogen gas, or as bitumen: graphite or plumbago (which is carbon usually mixed with iron), is also a native form of carbon: lastly, carbon is a constituent of all organic compounds.

Properties.—It is solid, infusible, and not volatile. By combustion in oxygen gas, it

produces carbonic acid. Its other properties are so different, that three varieties of it are made:—

(*a*) The *diamond*.

(*b*) *Plumbago* or *graphite*, used on the continent as a medicine.

(*c*) *Charcoal*, also used in medicine.

(*a*) *Plumbago.*

History, &c.—The substance denominated *plumbago*, *graphite*, or *black lead*, was formerly regarded as a chemical compound of carbon and iron, and accordingly was denominated *percarburet of iron;* but the quantity of iron present is subject to great variation, and in some specimens from the Brazils there is hardly any, so that the iron is now regarded as an accidental substance. The finest specimens are brought from Borrowdale, in Cumberland.

Characteristics.—It is known by its physical characters, and by yielding carbonic acid when burned in oxygen gas. Sulphuret of molybdenum very closely resembles plumbago in appearance.

Physiological effects.—Various properties have been assigned to it, but further evidence is wanting to establish its action on the body.

Uses.—It has been employed both externally and internally in chronic diseases of the skin, such as herpes. When used externally, it is mixed with lard or some other fatty body. Internally, it has been given in doses of 12 grains to a drachm.

(*b*) *Charcoal.*

History.—More than three hundred years before Christ, Theophrastus described the method of making charcoal.

Preparation.—*Vegetable* charcoal is procured by burning billet wood, piled up in conical heaps, which are covered with turf and sand, to prevent the access of the atmosphere, a few holes being left near the bottom, and one at the top, to occasion a draft. *Animal* charcoal is obtained by distilling bones or ivory in iron stills: it contains phosphate and carbonate of lime, from which it may be freed by washing with dilute muriatic acid.

Properties.—It is black, inodorous, insipid, and brittle: a good conductor of electricity, but a bad one of heat; it absorbs various gases without effecting any chemical changes on them, except in some particular instances. One volume of boxwood charcoal can condense within its pores 90 volumes of ammoniacal gas, but only $1\frac{1}{2}$ volumes of hydrogen gas. Another remarkable property of charcoal is that of removing the colour of various animal and vegetable infusions; an effect which is commonly said to be of a mechanical nature, but an experiment made by Bussy would seem to prove that the colouring matter and charcoal enter into chemical union; the colouring matter acting the part of a feeble acid, the charcoal that of a weak base. The best kind of charcoal for decolorizing is that obtained from animal matters; and therefore, for all ordinary purposes, *bone black* (called in the shops *ivory black*) will answer the purpose. Charcoal absorbs organic odorous matters, thus destroying the odour of putrefying or rancid substances; but in this respect it is far inferior to chlorine and the chlorides.

Characteristics.—Charcoal is known by its combustibility in oxygen gas, the product being carbonic acid; and by its physical properties. The form or texture of charcoal varies with that of the substance from which it has been obtained.

Physiological effects.—Charcoal is, I believe, an inert substance; for Burdin has given a pound of it daily, without any other obvious effect being produced than the blackening of the stools. A variety of properties and virtues have been ascribed to it, as I believe, without foundation. It has been called anodyne, emmenagogue, tonic, purgative, &c.

Uses.—In this country, charcoal is used as a therapeutic agent, principally as an antiseptic, to absorb the fœtid odour evolved by gangrenous and phagedenic ulcers. For this purpose it may be used in the form of powder or of poultice. Its disinfecting and antiseptic powers, however, are much inferior to those of chlorine, or of the chloride of lime. As a tooth-powder it is a valuable agent, freeing the teeth from the foreign matters which cover them, and at the same time counteracting the unpleasant smell of the breath arising from decayed teeth or disordered stomach. Brachet states that it checks caries of the teeth. Internally it has been exhibited in various affections of the alimentary canal, such as dyspepsia, cardialgia, diarrhœa, and dysentery. The beneficial effects said to have been produced in these cases, can only be referred to the action of charcoal on the secretions of the bowels; an explanation apparently supported by Dr. Chapman's statement, that in dysentery, where the stools are highly acrid and offensive, charcoal entirely divests them of their bad smell and acrimony. In consequence of the advantage said to have been obtained by Dr. Calcagno, of Sicily, by the use of charcoal in intermittents, it was tried by Dr. Calvert, physician to the British forces at Palermo, and with success. In this country, however, it is, I believe, never resorted to in ague by medical practitioners. Charcoal has been recommended in various other diseases, but experience has not confirmed its efficacy.

Application.—In the Dublin Pharmacopœia there is a formula for a *charcoal cataplasm;* the simplest mode of preparing it is to mix charcoal with a common linseed-meal poultice: it is applied to foul ulcers, to destroy the odour.

PHOSPHORUS.

History.—This substance was discovered in 1669, by Brandt, an alchymist at Hamburgh; and received its name from being luminous in the dark (from φως, light; and φορεω, I carry).

Native state.—It is found in both kingdoms of nature, generally in the form of a phosphate. It is a constituent of the cerebral substance, and a French chemist some little time since asserted, that in madmen there is an excess—in fools, a deficiency of it!

Prepared.—It is prepared from bone ashes *(phosphate of lime),* by digesting them in sulphuric acid; by which sulphate and superphosphate of lime are procured. The latter is distilled with charcoal, which abstracts the oxygen from the phosphoric acid of the superphosphate, setting free the phosphorus, which is volatilized.

Properties.—It is a pale yellow, semi-transparent, crystallizable solid. Mitscherlich says the cystals are rhombic dodecahedrons; so that they belong to the regular or tessular system.

Characteristic tests.—Phosphorus in substance is easily recognized by the following characters: its waxy appearance, fuming in the air, being phosphorescent or luminous in the dark, and, by friction or gentle heat, causing it to inflame, and, lastly, by its burning with a most intense white light and smoke of phosphoric acid. A solution of phosphorus in oil, æther, or alcohol, may be known by its garlicky odour, and when rubbed on the skin, rendering the latter luminous in the dark.

Physiological effects.—Phosphorus ranks among the irritant and corrosive poisons. According to Orfila, its activity depends on its absorbing oxygen, and thus becoming converted into an acid which acts as a corrosive poison, like the other mineral acids. Hence, therefore, ætherial and oleaginous solutions are more active poisons, inasmuch as the oxidation of the phosphorus is effected more rapidly. On man it also acts as a poison; and in Dr. Christison's work you will find references to several cases: in one, $1\frac{1}{2}$ grain, in another instance, 3 grains, caused death. Cases, however, are reported, in which 6, 10, 12 grains have been swallowed without any hurtful effect; but doubts have been entertained as to the correctness of the statements. Thus the authors of the *Dictionnaire de Matière Médica'e* think the phosphorus employed must have undergone some chemical change. I once administered 16 grains of phosphorus to a man, without any hurtful effect; and judging from its physical characters, I should say the phosphorus was that usually met with in commerce. The man here alluded to was Chabert, some years ago renowned in London under the name of the "*Fire King.*" I carefully weighed out 16 grains, placed them in a spoon, and put them in his mouth; and he washed them down with a tumblerful of water. He offered to take this dose daily. How he counteracted the effects, I know not; but I suspect he excited vomiting, for within ten minutes after swallowing the phosphorus, he left the room for about a quarter of an hour.

Administered in small doses, it is regarded as a stimulating substance, exciting the nervous, vascular, and secreting organs. It has more particularly been supposed to stimulate the sexual organs, and to act as an aphrodisiac.

Phosphorus becomes absorbed. When a solution of it in oil is injected into the jugular vein of a dog, or into the pleura, white vapours of phosphorous acid are evolved from the mouth, and death shortly takes place; in consequence, as Orfila imagines, of the phosphorus absorbing oxygen, and, in passing through the pulmonary vessels, exciting almost instantaneous inflammation.

Uses.—Internally it has been employed in typhus fever; in some chronic diseases of the nervous system, as epilepsy, paralysis, melancholy, amaurosis, &c.; in cholera; in dropsy, &c. When resorted to now, it is more frequently as an aphrodisiac.

Administration.—Internally it is exhibited either in substance, or dissolved in oil or æther. But I believe the safest plan is to dissolve it in oil, and then make the oily solution into an emulsion, by mucilage. Thus digest for several days, one part of phosphorus, cut in pieces, in sixteen parts of olive or almond oil. A solution is thus obtained, which may be given in doses of from 15 to 30 drops, in mucilage or other convenient vehicle. This same oil, mixed with lard, may also serve for external use.

Antidotes.—Our objects are to stop the oxidation of the phosphorus, and to neutralize the resulting acid as fast as it is formed. To attain the first of these indications, give large quantities of mild demulcent liquids, so as to envelop the phosphorus and exclude it from the air. Magnesia is to be exhibited, with the view of neutralizing the resulting acid of phosphorus. Parts burned with phosphorus are to be washed with a weak alkaline solution, to remove any adhering acid which might serve to keep up irritation.

ILLUSTRATIONS

OF

THE PHYSICAL DIAGNOSIS OF DISEASES OF THE VALVES OF THE HEART, &c.

Read to the Harveian Society, Feb. 1st,

By Charles J. B. Williams, M.D. F.R.S. &c.

President of the Society.

Few departments of practical medicine have derived more improvement from modern pathology than that relating to diseases of the heart. Yet I apprehend there are few who have had much experience in the theoretical and practical study of this subject, who will not in candour admit that our knowledge of it is still in an imperfect state, and that what we already know should serve as a basis for future researches, rather than satisfy us with its present sufficiency. The now acknowledged frequency of idiopathic affections of the heart, is in itself a sufficient reason for our making their signs a matter of prominent importance; and the considerable progress which has been already made in the diagnosis of these affections has compensated for the labours by which this knowledge has been obtained. But we should unduly limit the value of the physical signs of affections of the heart, if we confine their utility only to those which are idiopathic. These signs are constituted by the physical properties of the heart and arteries, as exhibited in the performance of their functions; and when these properties are changed by other causes in the system, besides those which primarily or exclusively affect the heart and arteries themselves, these signs will be also changed in proportion. Now although it is not professed that the diversified modifications which the function of the circulation undergoes in the various diseases of the body in which it is affected, have all their distinctive signs, yet I may venture to say, that by the physical exploration of the heart and arteries, in addition to feeling the pulse (this being itself also a physical sign), we may often obtain a much more accurate knowledge of the state of the circulation than can be gained in any other way; and this knowledge is sometimes immediately useful in elucidating the pathology of the case, and in suggesting the appropriate line of treatment.

But this field, though rich in promise, has scarcely yet been entered; and my reason for naming the subject is the hope of inducing others to assist in its cultivation. If any one accustomed to auscultation will take the trouble to examine half a dozen patients of different constitutions, and labouring under various complaints, especially those of opposite states, the sthenic and asthenic, cachectic and plethoric, nervous and inflammatory, he will find in the character, proportion, and degree of loudness of the sounds of the heart—in the nature and extent of the impulse, both of the heart and of the arteries—and in the interval between the ventricular systole and the radial pulse—he will find in these such marked differences as will convince him of the existence of particular physical signs, and further study and observation will teach him the value of these signs in the diagnosis and treatment of diseases, when their other characters are deficient or equivocal. To introduce illustrations of this position now, would be premature, inasmuch as the simpler diagnostics of flagrant organic disease of the heart, are by no means generally appreciated; but in commending to the attention of the Society the utility of auscultation of the heart in general, I could not resist the opportunity of enhancing the commendation by pointing to further advantages, which I hope will ere long be fully developed and acknowledged.

To the successful study of the pathology of the heart's condition and function, a knowledge of its physiological state and phenomena is essentially necessary. The cases which I have now to submit to the attention of the Society, are meant chiefly to illustrate the views which I have elsewhere advanced with regard to the healthy and morbid sounds of the heart*. Even on these common forms of valvular disease, ideas more precise than those commonly expressed in published cases and hospital reports, must be entertained, before their more prominent signs can prove truly characteristic: and until greater distinctness and accuracy on the essential points be observed, the cases become

* The Pathology and Diagnosis of Diseases of the Chest, &c. 3d Edit. Part III.

but imperfect facts, encumbering rather than improving the history of the subject.

It might, at first sight, appear of little consequence to be able to distinguish between the different forms of valvular disease, since the general character and treatment of all much resemble each other; but this is less the case than is generally supposed, particularly in the earlier stages of organic lesions of the valves; and the utility of the distinction is still more apparent in the fact, that without a knowledge of it, we shall often be unable to discriminate between organic and functional disease, and between the signs of old structural lesions, and some of the characteristic indications of incipient pericarditis. For example, the leather-creak of pericarditis often resembles certain valvular murmurs, and sometimes they are only to be distinguished from it by a full knowledge of those characters which distinguish one valvular murmur from another. The following cases, which occurred chiefly at St. George's Hospital, will illustrate the only physical signs that I have hitherto found distinctive.

Diseases of the Aortic Valves.

Wm. Watson, æt. 45, labourer; a patient at St. George's Hospital, November 9th —Has been rheumatic and had palpitation for more than six years. Suffers now from severe palpitation on ascent or quick movement, from cough, dyspnœa in paroxysms, and great sickness. For the last five or six weeks his legs have been swelled. He is also occasionally attacked with severe angina, which generally attacks at night. Pulse now 72, full and thrilling, but regular.

A double sawing murmur accompanies the pulsations of the heart. The first sawing, accompanying the impulse, is loudest and harshest at the top of the sternum, is deeper below this and to the left; it is heard also in the carotids. A whizzing murmur is heard, instead of the natural second sound, most distinct at the lower end and to the left of the sternum. Both murmurs are slightly audible in the back. Impulse of the heart diffused.

Diagnosis, written November 12th.* — *Obstructive and regurgitant disease of the aortic valves; probably diseased coats of ascending aorta. Heart enlarged.* Perhaps disease of the pulmonary valves (?).

November 26th.—Died suddenly in the water-closet.

Sectio. — Nov. 28th. — Some bloody fluid in the pericardium. Heart much enlarged, especially the left ventricle, whose walls were an inch thick, and cavity double the natural size. Aortic orifice ossified at the roots of the valves. The free margins of two aortic valves much thickened and shortened; that of the third less so; they could not nearly close the orifice. Ascending aorta somewhat dilated, and studded with fibro-cartilaginous deposit, in some of which were osseous laminæ. One large one projected from the origin of the coronary artery, and must have been directly in the current from the heart. The other valves were healthy.

John Hetcher, a patient of Dr. Macleod, June 30th.—Reports that he was quite well until nine weeks ago, when, after exposure to cold when heated by violent exertion, he had cold chills and general pains, especially across the chest; for which he was bled. Ever since, his breath has been short, with dyspnœa and occasional hæmoptysis. Legs much swelled. Liver large. He is subject to bilious vomiting.

The whole cardiac region dull on percussion, on and to left of the sternum. A loud double sawing was heard instead of the natural sounds, and was most distinct, with pulsation, at the top of the sternum. It was heard also in the carotids.

Diagnosis, written June 30th.—*Regurgitant disease of the aortic valves; probably dilatation of the arch of the aorta.* (Query, disease of the pulmonary valves?) The extent of dulness on percussion led me to suspect also effusion in the pericardium.

July 2d.—In a fit of palpitation, the heart acting violently, and pulse in carotids tumultuous. Each pulsation consists of a first impulse with sawing sound, then a second sawing, then a second impulse with a short sound; there is then a short interval. The pulse at the wrist is feeble, with a second weaker intermediate beat, corresponding with the second impulse of the heart. This double pulsation seems to depend on the regurgitation from the aorta, which causes the second sawing, stimulating the heart to a second con-

* The diagnoses mentioned in these cases were communicated, at the time, to several pupils and others present.

traction. This paroxysm subsided suddenly, and the pulsations became simple and regular, though quick, and still attended with the to-and-fro sawing, without any of the natural sounds.

July 6th.—A pulsation in the jugular veins, especially the right. Add to the diagnosis, *dilatation of the right ventricle.* Died the next day.

Sectio, July 8th.—Great œdema of the lower extremities up to the abdomen. Heart more than double its usual size, and presenting a flat surface four or five inches long, and three inches wide, in contact with the anterior parietes of the chest. (Hence the dulness on percussion.) Right ventricle and auricle much dilated; the parietes of the latter very thin. Right auriculo-ventricular orifice admitted three fingers. Membranous centres of tricuspid valve thickened, with a yellow deposit, not so hard as cartilage. Left ventricle dilated to nearly three times its ordinary capacity; its parietes at apex $\frac{1}{2}$ an inch, at base $1\frac{1}{4}$ inch thick. Columnæ carneæ greatly enlarged, and contrasted remarkably with the thread-like cordæ tendineæ, which seemed more slender than natural. The membranous portions and margins of the mitral valve thickened with a substance almost cartilaginous, but were large and mobile, and seemed sufficient to close their orifice. Aortic orifice more than three inches in circumference at the root of the valves, one of which was large, the other two considerably smaller, and must have permitted regurgitation. All were thickened, and on the large one were two short projecting bodies, of the same fibro-cartilaginous material that thickened the valves. Aorta greatly dilated from its origin to the commencement of descending portion: its coats thickened and puckered with several osseous scales, some of which protruded slightly into the artery. The pulmonary valves were thin, and as large as the largest of the aortic valves, and seemed quite sufficient to close the orifice, which was $2\frac{3}{4}$ inches in internal circumference. There were some spots of pulmonary apoplexy in the lungs.

William Gregory, æt. 36, shoemaker, a patient of Dr. Chambers.

Dec. 1.—Says that he was quite well till about eleven weeks ago, when he was attacked with a bad cough, shortness of breath, and some pain in the chest, which have continued.

Dec. 3d.—Expectoration deeply tinged with blood; legs swelled; pulse 96, full, and strong, but immediately receding under the finger; impulse of the heart diffused, not abrupt. A double sawing sound, loudest at the middle of the sternum, accompanies systole and diastole of the ventricles. The first sawing is heard in the carotid and subclavian arteries, where it is short, hoarse, and deep; the second sawing, superficial and whiffing, at the middle and to the right of the sternum; the natural second sound (flap) scarcely if at all heard.

Diagnosis, written Dec. 3d.—*Regurgitant and obstructive disease of the aortic valves; probably diseased ascending aorta* (dilated?); *hypertrophy and dilatation (?) of left ventricle.*

Dec. 7th.—Pulsations now irregular, every third beat being earlier and slighter. The sounds are now triple, there being a third, like the first, but shorter; (a second systolic action excited by the regurgitation from the aorta, as in the case of Hetcher).

Breathing became more and more difficult, and he died on the 14th.

Sectio, Dec. 16th.—A whitish red spot of lymph on the apex of the heart, under the pericardial covering; the muscular fibre there darker in colour. Left ventricle hypertrophied, an inch thick in parts, at apex three-quarters of an inch; its cavity rather smaller than natural, but elongated. The aortic valves were much diseased. The free margin of one, and of a portion of another, had become separated from their bodies, and formed a cord across the mouth of the aorta. The membranous portions, which ought to have been attached to this margin, were forced back into the ventricle, and afforded free passage to the blood backwards. These broken portions were ragged, very frangible, and elongated with soft red vegetations; one portion contained an osseous concretion. The root of the broken valve was also partially forced back into the ventricle, the two inner coats of its sinus having given way, so that an aneurismal pouch would soon have been formed there. The other valve remained attached; its arterial surface was smooth, and its sinus somewhat dilated; but it was much thickened, and covered with soft reddish vegetations on its ventricular surface, and similar depositions extended also over the membranous portion of the ventri-

cular surface of the mitral valve, which was otherwise healthy, and appeared to close well its orifice. In the ascending aorta (which was not dilated) there was some of the common deposition (miscalled atheromatous); there was some also at the arch, with two small osseous spiculæ, which formed slight projections from the surface. At the mouth of the innominata were two roundish laminæ of bone. Around these osseous depositions, as well as around the vegetations of the valves, there was a margin of redness in parts which could not be washed off. Pulmonary valves healthy; so were the tricuspid, but thin. Auricles and right ventricle natural. About eight ounces of bloody serum in the right pleura. The lungs were engorged, and in the right was an apoplectic spot. A white hardish degeneration, as large as a French bean, in one kidney.

In the three preceding cases, the diagnosis was pretty exactly verified by *post-mortem* inspection. The sonorous passage of the blood through the altered aortic orifice and tube constituted the murmur accompanying the systole, audible at the top of the sternum and in the carotids, as well as in the region of the heart. The second murmur, more or less whiffing, heard *instead of* the *flap* constituting the natural second sound, indicated a reflux of blood through the insufficient aortic valves into the left ventricle at its diastole. These cases, therefore, confirm the rules which I have given for the detection of diseased aortic valves, in my work on the Diseases of the Chest *. The only exception to this is in the complete absence of the natural second sound, although the pulmonary valves were healthy. I explain this circumstance by the constantly distended state in which the right cavities of the heart must have been kept in consequence of the insufficient propelling power of the left apparatus: this distention prevented the valves from flapping back with sufficient force to be audible. It is, therefore, useful to bear in mind that, although my experiments prove that the second sound is generally produced in both sets of semilunar valves, regurgitant disease in only one set, the aortic, may in some cases entirely supersede this sound. I have, however, also met with cases in which, besides the to-and-fro sawing indicating disease of the aortic valves, the natural double sound, with the second flap, was still audible to the right of the sternum, being produced in the right ventricle and pulmonary valves.

Elizabeth Holland, æt. 26, Dec. 24, a patient of Dr. Macleod, had rheumatism two years ago, since which has had dyspnœa and palpitation on exertion. Has become much worse for the last 11 days, with oppressed breathing, and pain below the left mamma; countenance pale and anxious; orthopnœa; pulse quick, and rather sharp, but regular. Both sounds of the heart heard well, with a sharp impulse below the left mamma. A rough bellows murmur with the first sound, heard best at top, and to left of sternum; heard also in carotids and subclavians, and in the left inframammary region, where it loses its roughness; it is less audible to the right of the sternum, where the double natural sound is very distinct.

Diagnosis given, Dec. 24.—*Disease of the aortic valves.* The general signs made Dr. Macleod and myself conclude that there was recent pericarditis. The distinctness of the sounds and impulse discountenanced the supposition that there was liquid in the pericardium, and the absence of the leather-creak, or sound of friction, rendered it probable that the inflammation had attacked the pericardium already adherent to the heart.

Sectio, Dec. 26, 12 hours after death.—Pericardium closely adherent, and could with difficulty be torn from the heart; the united membranes thickened, and very red; no recent lymph; heart about the usual size; aortic valves quite thin at the free margins, and perforated with several smooth oval holes, four in one valve, and two in each of the others. That with four was moreover very thin and elongated, the corpus arantii, with its thickening or hem, being 1-4th of an inch within its free margin, which was extremely thin. The smaller portion of the mitral valve was little more than a fringe: the other portion somewhat thickened, with cordæ tendineæ inserted on the whole of its ventricular surface, but it seemed sufficient to close the orifice, which was small. There were three small perforations also in the pulmonary valves.

* Page 197, *op. cit.*

No doubt the perforated state of the aortic valves acted as so many loops, catching the current from the heart, which was unduly excited by the pericardial inflammation, and thus caused the rough bellows sound.

These holes must have admitted of regurgitation from the artery at the ventricular systole, but in jets too small to produce sound. I suspect that such perforations of the arterial valves are not very uncommon, for during the last twelve months I have met with them in six cases, in all of which they would have been overlooked without more care than that usually bestowed on post-mortem examinations of the heart. In two of these cases there had been no symptoms of disease of the heart; and there was found besides no other change in the organ, except the remarkable shortness of the smaller portion of the mitral valve, which I shall have to notice further on. In another case there was also universal adhesion of the pericardium, and in two others, thickening of the mitral valve. These perforations are generally accompanied by a remarkably thin, atrophied state of the valves, and they are always roundish or oval, with perfectly smooth and somewhat thickened margins. I am inclined to think that they are allied in character to the shortened conditions of the mitral and tricuspid valves, to be noticed hereafter.

Diseases of the Mitral Valve.

A. C., æt. 22, nursemaid. Has had short breath, palpitation, dry cough, and pain in chest and head for the last three months; before which she lived in the country, and was quite well. Pulse now regular, but rather quick; a bellows murmur with first sound, heard best below left mamma; not heard in the arteries; catamenia regular, but scanty. Was somewhat relieved by acting on the secretions, regulating the diet, and enjoining tranquillity. The bellows murmur with first sound still continued: it was slightly audible in the back. Dimensions of the heart apparently not increased.

About two months afterwards she was attacked with bilious fever; pulse from 120 to 160, sometimes irregular; occasionally violent palpitation, with still the same bellows murmur; at times delirious; skin very hot; tongue brown, and furred. She was treated with calomel every three hours; leeches and ice to the head; leeches to epigastrium; saline draughts, &c.

The fever went on for a fortnight, when it gradually abated, tongue having become clean, skin cool, pulse more moderate, and dilirium gone; but there was still occasional palpitation, with a feeling of sinking; and one night she was attacked with sudden cough and dyspnœa, and died in a few minutes.

Sectio.—Lungs universally crepitant and healthy, but bronchi filled with frothy mucus; pericardium very thin and transparent; heart rather small, but walls and cavities well proportioned. It showed no other disease than a remarkable thinness and shortness of the membranous portions of the mitral valve, so that they could not have closed the orifice. Cordæ tendineæ fewer than usual, but their muscular pillars were very large. Intestines remarkably thin and pale; slight redness of mucous membrane of duodenum; the rest healthy.

I have reason to believe that shortening or wasting of the membranous portions of the mitral and tricuspid valves is by no means uncommon; and I have often found slighter degrees of it in cases where the heart had manifested no morbid symptoms during life. The smaller or posterior segment of the mitral valve is often little more than a fringe to which the cordæ tendineæ are attached; and unless there be at the same time a shortening or rigidity of these cords, or an enlargement of the auriculo ventricular orifice, it does not appear that much regurgitation ensues from this condition of the valve*. It is where the anterior or larger portion is shortened, stiffened, corrugated, or otherwise rendered unfit, or especially where, the tendinous cords being thickened and shortened, the valve cannot fall back close to its fellow, here regurgitation will accompany the systole of the ventricle, and if this be with sufficient force, it will be marked by the characteristic deep bellows murmur, heard below the left mamma, and in the other parts of the region of the heart, and not heard in the great arteries.

Charles Foster, æt. 25, gardener, a

* The best mode of opening the heart to display the ventricles and their valves, is to cut from the apex nearly to the base at right angles with the plane of the septum. If this be done accurately, the incision will expose the valves *in situ*, without severing the tendinous cords.

patient of Dr. Seymour, has been subject to rheumatism for four years. At his first attack he had palpitation, and was cupped on the chest. Has felt his breath short for only six weeks past, during which time his legs have become œdematous, and face puffy.

Pulse now regular and pretty strong. A bellows murmur, with the first sound, heard best an inch below left mamma; not heard at top of the sternum, nor in the carotids. Impulse diffused, and rather abrupt; region of heart more dull than usual on percussion.

Diagnosis, written about the end of October.—*Regurgitant disease of the mitral: dilatation with hypertrophy.*

Nov. 24th.—Had somewhat improved, but is again worse, and is now expectorating red viscid sputa (peripneumonic). Died on the 26th.

Sectio, Nov. 27th.—Heart much dilated, with some hypertrophy; no adhesion of pericardium; cordæ tendineæ of mitral valve thickened and shortened; the orifice dilated, so that it could not be closed by any position of the valve. Left auricle much dilated; a small vegetation on one of the aortic valves; the rest natural. The right lung inflamed in the first and second stages.

William Cooper, æt. 42, footman, a patient of Dr. Seymour, has had rheumatism occasionally from boyhood; and at the age of 20 suffered greatly from it. For the last two years his breath has been very short, with palpitation on exertion. These symptoms have much increased in the last five weeks, and he often wakes suddenly with a feeling of oppression and palpitation.

Dec. 15th.—Pulse very weak, irregular, and intermitting; pulsations of the heart from 140 to 160,—every fifth beat delayed, and many not propagated to the wrist; impulse felt most in epigastrium; diffused and indistinct on left side. Sounds weak; the first short, distinct, and somewhat rough, as if accompanied by a brief filing; the second weak, not audible in all pulsations.

Diagnosis in this case doubtful, on account of the quickness and weakness of the heart's actions, which were scarcely enough to cause a characteristic murmur; but all the signs rendered it probable that both sets of valves of the left side of the heart were much diseased; and this was the opinion given.

Symptoms increased more or less from day to day. Was once temporarily relieved by bleeding.

Dec. 26th.—Coughed up some half-coagulated blood, after suffering great oppression. Was bled again. Died the same night.

Sectio, Dec. 27th.—Body not emaciated. Subcutaneous veins marked all over the body by the brownish-red lines of sanguineous imbibition. About half a pint of serum in the right pleura, with some shreds of lymph. Many old adhesions on the left side. Lungs much congested, especially the right, in portions of which were some dark patches of pulmonary apoplexy. Pericardium throughout closely adherent to the heart, except a space of the size of a crown-piece on the posterior surface. Adhering membranes moderately thickened. Lining membranes of heart, valves, and arteries, deeply tinged with the claret red of imbibition. Aortic orifice contracted, admitting only a little finger; one of its valves rigid with bony deposit. They were all adherent to each other at the commencement of their margins, which at one conjunction formed separate fræna, between which and the membranous portions of the valves a probe could be passed. These valves would have admitted of some regurgitation. The left auriculo-ventricular orifice would only admit a finger. In its ring, and extending into the chief segment of the mitral valve, was a large irregular osseous deposit, which was broken on opening the orifice: the broken part was half an inch thick, and porous or cancellated, and the deposition of osseous matter an inch and a half in extent. The loose portion of the valve was thickened, but still flexible. Its tendinous cords thickened and corrugated, and their muscular pillars much enlarged. Tricuspid valve slightly thickened. Pulmonary valves healthy, except a *little oval perforation* near the attachment of the free margin of two of them. Left auricle much dilated, and lining membrane thickened, but the other cavities and walls were in ordinary proportions. Slight fibro-cartilaginous deposits at the arch of the aorta, but not causing roughness of surface.

In this case there was extensive disease, both obstractive and regurgitant, of the left valves of the heart, without the production of distinctive sounds;

and it illustrates well the position which I have elsewhere advanced, that in the production of valvular murmurs, as of sound in general, there must be motion of sufficient velocity, as well as a resistance or modifying influence opposed to it. The blood was not propelled through the contracted orifices with a force which could throw their resisting portions into vibrations sonorous enough to give the characteristic murmurs.

I have several more cases of this negative character, which it would be useless to relate.

OBSERVATIONS

ON

URINARY DEPOSITS.

By R. H. Brett, Esq. M.R.C.S.

[Continued from p. 851.]

Accidental Deposits from the Ingesta.

Having thus noticed, in a very imperfect manner, I fear, those deposits the constituents of which may be found in healthy urine, although not always in the same state of combination as we find them in that fluid, and having endeavoured to point out as completely as I was able the readiest modes of distinguishing them from each other, both as regards their physical character as well as their chemical habitudes, I now pass on to the consideration of the second class of deposits, viz. those which result from certain ingesta being taken up by the absorbents, and, after forming certain combinations, being transferred into the urinary canals. It is a well-known fact that many substances that have been received into the digestive apparatus, are removed again from the body with the urinary secretion, without undergoing any very marked change. A great many medicinal agents escape from the system by this outlet. The *oil of turpentine* and certain *balsams* communicate a well-known agreeable odour to the urine of individuals using these substances medicinally.

It has been affirmed by Cantu that *mercury* has been detected in the urine of persons using that substance in the form of ointment; he states that the urine in those cases yielded a deposit which, when dried and mixed with charcoal, gave by sublimation globules of metallic mercury. It has been satisfactorily proved by experiment that *nitre*, *prussiate of potass*, and *iodine* in the form of an hydriodate, have found their way into the urine of persons taking them in medicinal doses. The curious phenomenon of a blue, or bluish-green, deposit has been observed in the urine, and this substance has proved to be prussian blue. In all these cases, however, I believe the parties had been previously taking large doses of ferruginous preparations.

Garnier and Delens have several times observed urine of a blue colour; and this tint has appeared to depend upon a peculiar substance suspended through the fluid, which was not, however, prussian blue. It was but slightly soluble in water; neither alkalies nor acids altered its colour, excepting the nitric acid, which destroyed it. Braconnot has observed that this substance is entirely devoid of taste and smell, is of a deeper blue colour than prussian blue, and exists in particles of extreme fineness. When heated it yielded carbonate of ammonia and an empyreumatic oil, and was soluble to a slight extent both in water and alcohol at the boiling temperature. When the blue substance was treated with boiling alcohol, it imparted a green colour to that menstruum, which let fall upon cooling a powder of a deep blue colour, and almost crystalline structure. This blue substance taken up by alcohol was found to be soluble in acids, even the vegetable ones, such as the oxalic and gallic; its colour, however, becoming changed by such treatment from blue to red. The red acid solutions of this substance became again blue when the acids were saturated by an alkali. The blue substance being precipitated, the urine from which this blue deposit had taken place deposited, upon the application of heat, a fresh quantity of colouring matter, but of so deep a colour as to appear almost black, but which in other respects exhibited the same properties as the preceding. Braconnot seems to regard these peculiar deposits in the light of salifiable bases.

A black colouring matter has also been observed in the urine. Dr. Marcet

has noticed the urine of a child which contained neither urea or uric acid, and the colour of which was sometimes as black as ink. When treated with an acid it did not at first undergo any apparent change, but after some time it began to deposit a number of black flocculi, and became clearer. This black precipitate was insoluble in water and alcohol; but the sulphuric and nitric acids dissolved it, acquiring a black tinge. Water poured into these acid solutions precipitated it afresh, without its having suffered any material change. It was dissolved also by the caustic and carbonated alkalies, being thrown down from such solutions by the acids. With the metallic oxides it formed insoluble compounds, of a brown colour. Dr. Prout regards it as a weak acid, to which he has given the name *Melanic* acid. This organic substance, however, bears no inconsiderable resemblance to a black matter obtained from the urine by the following process:—Evaporate urine to a syrupy mass; add sulphuric or muriatic acid to it. After some time wash with pure water, by which means most of the adhering salts are removed. The brownish black mass remaining is to be treated with alcohol. Part only is dissolved, a part remaining in the form of a black powder, having many properties in common with that substance first noticed by Marcet. According to Berzelius it contains 65 per cent. of carbon. It is not improbable that the peculiar colouring matter in the urine of the child, noticed by Dr. Marcet, resulted from some morbid change principally taking place in the organic matter which gives to urine its peculiar colour in the normal state. It must be here observed, that the two deposits just spoken of, viz. the blue and the black, are not to be considered as coming under that class of urinary sediments immediately before our notice; such, I mean, as result from the transference of certain substances from the digestive apparatus to the urinary channels; they however, appeared of too much interest to be passed over in silence.

According to Woehler, who instituted a series of experiments on man, as well as on dogs, certain of the vegetable acids, when taken in frequently-repeated doses, caused an abundant deposit to take place from the urine. He swallowed tartaric acid in repeated doses, and found that his urine deposited upon cooling a white crystalline powder of tartrate of lime. The urine was also rendered unusually acid; and upon the addition of muriate of lime in solution to it, a further precipitate of a white colour took place. He found also that the citric and malic acids produced the same effects. Gallic acid also communicated to the urine the property of blackening the per-salts of iron. The experiments of Woehler would also seem to shew, that although certain of the uncombined vegetable acids can pass into the urinary secretion, either free or associated with bases, without undergoing decomposition by the vital agency of the digestive apparatus, still that when the neutral salts formed by the combination of vegetable acids and alkaline bases are introduced into the stomach, they undergo decomposition, and pass into the urine in the state of carbonates. But the extraordinary power of the vital principle in decomposing original compounds, and either simply re-arranging, or adding to, their elementary principles, so as to give rise to fresh combinations, is not limited to organic substances: thus sulphuret of potassium, when administered internally, has been detected in the urinary secretion in the mixed form of sulphuret of potassium and sulphate of potass.

Oxalate of Lime.

I have not spoken of the oxalic acid, or the deposit of oxalate of lime, in the urine, in connexion with that class of sediments which result from ingesta being transferred from the organs of digestion into the urinary apparatus, because, although experiment and observation alike have shewn that such a transference is as applicable to the oxalic as to any other vegetable acid, there can be no doubt that large concretions of oxalate of lime, as well as sediments of the same salt, are not unfrequently met with in cases where it has been quite impossible to refer it to the effect of certain ingesta, either containing the acid in a free state or saline form. What the peculiar condition of the system may be, or what may be the state of the urinary apparatus to give rise to this peculiar secretion, we are altogether in ignorance of. The experiments of Woehler go to prove that oxalic acid taken into the stomach

passes into the urine combined with lime.

M. Magendie relates the case of an individual who passed an orange yellow coloured concretion, consisting of the oxalate of lime. That person had been in the habit of eating large quantities of sorrel daily, for nearly the space of a year. The oxalate of lime, indeed, in the form of a urinary sediment, is an exceedingly rare occurrence. Dr. Prout had only seen it once when he published his work on Calculous Disorders, but had noticed it in some instances apparently mixed with what he calls the lithic amorphous sediments. Magendie in his work says that he has never seen it in the form of sediment. In the case related by him, and which I noticed above, the concretion was six or seven lines in length, and two lines in diameter.

I have myself detected this salt more than once in those non-crystalline deposits, consisting principally of the lithates; but even in such deposits I believe it has not unfrequently been confounded with urate or lithate of lime, which last is almost constantly found in small quantities mixed with the urates of ammonia and soda. This source of fallacy may be effectually avoided by taking care to remove previously all the matter contained in the deposit which is soluble in boiling water. If there shall then remain an insoluble substance, yielding by ignition a white ash, dissolving with effervescence in muriatic acid, and giving other evidences of the presence of carbonate of lime, there can be little doubt that oxalate of lime was mixed with the urinary deposit. If, on the other hand, all the urate of lime had not been carefully separated before ignition, the latter by its decomposition would yield carbonate of lime, which, if taken under such circumstances as evidence of the previous existence of oxalate of lime, would be evidently erroneous.

I have lately had an opportunity of examining a specimen of urine letting fall the oxalate of lime in the form of a non-crystalline sediment. The urine was given to me by Mr. Bird; and as it offers some points of considerable interest, both from the nature of the deposit as from other conditions of the urine itself, I shall briefly relate the case, and the result of experiments on the urine.

The individual, a man of about 35 years of age, has for the last twelve years been employed in a sedentary occupation; was formerly in the army, in a foreign service, when he was in the habit of eating large quantities of sour crout. Never had to his knowledge partaken of preparations of sorrel. Rather more than twelve years ago he attempted to raise, in a constrained position, 1 cwt. in each hand. In this, however, he failed, but felt something give way in his loins, followed by hæmaturia. This continued for some months, and then went off without medical aid; since which, upon any particular exertion, especially during the summer, he has had a discharge of bloody mucus in his urine, mixed with an amorphous deposit. During the winter he is generally free from these symptoms. They returned, however, in November, 1835, with increased severity, attended with extreme lumbar pain; no pain in the course of the ureters, no numbness of the thighs, or retraction of testicles. It was at this time that the urine was examined: it was of a dark reddish-brown colour, opaque from the suspension of the insoluble matter through it; its specific gravity was 1·060, exceedingly high; it exerted an acid re-action, which it did not lose for some days. After remaining twenty-four hours at rest, a deposit ensued, of a greyish-brown colour, the supernatant liquor still remaining turbid, though in a less degree than at first. When thrown upon a filter, a perfectly transparent fluid, of a rich claret colour, came through; the residue on the filter was washed with distilled water, until the wash fluid was no longer coloured; the insoluble residue on the filter was found of a greyish-brown colour, evidently tinged with the colouring matter of blood, which could not be entirely separated by washing. When a portion of it was treated with a little diluted nitric acid, and heat applied, a solution was obtained without effervescence, yielding, by evaporation, a saline pellicle, of a pale yellow colour, but giving not even the slightest trace of the presence of lithic acid or a lithate: when a portion was incinerated, it blackened, and rapidly passed into a bulky white ash (the rapidity with which the carbonaceous matter is dissipated, both in calculi as well as in urinary deposits consisting of oxalate of

lime, is highly characteristic); this white ash was abundantly soluble in muriatic acid, with evolution of carbonic acid gas. The acid solution was boiled, and treated with an excess of caustic ammonia; no precipitate ensued, pointing out the absence of the phosphates. The further addition of oxalate of ammonia caused a copious precipitate to take place. The claret-coloured fluid which had been separated from the deposit by filtration was then boiled; it became opaque from albuminous impregnation, and a number of light flesh-coloured flocculi came down; these, when collected on a filter, possessed a brick-red colour, were very difficult of incineration, and yielded a fixed residue, of a pale-brown colour, consisting mainly of oxide of iron. The urine, when filtered after ebullition, differed in no respect from the healthy secretion in colour.

Deposit of Red Particles of the Blood.

It is a well-known fact, that in various diseases connected with a deranged condition of the kidney, the red part of the blood, mingled with a smaller proportion of serum, finds its way into the urinary bladder, where it becomes mixed with the natural urinary secretion, giving very different appearances to the latter fluid under different circumstances of admixture, and according to the length of time it has been mixed with it. Thus it sometimes happens that in consequence of the small quantity of the colouring matter of the blood present, it shall remain for a considerable time either in a state of solution or mere suspension in the urine, without, however, interfering with its transparency, at the same time communicating to it a greenish tinge, so precisely like that produced by the colouring matter of bile, that it would be impossible to distinguish the one from the other by mere inspection; at other times, the quantity of red colouring matter being more abundant after the urine has stood some time, a copious precipitate, of a bright vermillion colour and granular appearance, possessing also considerable specific gravity, manifests itself: the brightness of the colour is sometimes exceedingly striking, quite equal to that of arterial blood, and doubtless resulting from the action of the salts of the urine upon the colouring matter of the blood; for it has been clearly shewn by Dr. Stevens, that certain saline substances have the power of converting the black colour of venous into the bright red of arterial blood. When, however, the deposit is examined some considerable time after the urine has been voided, it will frequently be found to have assumed a dingy brown colour; and it is in these cases that the supernatant fluid usually possesses a greenish colour, from small portions of red colouring matter (the latter having undergone some change) being still suspended through it. It sometimes happens that urine containing blood assumes quite a gelatinous appearance after it has been voided, moulding itself to the form of the containing vessel, looking not unlike thin calves'-foot jelly, tinged with red colouring matter, the latter occupying generally the lower stratum of the mass. When urine of this description is briskly agitated, the gelatinous appearance is entirely destroyed, and when thrown upon a filter, it passes through without much difficulty, leaving behind a small quantity of mucus, tinged with red particles. In some cases urine of this description contains mere traces of urea and uric acid, but in all cases is highly albuminous; it is occasionally, though not necessarily, alkaline in its reaction. This state of things was well marked in the case of a girl, a patient in Guy's Hospital, who died of fungoid disease of the kidney.

How to distinguish this Deposit.

Having thus noticed the more striking physical characters of that form of deposit which consists of the red particles of the blood, it is necessary that those chemical means should be pointed out which are alone capable of distinguishing it from all other urinary sediments, more especially since certain colouring matters, even of a vegetable nature, are capable of being transferred into the urinary secretion, and communicating to it and its ordinary deposits a colour in many respects little differing from that possessed by the colouring principle of the blood; for example, hematine, the colouring matter of the hæmotoxylon campechianum—olizarine, that of the rubia tinctorum—and the colouring matter of rhubarb—are all capable of passing undecomposed into

the urine; the two first also have a strong affinity for calcareous salts, especially phosphate of lime; they are also capable of tinging the alkaline and earthy lithates: therefore, without a due knowledge of this fact, and of the proper means of determining the difference, such tinged urine and sediments might be put down to the effect of the colouring matter of the blood.

In the first place, then, it may be stated as an invariable rule, that urine containing red particles of blood, either in the form of a deposit, or merely suspended through it, becomes affected by the application of heat, either manifesting a turbidity, or undergoing coagulation,—for red particles are never passed without an accompanying portion of serum, by which the urine acquires an albuminous impregnation; besides which the red particles of the blood, when freed as much as possible from serum, are themselves coagulated by heat, in consequence, I believe, of the constant association of the red colouring matter with fibrine or albumen, so modified as to be capable of undergoing solution in water; and it is not improbable that it is only when the red parts of blood are combined with a certain quantity of fibrinous matter or modified albumen, that they are capable of undergoing solution in water,—for where red particles form a sediment in urine, they are found insoluble in water, although they are still combined with a certain quantity of fibrinous or modified albuminous matter. If this deposit be collected on a filter, washed and dried, it will pass into a dingy red or brown colour, although when suspended through, or deposited in urine, it possesses a bright red hue. If it be ignited, it inflames, evolving a strong odour of burnt horn, and ultimately leaves an inconsiderable pale red ferruginous ash. This circumstance alone would serve to distinguish it from any other deposit which might assume a similar colour. When thrown down from the urine, from any of the urinary salts which may become precipitated, tinged with vegetable colouring matter, such as hematine or olizarine, it will yield a white ash by ignition, or leave no fixed residue at all. Again, the vegetable colouring matter will be changed from red to blue, or bluish violet, by the action of an alkali, and the acids will rather heighten the red tint.

[To be continued.]

CASE IN WHICH DEATH ENSUED

IN CONSEQUENCE OF

NITRIC ACID POURED INTO THE EAR;

ALSO A CASE IN WHICH THE EPIGLOTTIS WAS DIVIDED, IN AN ATTEMPT AT SUICIDE.

By J. Morrisson, M.D., M.R.C.S.,

And Surgeon to the Newry Dispensary and Fever Hospital.

Catherine O'Neill, aged 40, of a naturally good constitution, but latterly addicted to occasional drunkenness, had a quantity of nitric acid poured into her right ear, on the 6th of June, 1833, while in a state of intoxication.

I saw her about a week afterwards, when she stated that she and her husband were in the frequent habit of quarrelling; that she was roused from her sleep on the day above mentioned, by a severe burning pain in the right ear, and that this continued, though in a much milder degree, for two or three days, but had then altogether subsided; also, that she has since been very weak, unable to stand without assistance, and confined to bed; but has had no thirst, pain of head, nor heat of skin. The daughter of this woman stated, that her father on coming in, and finding her mother in bed drunk, went out, and returned in a few minutes, when he poured a great part of the contents of a phial which he had in his pocket into her mother's ear, and that the sides of her face and neck were immediately changed to a yellow colour, which could not be washed off*. She also states, that in six days afterwards a thick, stringy, membranous slough, was detached from the auditory foramen; and that this was followed, on the subsequent day, by a very copious hæmorrhage, probably to the amount of twenty ounces; and that on the day after this, her mother totally lost the use of her right arm, and was in such a state of debility, as to make the family expect her immediate dissolution, and cause her father, expecting that he would be committed as his wife's murderer, to perpetrate the act on himself which will be afterwards mentioned.

* It was discovered that O'Neill, the husband, procured nitric acid when he went out at the time specified.

There were now, the eighth day after the receipt of the injury, several ulcerated spots over the surface of the ear, particularly in the concha; and the lobe seemed to have altogether lost its vitality. Part of the face and neck was also in a state of ulceration; there was a trifling ichorous discharge from the meatus externus, and the sense of hearing was abolished. There was no headache, nor was there any apparent febrile symptoms. Pulse 88, small, weak, and intermitting; the skin's temperature below the natural standard; there was no stupor, stertor, nor vertigo; the debility seemed alone to demand attention.

Notwithstanding the plugging of the ear, and the application of astringent lotions, together with the internal administration of tonics, animal broths, &c., the hæmorrhage returned to some extent, almost daily, for about a month, when it ceased, and the general debility during that time increased. A fortnight after the beginning of the illness, the right side of the body, the use of which, from the onset, seemed gradually declining, was so deprived of its usual power, and its different parts so affected with frequent tremulous motions, which even in bed were quite apparent, as to make it evident that one half of the body was labouring under paralysis agitans. This latter affection continued about five weeks, when a considerable amendment took place, both as respects it and the general state of the system. The muscles of the right side were now more under the dominion of the will, and the tremulous motions had very nearly subsided. The woman at this time determined on seeing her husband, who was in hospital here, and by the support of two persons she walked through several streets to him; but on her return home she felt greatly exhausted, sunk into a state of general prostration, and so continued till her demise, which took place about six weeks afterwards. The side which had been subjected to the paralytic affection, with the exception of the arm, which remained totally deprived of its use, was for several weeks before her death quite free from the tremulous motions, and as capable as the other of the voluntary. Articulation remained distinct, and the mental faculties were unimpaired; the powers of the system at large, more than of individual parts, seeming to have undergone such thorough decay. There was, however, some cough, with muco-purulent expectoration, and night perspirations.

Dr. Mollan and I examined the body after death. There was much emaciation; the lower part of the right ear was away; a cicatrix occupied the remaining portion, and the meatus externus was much wider than natural. The dura mater presented no unusual appearance except in one spot, about the diameter of a sixpence, opposite the foramen auditorium internum, which seemed of a somewhat darker colour than it should be, but it was not thickened nor adherent on either side. There was not effusion of serum, lymph, nor pus, but a clot of blood about the size of a pea was found lying exactly at the entrance of the meatus internus. No morbid appearance could be discovered in any part of the brain, save some semblance of ramollissement of that portion of it which corresponded to the petrous portion of the right temporal bone; and even this was very conjectural. The right petrous bone itself, however, was completely carious, and the seventh nerve of this side, when compared with that of the other, *seemed* to have undergone a degree of wasting. These were the only appearances worthy of remark which the examination of the head had elicited. The lungs presented nothing peculiar.

This case seems to me interesting in several points of view, but particularly the following:—First, in the very novel and singular mode which was resolved on to effect death. Second, in perfect paralysis of the arm, and in paralysis agitans of the side, occurring conjointly and at the same side with, and following such an injury as is mentioned; and these coming on after copious evacuations of blood, and subsiding (the paralysis agitans was totally done away with very soon after the hæmorrhage from the ear ceased) as the blood subsided*. And third, in such extensive caries of the petrous part of the temporal bone, without the sensation of pain, or of almost any symptom either before or after death, to denote inflammatory

* Would the detriment which the portio mollis of the seventh must have received from the action of the nitric acid, and the communication of the affection to the calamus scriptorius, or corpus restiforme, to both of which we know this nerve is connected, throw any light on the pathology of paralysis agitans?

action either of it, the brain, or its membranes.

Case II.—On the morning of the 14th of June, 1833, John O'Neill, a barber, aged 44, husband of the woman whose case formed the preceding subject. On the eighth day after having poured the nitric acid into his wife's ear, supposing she was dying, he made a dreadful gash with a razor into his own throat, between the os hyoides and thyroid cartilage*.

I saw him very shortly afterwards, and had him conveyed to the hospital. The wound extended very deep, and so far across the neck, as to lay bare both carotid arteries, though neither was injured. Speech was altogether suspended when the wound was open, but returned, though nearly inaudible, when closed; and air passed through the wound freely. The surface of his body was cold, his face pallid, pulse scarcely perceptible, and his shirt, waistcoat, &c. saturated with blood. The hæmorrhage had then ceased. He was greatly tormented by cough. The wound was sponged and cleansed, when these vessels commenced to bleed anew. These were secured, the edges of the wound approximated by means of four sutures and adhesive straps, and the patient placed in bed with his head well raised by pillows. When visited in the evening, he was labouring under a constant suffocative cough; his deglutition was most difficult, and part of his drink came through the wound. The tube of a stomach pump was attempted to be passed into the œsophagus in the morning, so as to give him his drink, &c. by this means, but without avail, as the most violent cough and sense of suffocation were induced whenever the extremity of the instrument came near the seat of the wound. An attempt was now made to pass an elastic tube, but with a similar result; so that any drink which he got down, and which I believe was very little, was taken in the ordinary way. On the next morning, the cough and difficulty in deglutition were fully as urgent as on the day preceding; and as these were most harassing, and the drink continuing to escape through the wound, I resolved on opening this anew, fearing I had included a portion of the mucous lining of the trachea in the ligatures, as I could in no other way account for such a continued cough. On removing the dressings and sutures, I found the mucous membrane had been perfectly free from both, and could not discover any thing either by touch or vision which would be likely to excite such seeming irritation. I again put in three sutures, kept the wound closed by raising the head so as that the chin would rest on the upper part of the chest, retained it in this position by bandages, and dressed it, with the view of diminishing the seeming irritability of the part, with small bits of lint which had been made a little moist with tincture of opium. In the evening the harassing cough still continued, but he then could swallow some liquid put into his mouth imbibed by a bit of sponge; but from the receipt of the injury to this time, he said he was quite certain no fluid had entered the œsophagus. The next day, both the cough and difficulty in deglutition had in some measure abated. The opium dressings were persevered in. From this period all the uneasy symptoms gradually subsided, the wound commenced to granulate, the cough became much less urgent, and deglutition was performed with tolerable ease. He was discharged from hospital about four weeks afterwards, the wound being then healed, and his health nearly restored; some cough, hoarseness, and a low croaking voice, merely remaining as untoward symptoms. About three months afterwards, this man was admitted into hospital, on account of an inflammatory affection of the bladder; this was accompanied by a low typhoid fever, and quickly succeeded by death.

On inspecting the appearance of the parts connected with the former injury of the throat, it was found at the *post mortem* examination, that the epiglottis was deficient in almost the entire of its left lateral half; that its edge at this side was thick and marked by an indentation of about a line in depth, and that the arytenoid cartilage of the same side was considerably enlarged, and as as it were forced over toward its fellow, as if to supply the place where the epiglottis was wanting.

This case which was seen by several of the medical gentlemen of this town, also appears to me interesting, not only in giving us the symptoms of an injury

* We give this paragraph as we find it in our Dublin cotemporary.—Ed. Gaz.

of the epiglottis, but in shewing that disease may act to a great extent, even on this important organ, without such serious consequences ensuing, as might, at first sight, be naturally imagined. From the constant cough, sense of suffocation, inability to swallow, and almost total loss of voice, which immediately supervened the wound of the throat, I think it may clearly be inferred that the epiglottis had been injured, and as its left lateral half was found wanting at the *post-mortem* examination, I presume it may also be inferred, that the injury consisted of an incision extending from the left side of its base, in a somewhat longitudinal direction, probably not so far as to cause amputation in the first instance, but so far as to incapacitate that side from maintaining its vitality, and of course causing it soon afterwards to slough off. Wounds of the epiglottis are very rare, and it is difficult to conceive how the present one was inflicted, unless we suppose the most determined efforts at self-destruction.*

SUGAR OBTAINED FROM THE BLOOD OF A PATIENT, IN DIABETES.

To the Editor of the Medical Gazette.

Sir,

Having lately succeeded in obtaining sugar from the blood of a diabetic patient, and finding that the possibility of doing so has been denied by Wollaston and Marcet, I have been induced to make the fact public through the medium of your journal.

The eminent chemists above-mentioned failed in detecting the ferrocyanate of potash in blood, when they knew it to be actually present, which should have made them more cautious in denying the presence of sugar. Besides, the experiments of Wollaston on serum, with and without an admixture of sugar, were not sufficiently decisive to be considered an *experimentum crucis*.

The subject of the following experiments is at St. George's Hospital, under the care of Dr. Wilson, at whose request I examined the blood and urine. The case is well marked, the urine having amounted to twenty-eight pints in the twenty-four hours.

Four ounces of urine, when slowly evaporated to dryness, and purified by alcohol, produced two drachms of whitish sugar, in the form of granular masses, into which it broke spontaneously.

As twenty-four pints of urine were passed on this day (October 26th), the whole quantity of sugar formed in the system would amount to a pound and a half in the twenty-four hours.

Some weeks after, when the daily quantity of urine had decreased to about sixteen pints, eight ounces of blood were drawn towards evening. It yielded about five ounces of opaque serum, of a milky appearance, which was treated in the following manner:—

On gently heating it, a quantity of albumen was deposited; the liquid assumed a dark colour, and on being poured off and evaporated to dryness, left an abundant semitransparent residue.

On digesting this mass in hot alcohol, a solution was obtained, of a very pale straw colour, which, by spontaneous evaporation, produced crystals of carbonate of soda and chloride of sodium.

A thick syrup was left, which, being carefully drained off upon a watch-glass, and evaporated to dryness, became a light brown transparent mass, which, being somewhat charred, smelt strongly of burnt sugar.

Its taste, though not purely saccharine, was nevertheless decidedly sweet, and could be exactly imitated by a mixture of salt and sugar. In its appearance, deliquescence, and general properties, it perfectly resembled a former specimen prepared from the urine, which was also charred.

The syrup produced by its deliquescence, on being mixed with sulphuric acid, was instantly blackened, and deposited small carbonaceous flakes.

The whole quantity obtained from 8 oz. of blood, was about 4 grains; it would therefore require 7½ pounds of blood to furnish a drachm. This being the case, I think the error of Wollaston and Marcet lay in their attempting, not to *obtain*, but to *detect* the sugar; a substance whose only characteristic property (its sweetness) is completely obscured by foreign admixture.

To conclude: the hypothesis of Rollo, that diabetes originates in the stomach,

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* Dublin Medical Journal; March 1836.

is greatly confirmed by the circumstance that sugar exists in the blood; as the want of proof for this fact has been generally considered its weak point.

I am, sir,
Your obedient servant,
CHARLES MAITLAND.

St. George's Hospital,
Feb. 26, 1836.

DEATH BY THE GUILLOTINE.

To the Editor of the Medical Gazette.

SIR,

THE arguments of MM. Sue, de Fontenelle, and others, which you gave in your last number, satisfy me that death by the guillotine is not quite so instantaneous as many have supposed. But I cannot agree with M. Julia de Fontenelle that pain is for some time felt *both* in the head and in the body. I can no more believe the latter, than that when a man's leg is amputated, the man feels pain, and so does the leg. The fact seems to be, that the individual (so far as the head, *minus* the trunk, can constitute individuality) suffers pain—until hæmorrhage from the cerebral vessels takes place—which is not immediately, but, according to Mojon and others, after the lapse of seven, eight, or ten minutes.

I have been reminded, by your notice of the subject (which was induced, I presume, by the recent executions in Paris), of a case which I read in an American medical publication, the Boston Journal, about a year and a half ago. It was communicated by Dr. Spencer, of Ticonderoga; the following is an abstract which I took at the time:

"E. D., aged 50, a man of hale constitution and robust, in making an effort to scale a board fence, was suddenly precipitated backwards to the ground; striking first upon the superior and anterior portion of the head, which luxated the dentata anteriorly on the third cervical vertebra. He was at length discovered, and taken in (as the patient said) after he had lain nearly an hour, in a condition perfectly bereft of voluntary motion; but being present I did not even suspect that the power of sensation was also gone, until the patient (whose speech remained almost, or quite perfect, and who was uncommonly loquacious at that time) said, did he not know to the contrary, *he should think he had no body*. His flesh was then punctured, and sometimes deeply—even from the feet to the neck; but the patient gave no evidence of feeling, and when interrogated, answered that he felt nothing; 'and,' added he, 'I never was more perfectly free from pain in my life,' but he remarked that he could not live, and accordingly sent for his family, twelve miles distant, and arranged all his various concerns in a perfectly sane manner.

"The head was thrown back in such a position as to forbid his seeing his body. The pulse was much more sluggish than natural. Respiration and speech but slightly affected, but were gradually failing; but he could articulate distinctly until within a few minutes before his death. *All the senses of the head remained quite perfect to the last.* He died forty-eight hours after the fall.

"Repeated attempts were made to reduce the dislocation, but the transverse processes had become so interlocked that every effort proved abortive. There was, undoubtedly, in this case, a perfect compression of the spinal marrow, which prevented the *egress* of nervous influence from the brain, while the pneumo gastric nerve remained unembarrassed."

This case, I think, proves conclusively that no agony, no suffering, no sensation whatever, can exist in the *body* after its connexion with the brain is dissolved. With the *head*, however, it is quite another thing. The brain and cerebral nerves are evidently possessed of sensibility, and can execute their functions as long as the encephalic circulation is maintained.

Your obedient servant,
PHILANCHUS ANGLICUS.

Feb. 29, 1836.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

Zeitschrift für die gesammte Medicin, mit besonderer Rücksicht auf Hospitalpraxis und ausländische Literatur. Herausgegeben von DIEFFENBACH, FRICKE, und OPPENHEIM. Band I, Heft 1, und Heft 2. Hamburg; 1836.

OUR readers may be glad to know what this new Hamburgh medical journal

contains. The parts for January and February are before us, and give a fair idea of what the work is likely to be. Though, as its title states, its object is medicine generally, yet it has a special view to hospital practice, and the novelties in foreign medical literature. It professes to take for its model the English and French journals.

In their preface the editors point out the superiority of the clinical information procured in large hospitals, over that obtained within the limited range of private practice: they say they have already secured several contributors belonging to hospital establishments; and they invite others to come forward speedily with their interesting cases.

They dwell with evident satisfaction on the happy locality of Hamburg, as peculiarly suited for giving the earliest information respecting the literary productions of France, England, Holland, Denmark, Russia, and even America; and accordingly, confident in their resources, instead of coming out once in two months, like the Magazine of Foreign Literature. which preceded them, they have resolved to appear monthly.

The departments into which the journal is divided, are—1. Original papers; 2. Abstracts, or analyses of new medical works, chiefly foreign, and of a practical kind; 3. Bibliography, or an account of such new works as do not well admit of analysis; 4. Articles from foreign journals, condensed; 5. Original notices of new facts in surgery, midwifery, materia medica, &c.; 6. Miscellaneous department for personal news, correspondence, the principal professional occurrences of the day, &c.; and 7. The well-arranged index of literature which is to accompany the volume for the year.

The opening paper is by one of the editors, Herr von Fricke, of the Hamburgh General Hospital; he gives an elaborate *Report* of the surgical practice in that institution during the first six months of last year. This is followed by another paper, by the same author, *On the treatment of Orchitis by Compression.* To both these articles we shall probably take another opportunity of reverting.

In the analytical department we find a very full account (yet unfinished) of *Bouillaud on Diseases of the Heart:* critical abstracts of *French, Perier, and Coudougnés, on the nature of Cholera; Caffe on Poisoning with Tincture of Colchicum;* an ample sketch of the late discussion in the French Academy of Medicine *on Lithotrity* and *Lithotomy;* and a notice of a work by M. Franc, of Lyons, *On suprapubic Extraction of Stone from the Bladder.*

Then come the condensed abstracts from foreign journals, arranged under the several heads—Medicine, Surgery, Pharmacy, and Statistics; after this, there are short critiques on nine or ten new books, all foreign; and, finally, the miscellanies give us the *news*—among which we observe the announcement of Dr. *Parish's* (Paris's) appointment to the chair of Materia Medica in King's College. By the way, this is the only misspelt name we have noticed in our contemporary: we hope he will avoid the mangling and mutilation of proper names so shamefully practised by the French.

The February number opens with a paper, by Herr von Oppenheim, *On Amputation at the Hip-joint.* Drs. Stintzing and Trier, the physician and surgeon of the Hospital at Altona, are the next contributors: they supply *a Report* of their establishment for the year 1834. To these original papers we shall also recur.

The analyses in the second number are—1. of *Brayer's Nine Years at Constantinople*; and, 2. of *Arntzenius' treatise on Suicide*, published at Utrecht.

Among the papers extracted from foreign periodicals, we are glad to see *Mr. Daniel Noble's Observations on the Nerves of Taste, as distinguished from common Sensation;* and *Dr. Sloan's Case of Twenty-three Days' Starvation in a Coal-mine:* both from this journal. There are also extracts from the MEDICAL GAZETTE in the department of pharmaceutics—*Mr. Pereira on the Comparative Efficacy of various Diuretics*, and *Dr. Greenhow on the use of Sea-water.*

Notices of ten recent publications follow; and the miscellanies (among which, however, we do not observe any thing that would be particularly new to our readers) close the rear.

This general description of our new Hamburgh contemporary may serve for the present: we shall, as we have said, on subsequent occasions pay him our more particular respects.

A Manual of Medical Jurisprudence and State Medicine, compiled from the latest legal and medical works of Beck, Paris, Christison, Foderé, Orfila, &c. By MICHAEL RYAN, M.D. &c. &c. Second Edition.

WE did not notice this book when it was first offered to the public, because we thought it would die the death it deserved, or make a second appearance, after some time, in a much altered shape. It has now come forth in what is called a *second edition*, with all its old imperfections *multiplied* on its head. A more trumpery and worthless "Manual" has not issued from the press for many a day. *Trumpery* we entitle it, because the author has begged, borrowed, and—"compiled," the materials of which it is made up from all quarters; it is indeed "a thing of shreds and patches"—patches, too, which are occasionally of no small size—and almost in every instance selected without a particle of taste or judgment. *Worthless* we call it, because there is not half a page of what might seem to be the author's own that can be relied on: his references are few and carefully vague, and the hand of the plagiary can everywhere be detected.

The book, we observe, is extensively advertised as "Ryan's and Beck's Medical Jurisprudence." Now this we take to be a little "too bad" and too barefaced; especially when the only relation between the parties, whose names are so unceremoniously packed together, is that of pillager and pillagee. Beck is certainly plundered unmercifully; but he must not complain, for he has many brethren in the same predicament — Paris and Fonblanque, Christison, Orfila, &c.—all laid under levy of black mail, with the gentle denomination of being *compiled*. Dr. Ryan's motto surely ought to be what Voltaire applied to a kindred soul—

> "Il compilait, compilait, compilait !"

for he is a compiler *par excellence*.

But we hold that there is one quality in a good compiler, of which Dr. Ryan has no notion;—we mean that of *honestly* appropriating to each party what belongs to each. *Suum cuique* is not a part of his system. The author of the "Manual" so mixes up his own muddy cement with the harder materials employed in his crazy superstructure, that ordinary observers cannot say whose is this, or whose is that, part of the performance. There may be cunning in this—but there is no wisdom. Were the authors who have been plundered only to reclaim their property, our *compiler* would soon resemble the plucked daw in the fable. As an instance at once of the unfairness, and the slap-dash mode of compiling adopted by Dr. Ryan, we may mention, that in the body of his book we find two entire pages copied *litteratim* from a recent number of this journal, being an abstract of an article from the *Annales d'Hygiène*, in our exact phrases and words, yet without the slightest acknowledgment. The same trick was played, a short time since, in publishing the same extract in "my" journal,—but we said nothing, as few could have seen it there. We merely ask, is this fair, "honest Michael?"

It may be thought that we are somewhat severe in this notice, especially as we have not pointed out the particular overt acts and delinquencies which we lay to the author's charge. To do this, however, would occupy more space than we can at present afford. But only let the author, or his publisher, *reclaim* against our judgment, and we promise to bring forward such a body of evidence in support of our opinions, as will at once set the matter at rest. If we cannot serve up a rich banquet of blunders,—half a dozen at least on an average for every page we open,—and show on the part of the *compiler* a sample of literary unfairness (to use the mildest term) seldom heretofore exhibited, we are content to forego all character as honest and trustworthy critics.

A word or two more with Dr. Ryan ere we part—and that on the subject of fresh *compilations*.

In our last number but one, we published a lecture on White Swellings, by M. Lisfranc; last week the same paper appeared in the Medical and Surgical Journal, without any acknowledgment. Now that it was filched from us, is proved by this—that the lecture was not delivered as we gave it, but appeared in disjointed parts, some of which we took and omitted others, but so arranged as to make a complete paper. Well, where we began, our contemporary begins—where we ended, he ends—what

we omitted, he omits—what we inserted, he inserts. But let it not be supposed that the theft was made openly, or the reference to our journal omitted accidentally: oh, no! the beginnings of the paragraphs and sentences are carefully altered throughout the first page—as far, in fact, as he thought there was any risk of their being compared—after which the copy is exact, word for word, and line for line. Poor drudge!—he ought either to have left the wretched cheat unattempted, or to have given it a better disguise.

MEDICAL GAZETTE.

Saturday, March 5, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

PRINCIPLES *versus* PRACTICE.

QUACKERY ADVOCATED IN PARLIAMENT.

We last week alluded to the circumstance of both the medical and clerical professions having their vile quack nuisances to contend with: and we had almost ventured to congratulate the members of the other learned faculty on being free from all such pestilent annoyance. The profession of the bar has, indeed, ever been famous for taking good care of itself—not only in securing the dignities and emoluments of office, but in banishing from all possible competition, those unqualified intruders, whose impudence, urged by penury and avarice, would prompt them to try their chance as Counsel. Persons of profligate character, and notorious for their misdeeds, having often exhibited before courts of justice in the quality of accused, and having escaped the vengeance of the law through their peculiar adroitness and effrontery combined,—such persons, we say, flushed with their good luck, have often beeen known to advocate the propriety of throwing the bar open, and to declaim against the injustice of *monopoly* in that learned quarter. We give both the bar and bench the highest credit for hitherto resisting so successfully the inroads of those self-interested pretenders.

These remarks have been suggested to us by some late antics of the representative of Finsbury. We allude to his manœuvres, the other night, to introduce the *open* system into the *legal* profession. He who affects to contemplate quackery in *medicine* with such hatred and horror, it appears is most anxious to encourage every charlatan who wishes to figure in the courts of law in the character of an advocate. What this inconsistency of principle betokens, or what prospect the *honourable* member may at present have in view, we venture not to say; but the facts are as we state them.

On Wednesday night last, when Mr. Ewart's Prisoners' Counsel Bill was in committee, Mr. Wakley endeavoured to introduce, as as amendment, a clause that prisoners should not be limited for their defence to Counsel learned in the law. "Why," he asked, "should there be any such limitation? Why might not the prisoner be aided by *whomsoever he thought proper*, whether a member of the legal profession or not? He possibly might receive for answer, that Counsel were best qualified; but to that he should rejoin, *let the public judge for themselves*."

If this be not a direct plea for quackery generally, we are satisfied to forfeit our right henceforth to draw the simplest inferences. But we can see more in it than a mere oversight committed by the *honourable* member. It will be recollected that he has had no small experience of the practice of the bar, having been so often engaged in his own defence. However, let us hear what the Attorney-General said in reply.

The Attorney-General said that in

every country at all approaching to civilization, the practice of the law was under some sort of discipline and control. Whether it should continue to be so in this country, or being so, whether that control should reside in the societies which at present exercised the privilege, or be transferred to other authorities, were questions apart from the measure under discussion. If the proposition of the *honourable* member for Finsbury were adopted, *any convict three weeks returned from Botany Bay might put on a wig and gown, and take his place at the bar*. * * What the *honourable* member for Finsbury proposed amounted to permitting *any* person to defend a prisoner, and thus *an accomplished London thief might be appointed counsel, and be brought down to take his station beside the gentlemen of the bar!*

It is scarcely necessary to add, that Wakley's proposition was scouted.

But while we can admire the warmth with which the learned Attorney-General spurned the idea of having himself and professional brethren degraded by the association of quack counsel, we cannot help wondering that he did not put it to the Finsburian representative whether he thought it proper to countenance quack practitioners in *medicine*?—for the principle is exactly the same. If pettifogging pretenders to the character of counsel are to be admitted to plead at the bar, because *prisoners* make choice of them, and "because the public have a right to judge for themselves,"—*à fortiori*, the Eadys, the Morisons, and the whole host of mountebanks and charlatans, must be accounted fit and proper practitioners of medicine; because *patients* choose to have them, and the public *have a right to judge for themselves!* The learned Attorney-General probably did not think it worth while to bandy arguments *ad hominem* with the Finsbury M.P.; but *we* conceive it our duty to warn the profession what sort of advocate they must expect to find in Mr. Wakley to support measures for the suppression of quackery. He has even given notice, it seems, of an intention to bring in a Bill with that especial object. What its fate may be in such hands it is difficult to hazard a conjecture; but we would recommend the *honourable* member to be prepared for one ground of opposition which the measure may possibly meet with—namely, that it comes before the House introduced by a party who has so openly advocated the rights of charlatans.

REMUNERATION OF MEDICAL WITNESSES.

The member for Finsbury has obtained leave, we find, to bring in a Bill for the remuneration of professional witnesses attending coroners' inquests. We did not foresee any difficulty in carrying the point thus far, for the principle was generally admitted last session in Mr. Cripps' Bill, and the amount of the fee for *post-mortem* examinations actually fixed. Nor, indeed, do we despair of the measure being *ultimately* carried,—although, excellent as its object is, its chance of success is rendered not a little precarious by the unlucky hands into which it has fallen.

ANOTHER TRIUMPH OF PHRENOLOGY.

FIESCHI'S HEAD.

It was our lot, a few weeks ago, to record a remarkable example of the infallibility of phrenology, as displayed by Dr. Elliotson's examination of one Rhugoburg Sing, "in every respect a highly respectable character,"—who was hanged in India "for killing one man and wounding two others."

The examination of the head of Fieschi, which has recently taken place in Paris, is no less felicitous. Dr. Lelut, a gentleman of acknowledged experience and skill in such investigations, has found the organs of *vanity*, *firmness*, *courage*, and *destructiveness*, "extremely small, or altogether wanting!" The case is perfect, and would be weakened by comment.

BOMBASTES FURIOSO.

"SINCE our last, *three* out of *five* London Medical Journals have opened their fire upon *us and our institutions*, we conclude in anticipation of a critical session of parliament. The coincidence is not accidental; we are fully aware of the designs of our enemies, and prepared to meet them; we are *girded for the fight*, and that fight shall not be with shadows, but *hilt to hilt, in the face of our fellow-citizens!*"—DUBLIN JOURNAL.

Bless the silly, pugnacious, little man, what is he raving about? What *five* journals are those, three of which have opened a fire upon him? We did not think there were so many in London. But if he dreams that *this* journal is one of the three, we beg him to awake, and know that here has been no fire opened upon him and his institutions. *We* never thought "us" worth powder. Hoity-toity, what a doughty champion we have to deal with!—"girded for the fight"—"hilt to hilt"—and doing battle "in the face of our fellow-citizens!" What a formidable opponent we have got! we fancy we see a new Bombastes throwing down the gauntlet to us, or rather hanging up his boots, strutting about in his slippers, (ought he not to have a strait waistcoat?), and bidding us defiance—

> "Whoever dares these boots displace,
> Must meet *Bombastes* face to face!"

SCHOOL OF PHYSIC IN IRELAND.

LETTER FROM DR. LENDRICK.

To the Editor of the Medical Gazette.

SIR,

I BEG leave to state the facts connected with the recent proceedings in the School of Physic in Ireland, to which you allude in your Gazette of Saturday last (20th Feb.)

I was elected Professor of the Practice of Medicine during the Christmas recess in 1832, and found that my predecessor (a *locum tenens*) had been engaged in a dispute with the Professor of Anatomy and Surgery, the latter having at the beginning of that session commenced to lecture on surgery at 3 o'clock, an hour that had appertained to the Professor of the Practice of Medicine during the eighteen preceding years.

From the advanced period of the session nothing could be done on my part; however, on the remonstrance of several students as to the inconvenience sustained by them from the delivery of two medical lectures at the same hour, I applied to the Provost and Senior Fellows before the ensuing winter. I suppose it is unnecessary for me to specify in full the inconvenience to which I allude, as the letter which bears Dr. Macartney's name (Gazette, p. 820,) states the objection of "interfering with tutors' lectures and those on divinity." The interference of two *medical* lectures with each other, would of course be still more objectionable, and such a practice is, in this city at least, unprecedented in any school.

On the 9th of November, 1833, I received a letter from the Registrar of T. C. D.*, informing me that the Board would not require as a qualification for a degree, attendance on any medical lectures delivered at 3 o'clock, except those on the practice of medicine. The Provost also mentioned, that *all* the facts of the case had not been stated, nor the consequences of the interference of hours, when the Board had permitted the surgical lectures at 3 o'clock the preceding session.

The letter of the Registrar was immediately forwarded by me to the Professor of Anatomy and Surgery.

A letter was sent from the Board to Dr. Barker (Registrar to the Professors) on the 30th of October, 1834, informing him that they would not recognise any medical lectures at 3 o'clock except those on the practice of medicine, and which he communicated to the parties concerned: I believe they also stated that whatever lectures were delivered by one Professor, they would consider but as one course. Just before the Christmas ensuing, the Registrar of T. C. D. wrote to the Professor of Anatomy and Surgery, prohibiting the delivery of his lectures at 3 o'clock, but on his remonstrance as to the advanced period of the session, and the inconvenience attendant on an alteration, the Board assented to his delivering the remainder of the course, with an intimation however, that they would not permit in future the delivery of a part of his statutable course at 3 o'clock.

Here then are *four* distinct resolutions of the Provost and Senior fellows between the date of the Provost's letter that you quote, Sept. 29, 1832, and your next quotation, Oct. 13, 1835. Why have you (who appear to have had access to the correspondence†) not only omitted to make the

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* Trinity College, Dublin.

† Dr. Lendrick is here in error: we have seen no letter from the Provost or Board dated in the intervening period.—ED. GAZ.

slightest allusion to them, but even called on your readers to "mark the change that took place on the opening of a fourth session?"

Since the Provost and Senior Fellows have by law the power to make "rules and orders" to regulate the conduct of the University medical professors, who receive a large emolument (I believe several hundred pounds annually, each) from the *University funds*, independently of the pupils' fees, and since the practice to which the Board objected, continued, notwithstanding the order made by them *before* the commencement of the present session (see Gazette, p. 820,)—what alternative was left but to close their Anatomical and Surgical theatre at 3 o'clock? They must else either have permitted two medical lectures to be delivered at the same hour, or required the Professor of the Practice of Medicine to surrender the one hour possessed by himself and predecessors (now) for twenty-one years, in order that another Professor might select a second, to which he had only laid claim three years since, and against which claim five distinct resolutions, progressively more peremptory, had been enacted and promulgated in vain.

You assume that the Board have prevented the Professor from lecturing *twice* in the day; this is not the case; it is in the Professor's power to select for *additional* lectures on anatomy or surgery either the hour from 9 till 10, or, that from 12 till 1. Besides, he can lecture at any hour before 9 or after 5 P.M.; and I suppose it is unnecessary to add, that the medical lectures at Edinburgh commence at 8 in the morning, or that Sir Astley Cooper delivered for many years the best *surgical* lectures probably in Europe, at 7 in the evening.

Should the Professor decline to deliver surgical lectures at any other hour than 3 o'clock, it is not for me to calculate the consequences to our School of Physic; I can only say that no possible modification of the regulations would enable the student to *complete*, as you suggest, his education for the Edinburgh degree here, or indeed any where else, except at Edinburgh itself; inasmuch as the regulations of that University require that, at least, *two courses* should be attended at Edinburgh.

If, notwithstanding the well-meant "*warning*" of your correspondent, the foreign students should resort to our School of Physic as heretofore, with the object of graduating in Edinburgh, it will be in their power to make surgery one of the two courses they are *indispensably* required to attend there; and they will therefore suffer no inconvenience from the want of it as a separate course in our school, should even the deficiency continue. However, separately or not, the Professor *must** teach surgery to the students, and, I conceive, that the completeness of a school depends on what is taught, and not on its nominal division into departments under the same Professor.—Excuse this hasty letter.

Your obedient servant,
CHARLES LENDRICK.

Dublin, Tuesday,
Feb. 23, 1836.

PLAN FOR SECURING ADEQUATE

MEDICAL ATTENDANCE ON THE COUNTRY POOR.

PAROCHIAL DISPENSARY SYSTEM.

To the Editor of the Medical Gazette.

SIR,

I HAVE read with much satisfaction the several articles in your Gazette on the subject of medical attendance on paupers under the New Poor-law system, the evils of which are ably exposed both in your editorial remarks and by your correspondents, and with singular perspicuity and force by your correspondent "Ruricola." Nevertheless, I must confess I was disappointed by his last communication in your number for January 16, in which he explains his plan for amending the system; and notwithstanding the unqualified approbation you express of his scheme, I am induced to send for insertion, in your next number, one which I think still better adapted for all the purposes so ably advocated by you and him, as well as your other correspondents.

My plan is neither more nor less than the establishment of a Parochial Dispensary in every district, to which one, two, or three surgeons, might be appointed, according to circumstances.

The first object that should be kept in view by all parties, is to furnish, in the easiest and readiest way possible, proper medical and surgical assistance to such of the poor in every district, as cannot afford to purchase it for themselves.

The second object ought to be, so to arrange and distribute the medical charge of the poor amongst the practitioners of each district, as to do away with undue monopoly, and the undertaking on the part of any single individual, duties (and duties of such importance, too) which it is mo-

* I mention this, as you say "that surgery is no longer deemed requisite to be taught in the University of Dublin."

rally, not to say physically, impossible he can ever perform; and also to set aside that disreputable and degrading competition, by which one practitioner is induced to underbid another.

These two objects are not only compatible with each other, but they so far coincide, that the means which best promotes the one, will be found most effectually to secure the other; and the attainment of both will be attended with advantages to the public at large far more important than could arise from any paltry pecuniary saving.

To illustrate my plan, let us take a market town which is the centre of a union of parishes, the whole including a population of say from 10,000 to 20,000 inhabitants: let us suppose from two to six or eight medical practitioners reside in the town. A small house, containing at least three rooms, should be taken for a Dispensary, or two or three contiguous apartments in the Workhouse may be appropriated to that purpose. One of these rooms must be furnished with drugs, and the necesary means of compounding them, the fittings-up of which may cost ten or fifteen pounds. The necessary drugs, &c. will probably cost from fifteen to thirty pounds per annum. An apothecary or dispenser will be required; and either a junior practitioner who had not fully established himself, and whose time is consequently not entirely filled up, or the son or assistant of one of the medical men, or even a dispensing chemist, might in every such town be found to undertake the necessary duties, and probably for a salary of from fifteen to thirty pounds per annum.

In such a district as we are speaking of, two or three surgeons would be required, who should be chosen from the surgeons of the town, if possible by the rate-payers generally; but whether by them or the Guardians, some such rules as the following ought to be attended to in the selection:—

1stly. No surgeon should be appointed who had not resided at least two years in the district.

2dly. The preference should always be given to men of some experience, but who are not so old as to be infirm, and thus likely to make the office a sinecure.

3dly. The preference should likewise be given to members of the College of Surgeons; such a preference is due to them on all occasions from the public, as they have taken the only means in their power to prove to the world that they have been regularly educated, and are qualified to fulfil the duties of their station: others may be qualified, but the presumption is not in their favour, and it is, to say the least of it, highly discreditable to any man in the present day, to have entered upon the practice of his profession without a surgeon's diploma.

4thly. The surgeons should be chosen annually, and if, where there are several eligible candidates, a rotation could be established, it would be desirable.

5thly. The surgeons for the time being should be *ex officio* members of the Board of Guardians; they would be very useful members on many occasions, and always ready to be appealed to in cases of alleged sickness or infirmity, but of course would be excluded from the right of voting on all questions relating to themselves.

6thly. The salaries of the surgeons should vary according to circumstances. Let us suppose, in the present case, that the district or union includes a circuit of about five miles from the central town, and that it is in a medium state as to pauperism. I suppose a salary of from 50*l.* to 100*l.* per annum (take the medium, 75*l.*) to each of the two or three surgeons, would afford a very moderate remuneration for their services. It would, however, be much more respectable and consonant to the feelings, than any compensation calculated at so many shillings per head, as proposed by Ruricola and some others.

Thus we have seen that the whole expense of providing attendance and medicines for such a district, consisting, perhaps, of ten or fifteen parishes, would not exceed between 200*l.* and 300*l.* per annum,—a smaller sum, probably, than had been paid under the old system for far less efficient services. Of course, the principle being understood, it would be easy to vary the plan according to the peculiar circumstances of different districts. Let us glance at one or two such cases.

It may happen that a union may consist of two or three towns, each having resident surgeons, with or without a number of villages. In these cases a Dispensary, with one or more surgeons attached to it, should be established in each of the towns, and the country places united to those towns to which they are most contiguous.

Again, it may happen that villages included in a union may be so distant from the town or towns of such union, as to render applications for medical aid to the Dispensaries established there very inconvenient to the poor, and perhaps they may have been heretofore in the habit of employing medical men in other towns or villages nearer to them, but not now included in their union. In all such cases the districts in question should continue to be attended by the same practitioners, and from the same stations as before; for

it would not only be positive injustice to the established practitioners to force new men into their districts under the pretence of serving the poor, but most inexcusable cruelty to the poor themselves, to oblige them to seek for medical assistance at a distance of seven or eight miles, perhaps, from their homes, when they might obtain it, and had been accustomed to obtain it, within two or three. And it may be observed here, that whatever advantages in other respects may be expected from more extended unions, the dispensary districts ought never, if possible, to extend beyond a circuit of five, or at most six miles, from the central town or station; and in the larger unions these stations should be multiplied according to their extent.

Then as to the rules for regulating the Dispensaries, they would be very simple, and such as would naturally occur to all medical men; in short, they would be pretty much the same as those which regulate charitable Dispensaries in cities: each surgeon would have his days, and would be bound to attend all applicants on those days; he would also have his hours of attendance at the Dispensary, where all patients who were able would be expected to appear. Having once taken charge of a patient, he should attend him throughout his illness, as often as would be necessary. Then rules might be laid down for consultations, operations, &c. according to the convenience of the surgeons.

Extra fees should be allowed for midwifery, and perhaps vaccination. The surgeons should keep brief journals of the cases, and enter their prescriptions in the same book. Minor regulations would readily be suggested as they were called for. One word as to the admission of patients into the Dispensary:—In all cases, the certificate of a guardian, or parish officer, stating that the applicant belonged to the district and was receiving parish relief, or was an inmate of the Workhouse, would be a sufficient title to relief; in other cases, the certificate (of which there should be printed forms) of any guardian, or respectable rate-payer, stating that the applicant resided in the district, and had no visible means of paying for medical advice, should be admitted.

It will be observed, that one of the principal and most essential features of this plan is the establishment of a common Dispensary for the distribution of medicines; and as this is a great innovation upon the former practice, it will probably be the first to be objected to, as entailing an additional and unnecessary expense. There are several weighty reasons why surgeons who attend the poor should not be called on to supply them with the necessary medicines, but they are not all such as could be fully appreciated by the public. One, however, I will venture to state, because it is of itself quite sufficient to justify the innovation, and will be readily understood by every one. It is this:—The greatest advantage that the public (and the poor especially) would derive from the plan now suggested, would arise from the active competition amongst the surgeons of the Dispensary: he who attended most diligently, and proved himself most skilful in the treatment of his patients, would be most sought after by the poor; but mark the unfairness that would arise if his diligence and extra exertions entailed upon him likewise an additional expense for medicines. Add to this, that the Dispensary system will bring the medical treatment of the poor more completely under the eye of the public, and I think its advantages will be sufficiently apparent.

I fear I have trespassed too much on your space and on the patience of your readers; nevertheless, the subject is one of primary importance at the present moment, and if you think the plan I have propounded applicable to the exigencies of the occasion, you will join with me in requesting that other journals and respectable newspapers would take the trouble to reprint it, that it may be circulated widely amongst the public.—I remain, sir,

Your constant reader, and

A Country Practitioner.

March 2, 1836.

ROYAL INSTITUTION.

Friday, Feb. 19, 1836.

Professor Faraday on Magnetism as a common character of all the Metals.

The lecture of this evening was highly interesting, as it contained the first announcement of a *new fact*, or what is likely to be, ere long, generally admitted as such, though at present it rests wholly on analogical evidence. In proceeding to give his proofs that magnetism belongs to every known metal, the accomplished lecturer called attention to the other general properties of metals—their gravitation, their fusibility, combustibility, &c. — all of which are possessed by metallic substances in extremely different degrees. That magnetic power should belong only to two metals, iron and nickel, Dr. Faraday argued, was contrary to the usual disposition of nature, especially as it is under-

stood that either iron or nickel, raised to a certain temperature, becomes as destitute of magnetic virtue as any of the other metals. It was the observation of this last circumstance which seems to have led the Professor to infer, that most probably, if not quite certainly, if we could lower the temperature of the other metals—copper, tin, lead, &c.—to a sufficient degree, we should find them endowed with magnetism. All this was beautifully illustrated by experiment. But as we happen to have before us a paper just published by Dr. Faraday himself on this subject, in the *Philosophical Magazine* (for March), we shall probably make our readers best acquainted with his views by quoting his own words:—

"General views have long since led me to an opinion, which is probably also entertained by others, though I do not remember to have met with it, that *all* the metals are magnetic in the same manner as iron, though not at common temperatures, or under ordinary circumstances. I do not refer to a feeble magnetism, uncertain in its existence and source, but to a distinct and decided power, such as that possessed by iron and nickel; and my impression has been that there was a certain temperature for each body (well known in the case of iron), beneath which it was magnetic, but above which it lost all power; and that, further, there was some relation between this *point* of temperature, and the *intensity* of magnetic force which the body when reduced beneath it could acquire. In this view iron and nickel were not considered as exceptions from the metals generally with regard to magnetism, any more than mercury could be considered as an exception from this class of bodies as to liquefaction.

"I took occasion, during the very cold weather of December last, to make some experiments on this point. Pieces of various metals, in their pure state, were supported at the ends of fine platinum wires, and then cooled to a very low degree by the evaporation of sulphurous acid. They were then brought close to one end of one of the needles of a delicate astatic arrangement, and the magnetic state judged of by the absence or presence of attractive forces. The whole apparatus was in an atmosphere of about 25° Fahr.: the pieces of metal, when tried, were always far below the freezing-point of mercury, and as judged, generally at from 60° to 70° Fahr. below zero.

"The metals tried, were arsenic, antimony, bismuth, cadmium, cobalt, chromium, copper, gold, lead, mercury, palladium, platinum, silver, tin, zinc, and also plumbago; but in none of these cases could I obtain the least indication of magnetism.

"Cobalt and chromium are said to be both magnetic metals. I cannot find that either of them is so, in its pure state, at any temperature. When the property was present in specimens supposed to be pure, I have traced it to iron or nickel.

"The step which we can make downwards in temperature, is, however, so small, as compared to the changes we can produce in the opposite direction, that negative results of the kind here stated could scarcely be allowed to have much weight in deciding the question under examination, although, unfortunately, they cut off all but two metals from actual comparison. Still, as the only experimental course left open, I proceeded to compare, roughly, iron and nickel with respect to the points of temperature at which they ceased to be magnetic. In this respect iron is well known *. It loses all magnetic properties at an orange heat, and is then, to a magnet, just like a piece of copper, silver, or any other unmagnetical metal. It does not intercept the magnetic influence between a magnet and a piece of cold iron or a needle. If moved across magnetic curves, a magneto-electric current is produced within it exactly as in other cases. The point at which iron loses and gains its magnetic force appears to be very definite, for the power comes on suddenly and fully in small masses by a small diminution of temperature; and as suddenly disappears upon a small elevation, at that degree.

"With nickel I found, as I expected, that the point at which it lost its magnetic relations was very much lower than with iron, but equally defined and distinct. If heated and then cooled, it remained unmagnetic long after it had fallen below a heat visible in the dark: and, in fact, almond oil can bear and communicate that temperature which can render nickel indifferent to a magnet. By a few experiments with the thermometer it appeared that the demagnetizing temperature for nickel is about 630° or 640°. A slight change about this point would either give or take away the full magnetic power of the metal.

"Thus the experiments, as far as they go, justify the opinion advanced at the commencement of this paper, that all metals have similar magnetic relations, but that there is a certain temperature for each beneath which it is magnetic in the manner of iron or nickel, and above which it cannot exhibit this property. This magnetic capability, like volatility or fusibility,

* See Barlow on the Magnetic Condition of Hot Iron. Phil. Trans. 1822, p. 117, &c.

must depend upon some peculiar relation or condition of the particles of the body: and the striking difference between the necessary temperatures for iron and nickel appears to me to render it far more philosophical to allow that magnetic capability is a general property of all metals, a certain temperature being the essential condition for the development of this state, than to suppose that iron and nickel possess a physical property which is denied to all the other substances of the class."

THE LATE PROTESTS FROM THE METROPOLITAN SCHOOLS.

To the Editor of the Medical Gazette.

SIR,

I PERCEIVE with great pleasure, that the Students of the Metropolitan Schools have nobly come forward to disclaim any participation in the late disreputable transactions at the Crown and Anchor Tavern, and in just, yet temperate language, have expressed their disapprobation of the whole proceedings. Permit me, sir, as a Student of a Provincial School, to give my testimony in accordance with theirs; a testimony in which, I am sure, a majority of Provincial Students concurs, and which will tend to remove that odium which may have become attached to us, as a body, in consequence of the ill-directed efforts of some of our misguided brethren.

It would be superfluous on my part to make any observations on the proceedings of the meeting; the pages of your journal record sufficient to excite feelings of disgust in every unprejudiced mind; yet I cannot but remark, that the replies of the candidate, even as they are reported in the Lancet, if not decidedly erroneous, savour strongly of presumption. An air of self-confidence, and of contempt, is visible throughout, while that profundity of knowledge which alone could have warranted these, no where appears. If this be the case, need we be surprised that the examiner should have expressed (perhaps not in a very gentlemanly manner) his feelings at this improper bearing; and if the replies referred to above may be taken as evidences of the general professional knowledge of the candidate, it is not, I think, assuming too much to say, that his examination must have been far from creditable, and thus have afforded sufficient grounds for his rejection. In the eyes of the more industrious students, the Examiner is acquitted of injustice, and they are confirmed in their diligence and application,—new stimulus also is given to unassuming and unobtrusive merit.

I say the industrious class, not that I wish to imply that there was a want of industry on the part of the candidate, but, if I may be permitted the use of a technical expression, he was "ground"—for how long I know not. It is said that some require to be "ground" for more than two years; be that as it may, "grinding" has become a very lucrative profession, and the "grinders" a very important class of men; and if we may judge from some remarks made at the late meeting by one of the most eminent of the class, they alone are capable of determining as to the competency of a student to practise our profession.

Notwithstanding this, sir, I cannot divest myself of the opinion that this system is every way injurious to the interests of many students. Catechetical instruction cannot impart a knowledge at all comparable, either as to durability or usefulness, with that derived from close observation and study; yet there are many who rely upon it alone, and neglect every opportunity for solid and lasting improvement.

This is a matter of common observation, and its increasing prevalence has induced me to offer these remarks; and, in conclusion, I would observe, that if to a love for our profession are added industry and patient research, we may, without artificial aids, most certainly avoid

> Quæ maxima, credo
> Esse mala, exiguum censum, &c.

I am, sir,
Your obedient servant,
Σ.

Hull, March 3, 1836.

REPUBLICATION OF MR. LAWRENCE'S WORK ON MAN.

To the Editor of the Medical Gazette.

SIR,

I PERCEIVE you not only reject the letter I wrote in reply to Mr. Wickham, but you state I "ought to have more sense than to send a letter utterly unfit for your pages."

I had ever imagined that justice was even-handed, and that the editors of public journals professed its impartial distribution as the rule of their conduct. If such be the rule which you profess, I am at a loss to understand how you will reconcile the exclusion of my letter on account of its "coarse personalities," when you have admitted a no very measured attack upon my character and honesty as a publisher.

It is true Mr. Wickham did not mention

my name; but, sir, who can doubt (and I put it to your candour whether you for a moment doubted) the following sentence was directed to apply to me—" or with the *moral honesty* of the man who can *plunder* his fallen brother, and *rob* him of the only gains of their unhallowed craft."

I know nothing of Mr. Wickham any more than he does of me; but I humbly submit that as nice a sense of honour may be found in the breast of a p or bookseller as in the heart of a surgeon; and the good taste which precludes me defending myself from attack in the pages of your journal, should have saved me the necessity (by the suppression of what I conceive a very virulent, wanton, and uncalled-for, reprehension of my motives and intentions) of a reply.

However, sir, I leave the matter in your hands: your own character as a journalist is as much at stake as mine; and unless you admit my reply to Mr. W., I should hope you will be able to reconcile such a course upon much more tenable and adequate grounds than any you have yet advanced.

I call upon Mr. Wickham himself to explain why, in denouncing the principles inculcated in a certain publication, he has not thought proper to do so only after a period of three years since the last edition, and seventeen years subsequent to the first—why his zeal should have slumbered so long, only to burst forth at last in a tirade involving the unlucky publisher in his anathema.—I am, sir,

Your obedient servant,

EDWARD PORTWINE.

124, Aldersgate-Street,
Feb. 26, 1836.

[We can assure the reader that the above is a *very mild* letter compared with the former one sent us by Mr. Portwine. We ought, perhaps, to add, that the facts of the case stand thus:—Mr. Lawrence publishes a book containing certain passages, which are generally looked upon as calculated to do harm, particularly to young men: the author, either from conviction, or in deference to the opinion of others, suppresses the work at a considerable loss. Upon this another party avails himself of the literary labour of Mr. Lawrence, and issues a fresh edition. Mr. L. appeals to the Court of Chancery; and Lord Eldon rules that the work being *contra bonos mores*, is not entitled to the protection of the law. The publisher then sets at nought the author's wishes, disregards the alleged immoral tendency, and renders accessible to all a work of which he dare not have published a single copy, had not the law, in its wisdom, declared it to be too bad to be acknowledged as the property of any one.—ED. GAZ.]

LITERARY INTELLIGENCE.

Preparing for publication, The Human Brain: its Structure, Functions, and Development; illustrated by the Nervous System of the Lower Orders. By Samuel Solly, Lecturer on Anatomy and Physiology at St. Thomas's Hospital.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

March 3, 1836.

Robert Heywood M'Keand.
Peon Blundell, Spern, Berks.
Joseph Rogers, Newport, Isle of Wight.
Thomas James, Uxbridge.
William Roden, Kidderminster.
David Nicols Carr, Alnwick.
John Hartley, Marton.
William Henry Jones, Lutterworth.
Robert Long, Berkeley.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Mar.* 1, 1836.

Abscess	1	Indigestion	2
Age and Debility	43	Inflammation	19
Apoplexy	3	Bowels & Stomach	2
Asthma	23	Brain	2
Cancer	1	Lungs and Pleura	5
Childbirth	5	Insanity	5
Consumption	56	Jaundice	1
Convulsions	14	Liver, diseased	5
Croup	3	Measles	8
Dentition or Teething	4	Mortification	7
Dropsy	21	Paralysis	3
Dropsy on the Brain	9	Small-pox	8
Dropsy on the Chest	1	Sore Throat and Quinsey	2
Epilepsy	1	Stricture	1
Fever	7	Tumor	2
Fever, Scarlet	8	Unknown Causes	6
Gout	1		
Heart, diseased	3		
Hernia	1	Casualties	3
Hooping Cough	2		

Increase of Burials, as compared with the preceding week } 56

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ *N.* *Longitude* 0° 3′ 51″ W. *of Greenwich.*

Feb. 1836.	THERMOMETER.		BAROMETER.	
Thursday . 25	from 25	to 42	29·06	to 29 05
Friday . . 26	18	39	28 97	28 91
Saturday . 27	31	38	28·91	28 96
Sunday . . 28	27	39	29·1	29 33
Monday . . 29	25	39	29 36	29·48
March.				
Tuesday . . 1	34	47	29·25	28·84
Wednesday 2	35	50	29·34	29·51

Prevailing winds, N.W. & S.W.

Except the 25th, generally cloudy; with frequent showers of rain; a little snow on the 26th.

Rain fallen, ·75 of an inch.

CHARLES HENRY ADAMS.

NOTICE.—Were we to open our columns to protests against the lies and misrepresentations of the Lancet, we should never have done. Who does not know the character of that journal? Who believes a word it states? This must be our brief apology for declining several letters.

WILSON & SON, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 12, 1836.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XXIV.

THE MINERAL ACIDS.

UNDER the head of *mineral non-metallic acids*, I shall examine *sulphuric, sulphurous, nitric, hydrochloric, nitro-hydrochloric,* and *carbonic* acids.

(a) Sulphuric Acid.

History, synonymes, &c.—This acid was known to Geber, the Arabian chemist, in the seventh century. In the state in which we usually meet with it in English commerce, it is termed *protohydrate of sulphuric acid, oil of vitriol, spirit of sulphur,* &c.

Native state.—It occurs in both kingdoms of nature. It is found in the waters of many volcanic regions. Thus the *Rio Vinagre* (the Vinegar River), which descends from the volcano of Gurose, in Colombia, to Popayan, has received its name from its acid properties, which it derives from being impregnated with sulphuric and hydrochloric acid.

Rivero states that a *litre* (equal to 1 $\frac{75}{100}$ imperial pints) of this water consists of—

	Troy grains.
Sulphuric acid	16·679
Hydrochloric acid	2·811
Alumina	3 706
Lime	2·471
Iron	traces.

Issuing from the crater of Mont Ida, in Java, is a river which also contains this

FIG. 91.—*Cascade of Vinagre.*

acid. In many of the grottoes of Italy it is likewise found. Dr. Thomas Thomson, in his "Mineralogy," states, that in Persia there is an earth so strongly impregnated with this acid, that it is used by the natives as an acidulous seasoner of food. Combined with bases (more particularly lime, alumina, barytes, and strontian), sulphuric acid is frequently met with.

Preparation.—The mode of manufacturing it varies in different places:—

1. *Nordhausen method.*—At Goslar, Nordhausen, and other parts of Saxony, the following process is adopted. Crystallized protosulphate of iron is calcined in the open air, and then distilled in earthen vessels. The product in the receiver is called *fuming sulphuric acid,* while *peroxide of iron* is left in the retort. Protosulphate of iron is thus composed:—

1 atom of Sulphuric Acid	=	40
1 atom of Protoxide of Iron ..	=	36
6 atoms of Water (6 × 9)	=	54
1 atom crystallized Protosulphate		130

By calcination, the greater portion of the water of crystallization is driven off, while the *protoxide* is converted, by the oxygen of the air, into *peroxide* of iron. By the subsequent distillation two atoms of sulphuric acid, combined with one of water, pass over.

2. *English and French methods.*—Although various methods of manufacturing sulphuric acid are adopted in England and France, yet they all agree in this, that by the combustion of sulphur, sulphurous acid gas is obtained, which is conveyed into a leaden chamber, where it becomes oxidized by an acid of nitrogen (nitric, nitrous, or hyponitrous acid), and converted into sulphuric acid.

(*a*) *The most improved plan* now followed in this country is the following:—Sulphur is burned on an iron plate, in a furnace, (Fig. 95, *a*,) the admission of air being regulated by one or more apertures in the iron door of the furnace. In the act of combustion every atom of sulphur unites with two atoms of oxygen of the air, and produces one atom of sulphurous acid gas; which is conveyed by a pipe or chimney (*f*) into a leaden chamber (*b*), the bottom of which is covered, to the extent of four or five inches, with water. The size of the chamber varies considerably in different manufactories. The one from which I made the accompanying sketch was 70 feet long, 20 feet wide, and 20 feet high.

To oxidize this sulphurous acid, and convert it into sulphuric acid, nitric acid vapour is generated (by the action of sulphuric acid on nitrate of potash, contained in an iron pot or basin placed on the furnace plate), and conveyed, along with the sulphurous acid, into the chamber. To facilitate the mutual action of the gases, steam is also conveyed into the chamber. Certain chemical changes now take place, one of the results of which is sulphuric acid, which dissolves in the water.

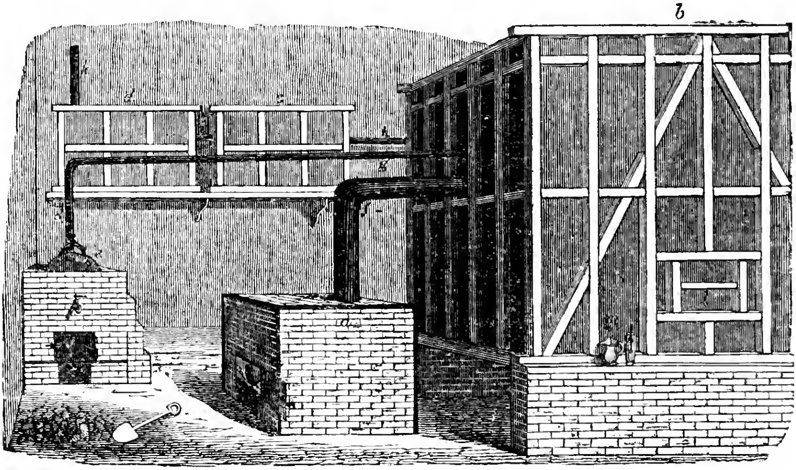


Fig. 95.

(*a*) *The furnace.*—In lieu of bars is an iron plate, on which the sulphur is burned. To heat this plate when the combustion is commenced, a few live coals are thrown on, and then a shovel-full of sulphur. The furnace-door has two apertures, one of which can be closed when the combustion is too rapid. On the plate is seen the *iron pot* containing the nitrate of potash and sulphuric acid. The sulphurous and nitric acids are conveyed by the chimney (*f*) into the *leaden chamber* (*b*). At the side of this chamber is seen the *man-hole* (*l*), by which the workmen can enter when the process is not going on. The strength of the liquid in the chamber is ascertained from time to time by a hydrometer: for this purpose a small portion of the liquid is withdrawn by the pipe (*m*). (*e*) The *boiler* supplying steam to the chamber by the pipe (*g*, *g*). (*c*) and (*d*) are *smaller leaden chambers*, through which the residual gas passes from the first, or large chamber, by the pipe (*h*) and (*i*), before it is allowed to escape into the air by the waste-pipe (*k*.)

Theory.—Let us now examine into the nature of the changes before alluded to. If dry sulphurous and nitrous acid gases be mixed, no change is observed; but if a few drops of water be admitted, a *crystalline compound* (*hydrate of hyponitro-sulphuric acid?*) is formed. When this comes in contact with a large quantity of water, effervescence takes place, binoxide of nitrogen is evolved, and sulphuric acid left

in solution. Davy thought this crystalline compound consisted of *sulphurous* and *nitrous acids*, and *water*; but Gaultier de Claubry states it is composed of

Anhydrous sulphuric acid....	68,000	or about	$4\frac{1}{2}$ atoms = 180
Hyponitrous acid	13,073		1 atom = 38
Water	18,927		6 atoms = 54
	100,000		272

Gay Lussac, on the other hand, regards it as a compound of *sulphuric* and *nitrous acids* and *water*, and his inferences are adopted by Dumas. Gaultier de Claubry's view is perhaps the most consistent, and therefore I shall adopt it, and accommodate our explanation accordingly.

Sulphurous acid abstracts oxygen from nitric acid, by which sulphuric acid is obtained, as well as some compound of oxygen and nitrogen (perhaps hyponitrous acid.)

Re-agents.	Results.
$2\ \ddot{S}$	$\dddot{N}$
$\overset{\cdot\cdot\cdot\cdot\cdot}{N}$	$2\ \ddot{S}$

It is probable, however, that the hyponitrous acid is not immediately formed, but that one atom of sulphurous acid abstracting an atom of oxygen from the nitric acid, the latter is converted into nitrous acid, which is further deoxidated by another atom of sulphurous acid; and thus is obtained the hyponitrous acid. It is not unlikely that binoxide of nitrogen may be also generated, as Dr. Thomas Thomson thinks.

By the union of hyponitrous and sulphuric acids with some water, the crystalline compound already spoken of is formed, and which, falling into the water of the chamber, is decomposed; the sulphuric acid is dissolved, and the hyponitrous acid converted into nitrous acid and binoxide of nitrogen. The latter gas escaping into the chamber, absorbs two atoms of oxygen, and is reconverted into nitrous acid, which, acting on more sulphurous acid, undergoes a similar series of changes to those already described.

On the large scale this crystalline compound is rarely formed, owing to the great excess of aqueous vapour in the chamber. It is, however, sometimes generated in the tube, or chimney, leading from the furnace to the leaden chamber.

Concentration.—When the liquid in the leaden chamber has acquired a specific gravity of 1·5, it is conveyed by leaden pipes into *rectangular leaden boilers*, where it is evaporated and concentrated; but in some manufactories this part of the process is omitted. The final concentration is effected by boiling it down in glass or *platinum retorts*; the latter, notwithstanding their great expense, are now usually adopted. The price, of course, depends on its size. Mr. Parkes had one which held 30 gallons, and cost about £360; but sometimes they are made so large that they are worth £1000. In this apparatus the acid is deprived of a part of its water and some sulphurous acid, and when it has attained a sufficient degree of concentration, it is drawn off by means of a platinum syphon into carboys.

By theory 16 parts of sulphur ought to yield 49 of protohydrate of sulphuric acid, namely—

16 of Sulphur.
24 of Oxygen.
9 of Water.
—
49 of Protohydrate of Sulphuric acid.

In practice, however, 48 parts may be regarded as the maximum; and few get so much as this.

(*b*) *Another method*, still followed in some manufactories, is the following:—Sulphur, mixed with one-eighth of its weight of nitrate of potash, (or of nitrate soda), is burned on iron or leaden plates, either placed within the chamber, or in a furnace on the outside.

Theory.—In this process one portion of the sulphur combines with the oxygen of the air, and forms *sulphurous acid gas*; while another portion decomposes the nitre, and furnishes *sulphate of potash*, and *binoxide of nitrogen*.

Re-agents.	Results.
2 Ox. (of the air.)	$\ddot{S}$
2 S	$\ddot{N}$
$\dot{K}\ \overset{\cdot\cdot\cdot\cdot\cdot}{N}$	$\dot{K}\ \dddot{S}$

The sulphate of potash is left on the plates; while the binoxide, escaping into the air, combines with two atoms of oxygen, and forms *nitrous acid*; which, reacting on some sulphurous acid, produces sulphuric and hyponitrous acids. The latter is decomposed by the water into nitrous acid and the binoxide, as mentioned in the previous process.

(*c*) Sometimes *bisulphuret of iron* is employed in lieu of sulphur to yield sulphurous acid.

(*d*) In France a mixture of treacle and nitric acid are boiled together, in order to produce the gases (binoxide of nitrogen and

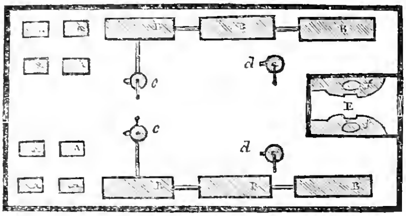


FIG. 96.—*Plan of a Sulphuric Acid Manufactory.*

A,—Rectangular leaden boilers.
B,—Leaden chambers.
E,—Retort house.

nitrous acids) necessary for the oxidation of the sulphurous acid. Oxalic acid is formed in the boiler (which is made of platinum.)

Preparation of anhydrous sulphuric acid.—Pure or anhydrous sulphuric acid may be readily obtained by distilling the *fuming* or *Nordhausen* acid in a glass retort, by a moderate heat, surrounding the receiver by ice. It condenses in a solid state in the neck of the retort, and in the receiver.

Properties.—(*a*) *Of the anhydrous or pure sulphuric acid.*—It is a crystalline solid, having very much the appearance of asbestos. It gives out dense white fumes in the air, like fluoride of boron. It melts at 64° F. and boils at from 101 to 122° F. It is composed of

1 atom Sulphur	16
3 atoms Oxygen (8×3)	24
1 atom Anhydrous Acid ..	40

(*b.*) *Properties of fuming, or Nordhausen sulphuric acid.*—This is a dark brown oily-like liquid, which gives out copious white fumes in the air. Its specific gravity is about 1.9. It is imported in stone bottles, having a stoneware screw for a stopper. Its composition is as follows:

2 Anhydrous Sulphuric Acid (40×2)	80
1 Water	9
1 Dihydrate of Sulphuric Acid	89

or,

1 Anhydrous Sulphuric Acid	40
1 Protohydrate of Sulphuric Acid..	49
	89

(*c.*) *Properties of protohydrate of sulphuric acid*, or the *strong sulphuric acid of English commerce.*—When pure, it is a colourless, transparent, inodorous liquid, of an oily consistence, having a specific gravity of 1,847 or 1,850. It boils at about 620° F. It has a most powerful affinity for water, and when mixed with this liquid evolves heat; condensation taking place. When exposed to the air, it abstracts water, and hence increases in weight.—It is composed of

1 atom Anhydrous Sulphuric Acid	40
1 atom Water	9
1 atom of the Protohydrate of Sulphuric Acid	49

Characteristic tests.—(*a*) If a solution of a barytic salt (say chloride of barium) be added to a liquid containing this acid in solution, a white precipitate of the sulphate of barytes is obtained, and which is distinguished from other insoluble barytic salts by two characters; namely, by its insolubility in strong nitric acid, or the alkalies; and secondly, by heating it with charcoal the sulphate is converted into the sulphuret of barium, which, by the action of muriatic acid, evolves sulphuretted hydrogen, easily recognised by its odour, and by its blackening paper moistened with a solution of sugar of lead. (*b*) Another test for sulphuric acid is a soluble salt of lead (as the acetate) which, occasions a heavy white precipitate of the sulphate. (*c*) Heated with organic matters, it gives out sulphurous acid gas, recognisable by its smell, and by its deoxidating properties. Thus, if we distil in a small tube retort, sulphuric acid with some gelatine, the liquid in the receiver will be found to contain sulphurous acid; and if to this a little iodic acid be added, sulphuric acid and free iodine are immediately formed: the latter may be recognised by its action on starch.

Impurities.—This acid is, I believe, never adulterated, though it is apt to contain impurities derived from its mode of manufacture. Thus it frequently holds in solution sulphate of lead, obtained from the leaden chamber. You may readily detect this by diluting the acid with water, by which the sulphate is precipitated.

Incompatibles.—It seems quite unnecessary to enumerate all the substances which are incompatible with this acid; because some, such as the alkalies, and their carbonates and many others, are known to every tyro. I would, however, particularly caution you against employing, simultaneously, sugar of lead and this acid. In more than one instance I have known a pill containing sugar of lead given in hæmoptysis, and at the same time draughts exhibited containing the infusion of roses—one of the ingredients of which, you are aware, is this acid. Now the efficacy of the lead is completely destroyed by the sulphuric acid, since an insoluble sulphate is formed.

Physiological effects.—In the *concentrated*

form, hydrous sulphuric acid is a powerfully corrosive poison. Its chemical action depends in a great measure on its affinity for water, so that when placed in contact with the animal tissues, it first destroys their vitality, and then exercises its attractive power for water—charring or carbonizing them, and giving rise to the formation of the sulphate of ammonia. Assuming the composition of the fibrous and albuminous tissues to be that which Raspail represents (though I would not have you infer I am an advocate for his notions in this respect),—the phenomena of the local action of this acid are readily comprehended:—the acid attracts the *water*, precipitates the *carbon*, and unites with the *ammonia*. On examining the stomachs of persons poisoned with this acid we sometimes find them whitened, but where the action has been more powerful they are usually blackened. Here is a wax model, executed by Mr. Miller, of No. 3, Theobald's Road, which shows admirably the destructive operation of this acid—the blackening of the part touched by the acid—the acute inflammation of the neighbouring parts. The unfortunate individual (a medical man) was assistant to my friend Mr. Saner, of Finsbury-square. In Dr. Roupell's plate the same kind of blackening is shown.

The *symptoms* of poisoning with this acid are as follows:—Alteration, or even destruction, of the soft parts about the mouth; burning pain in the throat, stomach, and bowels; frequently, alteration of the voice, from the swelling, &c. of the parts about the larynx; breath fœtid, from the decomposed tissues; constant and abundant vomiting of matters which may be bloody or otherwise, but which effervesce by falling on a marble hearth; bowels variously affected, sometimes constipated, though usually purged, the stools being bloody. The constitutional symptoms are principally those arising from a disordered action of the vascular system: thus the pulse is frequent and irregular, feeble, often imperceptible; extremities cold; great feebleness, or even fainting, with cold sweats. The same constitutional symptoms are observed when the stomach is wounded or ruptured. One remarkable characteristic is, that the mental faculties are unaffected, even up to a few minutes before death.

If the acid be *somewhat diluted* (as the dilute sulphuric acid of the Pharmacopœia) it ceases to be a corrosive, but is still a powerful irritant poison, causing acute gastro-enteritis.

When *largely diluted*—that is, in a state proper for therapeutic employment—its irritant operation is no longer perceptible, unless indeed the stomach be inflamed. In the latter case it excites pain, heat, and sometimes vomiting. If, however, this viscus be in a healthy or merely weakened condition, the acid acts as a *tonic*, that is, it increases the appetite, and assists the digestive function. In some cases of mild fever, and more frequently in hectic fever, it allays thirst, diminishes preternatural heat, checks profuse sweating, and even, I have in several instances seen, reduces the frequency of the pulse: these effects have given rise to its appellation of *refrigerant*. Sometimes it increases the secretion of urine,—at the same time rendering this fluid unusually acid.

The continued use of the acid, however, generates a slow kind of gastro-enteritis; heat and pain are experienced in the throat, stomach, and intestines; the digestive functions are disordered; gripings are frequently observed; sometimes purging, and subsequently fever. Of all the mineral acids employed, this, however, may be administered for the longest period without occasioning such symptoms. When thrown into the veins it coagulates the blood and causes death.

Modus operandi.—Administered in the very dilute state, this acid probably becomes absorbed, for it influences the quality of the urine, and sometimes communicates to the milk a griping quality. Its use is said even to be hurtful to the fœtus in utero.

Uses.—(*a*) *As a local agent* it may be employed as a caustic, an irritant, or as an astringent. As a *caustic* it has no advantage over many other agents, except that which arises from its liquid form, which, in most cases, renders it disadvantageous. For example, the difficulty of localizing it, would be an objection to its employment in the production of an issue, but would be an advantage in applying it to wounds,—such as those caused by rabid animals, or poisonous serpents, since the liquidity of the acid enables it to penetrate into all parts of the bites.

In *entropium*, or that disease in which the eyelid is inverted or turned inwards upon the eye, this acid has been applied as a caustic. In this complaint, the friction of the eyelashes on the globe is most distressing, giving rise not only to inflammation, but even ulceration of the cornea. Now when the disease is permanent, two modes of curing it have been proposed; either to remove a fold of the integument by the knife, so that, by the subsequent cicatrization, the lid may be drawn outwards—or to destroy a portion of the skin by a caustic, as sulphuric acid. The latter plan of treatment has been practised successfully by several eminent oculists, among whom I may name Mr. Guthrie and Mr. Lawrence. So also in *ectropium*, in which the lid is everted or turned outward, Mr. Guthrie has applied the concentrated acid to the

inner side of the everted lid with advantage.

Mixed with fatty matters, either lard or olive oil, sulphuric acid has been employed sometimes to occasion rubefaction—sometimes as a styptic—and lastly, as a remedy for the itch. Thus 1 part of sulphuric acid mixed with 6 or 7 parts of lard, has been employed as a rubefacient in paralysis; or applied to the skin in the second stage of inflammation of the joints, when the violence of the disease has subsided; or to wounds, to suppress hæmorrhage from numerous small vessels. Lastly, properly diluted, we employ this acid on account of its *astringent* qualities as a gargle, in ulcerations of the mouth and throat; but after using it the mouth should be carefully rinsed, to prevent the action of the acid on the teeth.

(*b.*) *Internally* we frequently employ the acid as a *tonic*, usually as an adjunct to bitter infusions. For example, in fevers, especially of the kind formerly termed putrid: when the violence of the disease has subsided, and when no marks of gastro-enteritic inflammation are present, you will occasionally find a few drops of the dilute acid may be given in the infusion of quassia, or of gentian, or even of cinchona bark, with great advantage. *To check profuse sweating* in pulmonary and other affections, whether phthisical or not, it is sometimes a valuable agent, though occasionally it fails to give the least relief. In *hæmorrhages*, as from the lungs, stomach, and uterus, it is administered as an astringent, but its efficacy is doubtful. So also in purpura hæmorrhagica it is given with the same intention, but though I have several times employed it, I have not observed any evident benefit derived therefrom. In *febrile diseases* generally it may be administered largely diluted, as a refrigerant, to diminish thirst and preternatural heat, but the vegetable acids are to be preferred. On account of its chemical properties it has been employed as an *antidote* in cases of poisoning by the salts of barytes and lead, but it is more advisable to give the sulphates, as of magnesia and of soda. In some forms of *lithiasis* it has been given with occasional advantage. But in order to place more clearly before you the cases that may perhaps be relieved by the employment of acids, I shall arrange urinary sediments in the following manner,—for I put out of the question the solid concretions or calculi, as we have at present no probable means of removing them except by a surgical operation (lithotomy or lithotrity):—

- 1. *Pulverulent* or *amorphous* sediments ....
  - (*a.*) *Lithates* (called also urates) of ammonia, soda, and lime, *tinged* by the *purpurates*, or by the colouring matter of the urine ......
    - 1. *Yellow* sediments; called by Prout, sediments of health, because they occur in the urine of healthy or slightly dyspeptic individuals, by errors of diet.
    - 2. *Red* or *lateritious* sediments, indicative of feverish, or inflammatory action.
    - 3. *Pink* sediments: these occur in dropsy, hectic fever, and visceral affections.
  - (*b.*) *Phosphate* of lime, and the triple phosphate of magnesia and ammonia; constituting sediments nearly *white*.
- 2. *Crystallized* sediments, commonly termed *gravel* ....
  - (*a.*) More or less *red*, and composed of nearly pure *lithic acid*.
  - (*b.*) *White*, composed of the *triple phosphate* of magnesia and ammonia.
  - (*c.*) *Dark blackish green*, composed of *oxalate of lime*: these are very rare.

Now you observe, from the above table, that the *white sediments* (whether amorphous or crystallized) are composed of the phosphates; while the *coloured sediments* (the dark blackish green excepted) consist of lithic acid, or the lithates. It must be admitted, however, that the colour of the sediments cannot always be relied on for distinguishing their composition, and, therefore, it is proper you should determine by chemical means to which of these two kinds of sediments the one under examination belongs. On this depends, in some measure, the plan of treatment to be adopted; for when the *white* or *phosphatic* sediments are observed, the use of the mineral acids will oftentimes be found beneficial; and the alkalies, their carbonates, and their vegetable salts, should be most carefully prohibited. On the other hand, when the deposits are *lithic acid*, or the *lithates*, the alkalies are beneficial, and the acids may be regarded as objectionable. The acids employed in the white or phosphatic deposits, are principally the sulphuric and

muriatic; the first being preferred, since it can be continued for a longer period without occasioning gastric disorder. You will, however, be much disappointed if you expect to effect a cure by a chemical mode of treatment merely, which, in fact, is to be regarded rather as a palliative than a curative means. Laxatives, alteratives, tonics, diet and regimen, &c. are among the most efficacious remedies. In *skin diseases*, especially lichen, the employment of sulphuric acid has been found beneficial.

Administration.—The *dilute sulphuric acid* of the Pharmacopœia, made of $1\frac{1}{2}$ fluid ounces of the strong acid, with $14\frac{1}{2}$ fluid ounces of water, may be given in doses of from 10 to 40 drops in any convenient vehicle. The *compound infusion of roses* is a very agreeable form of employing it. We may give of this infusion from 1 to $1\frac{1}{2}$ ounces. Another mode of exhibiting it is in the form of the *aromatic sulphuric acid*, of the Edinburgh Pharmacopœia. This compound consists of alcohol, sulphuric acid, and some aromatics (cinnamon and ginger), and is sold in the shops under the name of *elixir of vitriol*. In this preparation the alcohol is partially converted into sulphuric æther. It is given in doses of from 10 to 30 drops. In the Dublin Pharmacopœia is a formula for a *sulphuric acid ointment*, or *acid soap*. It consists of a drachm of the strong acid to an ounce of lard. The ingredients should be mixed in a mortar (of glass or earthen-ware).

Antidotes.—In the event of poisoning by the strong acid, instantly administer chalk, or magnesia, suspended in water, or whiting, or soap, or white of eggs, or gelatine, or milk. In the absence of any of the agents now mentioned, large draughts of water, or oil, or in fact any mild diluent, should be instantly administered, for a few minutes' delay may perhaps seal the fate of your unhappy patient. The case is afterwards to be treated as one of gastro-enteritis.

(b.) *Sulphurous Acid.*

History and synonymes.—Although known to the ancients, yet we are indebted to Stahl, Scheele, and Priestley, for the first accurate examination of it. It has been termed *volatile sulphurous acid*, *spirit of sulphur by the bell* (from the mode of procuring it).

Native state.—It escapes from the earth in a gaseous form, in the neighbourhood of volcanoes.

Preparation.—For chemical purposes it is prepared by mixing two parts of mercury with three parts of strong sulphuric acid, applying heat and collecting over mercury. The results are the bipersulphate of mercury and sulphurous acid.

Re-agents.	Results.
$4\,\dddot{S}$	$2\,\ddot{S}$
M	$\ddot{M}\,2\,\dddot{S}$

For pharmacological purposes, however, it will seldom be necessary to procure it in this way. By the combustion of sulphur in atmospheric air, this gas is readily obtained; and when we are about to employ it, either as a disinfectant or vapour bath, this method is always followed.

Properties.—In the gaseous state it is colourless and transparent; having a remarkable and well-known odour, incombustible, and not being a supporter of combustion. It reddens litmus and bleaches some colouring matters, especially infusion of roses. It is irrespirable, is absorbed by water, and has a specific gravity of 2·2.

Composition.—It is composed of

			Constituents.		Resulting Compound.
1 atom of sulphur	16	or by volume	oxygen = 16	& sulph. = 16	sulphurous acid =32
2 atoms of oxygen	16				
1 atom of sulphurous acid	32				

Characteristic tests.—Its odour (that of burning sulphur) is most characteristic. Peroxide of lead, with which it forms protosulphate of lead. A solution of it added to iodic acid, abstracts oxygen from the latter, and sets iodine free, which may be recognized by its producing a blue colour with starch.

Physiological effects.—It is a powerful local irritant: inspired in the pure state, it causes spasm of the glottis; but diluted with air, it enters the lungs, and occasions cough, heat, and pain. Applied to the skin, it produces heat, pain, and itching.

Uses.—It has been employed as a *disinfectant* from very remote periods, for Homer alludes to it both in the Iliad and Odyssey. In the cure of *itch*, baths of sulphurous acid gas are mentioned by Glauber, in 1659. At the Hôpital St. Louis, a very complete apparatus for the application of this remedy in diseases of the skin has been erected by D'Arcet: it is a kind of box, inclosing the whole body, with the exception of the head. The sulphur is placed on a heated plate in the lower part of the box. From ten to twenty baths, or even more, are requisite to effect a cure. Rayer, in his elaborate work on skin diseases, admits these baths are some-

times serviceable in chronic eczema, but objects to their use in scabies, on the ground of "the long continuance of the treatment necessary to relieve the disease."

As a stimulant in syncope, or asphyxia, this gas has been recommended by Nysten. It is readily applied by holding a burning sulphur match under the nose.

Antidotes. — When this gas has been respired, we may inhale the vapour of solution of ammonia. A few drops of the liquid may also be swallowed.

OBSERVATIONS ON URINARY DEPOSITS.

By R. H. Brett, Esq. M.R.C.S.

[Concluded from p. 851.]

Sediment containing Red Particles of the Blood.

I have already stated, that in some cases where the quantity of red particles is small, the urine suffers them to subside only after a considerable time, and the fluid possesses, in many cases, a greenish tinge, so strikingly analogous to that produced by biliary colouring matter, as to require means besides those afforded by mere inspection to distinguish the one from the other. This peculiar green tinge has been supposed to depend upon the ultimate blending of the peculiar red colouring matter of the blood with the yellow colour of the urine; and in certain cases, after blood has been drawn, and the usual coagulation of the red and fibrinous portions perfected, the super-natant fluid has possessed more or less of a green tinge; but I doubt whether this is to be set down solely to the admixture of red particles with the yellowish serum: for if we take dried cruor, finely powdered, and add it in variable proportions to healthy urine which is of a fine amber-yellow colour, no green tinge similar to that just alluded to can be produced. The same holds good if perfectly fresh or moist cruor be employed; if also, instead of employing urine, we make use of carefully-decanted serum of blood, or any fluid rendered yellow by colouring matter, the same state of things will be observed. I think I have observed, that when urine or serum of blood containing a small proportion of red particles suspended through or dissolved in it, has been allowed to remain for some hours, especially if the temperature be somewhat elevated, the fluid has assumed somewhat of a green tinge. This I am inclined to look upon as the result of decomposition, and probably the production of sulphuretted hydrogen, which, reacting upon the ferruginous matter contained in the blood, furnishes the green tinge in question. The following experiment in particular led me to entertain such a notion. A small quantity of colouring matter of blood was dissolved in water, sulphuretted hydrogen was then passed through the fluid; it first assumed a purplish tinge, and ultimately passed into a dingy green. Be this explanation correct or not, certain it is that mere artificial mixtures of the red particles of blood and yellow fluids are not capable of producing that peculiar green tinge which is occasionally observed in urine.

How distinguished from Bilious Urine.

Since, then, it appears that urine mixed with small quantities of red colouring matter of the blood does not unfrequently put on an appearance strikingly similar to bilious urine, it becomes a matter of first-rate importance to distinguish between them. In both cases there may be a deposit independently of the red particles of the blood on the one hand, or the biliary matter on the other, from the precipitation of some of the salts naturally existing in the urine, and which may become tinged with the colouring matter of the blood, or of the bile diffused through the secretion.

I have already observed that bloody urine always undergoes a change, more or less marked by the application of heat, in consequence of an albuminous impregnation. Now bilious urine does not undergo this change, inasmuch as there exists no albumen capable of being coagulated by heat, at least in healthy bile. It must, however, be confessed that the bile, passing through the urinary passages, might become mixed with some albuminous matter; but even in such a case, unless the latter were very considerable in quantity compared with the biliary matter, no coagulation would ensue upon the application of heat, for bile appears to possess the curious property of preventing the coagulation of small quantities of albuminous matter, even after the latter has undergone that pe-

culiar change which is produced by coagulation. This conclusion was arrived at by Powell, who instituted a series of experiments on that subject. Again, it is well known that muriatic or nitric acid, when added to bilious urine, causes, in most cases, after some time, a deposit of a green colour to take place,—a circumstance not observable in urine impregnated with red particles of blood. If the green deposit obtained by the addition of an acid to bilious urine be collected and washed, the addition of nitric acid will cause it to dissolve, with a considerable play of colours,—the green passing into various shades of blue, violet, and pink; ultimately, however, especially if the acid be in considerable quantity, or heat be applied, into yellow. This is highly characteristic of biliary colouring matter; it must, however, be remembered, that bilious urine contains that peculiar modification of colouring matter which is not precipitated of a green tint, but of a brown colour, by acids. This description of bilious urine is, however, easily distinguished from bloody urine, from the fact of its not being coagulated by heat. It sometimes happens that bilious urine yields a deposit which, when collected upon a filter, appears in the form of orange-yellow flocculi, consisting chiefly of the colouring matter of the bile. By exposure to the air, these flocculi become green; and when dissolved, which they easily are in a solution of caustic potass, give the same re-actions with nitric acid as described above. In those cases, however, where bilious urine lets fall a pretty abundant and somewhat ropy deposit, of a green or yellowish-green colour, it will, when carefully examined, be found to consist mainly of mucus, tinged with biliary colouring matter, in the same manner as mucus from the stomach is not unfrequently found of a green colour, from admixture with bile.

I have occasionally examined urine of so deep a colour as to lead to the suspicion of the presence of the colouring matter of blood or bile. In these cases the quantity voided at a time is generally small, and it might *à priori* be supposed that there was a deficiency of the watery parts of the urine, by which the fluid became concentrated; but upon examining it, comparatively with healthy urine, there has been found a positive deficiency of urea and uric acid, the only organic principle in excess appearing to be the colouring matter; there was no evidence of the presence of bile or red particles of the blood.

Creamy and Caseous Urine.

The urine has occasionally been observed of a milky colour, throwing up a creamy matter to its surface, and yielding by filtration a fibrinous residue of a reddish colour. The fluid portion of such urine is found to be coagulable by heat; at other times, however, this effect is not observed to take place. Dr. Prout has designated this *chylous* urine, and seems to consider the extraneous matter in it as imperfectly-formed blood.

Wurzer relates the case of a man who was subject to catarrhal affections, in whose urine he found a deposit having all the properties of caseous matter; there was also a deficiency of urea. In a case detailed by M. Petroz, of a woman who died in child-bed, the urine, upon examination, was found to contain caseous matter. M. Cahol examined a specimen of urine, transmitted to him by M. Alibert, physician to the Hôpital St. Louis. Having examined it by several re-agents, he found that it gave evidence of the presence of caseum. "We commenced," says he, "a series of experiments, of a comparative nature, with this substance, and the caseous matter obtained from skimmed milk. Upon subjecting each to the action of the same tests, the same results were obtained; and both yielded by distillation precisely analogous products. The urine, freed from the caseous matter, had all the chemical properties of that secretion in its ordinary condition."

Purulent Deposits.

The last urinary deposit which I shall enter upon the consideration of is the purulent. Perhaps none of the preceding are of more importance; for the existence of this substance in the urine will always lead us to suspect a very serious morbid condition of the kidney, urinary passages, or bladder, if not positive disorganization of some one or more of these structures. The difficulty of distinguishing very small quantities of pus from equally minute proportions of mucus under certain conditions, be these principles derived from whatever part of the body they may, has long been universally allowed; and although

that difficulty is far from being entirely removed, when we confine our attention to the deposit of that description, as occurring in the urine, still it is somewhat diminished; for in those cases in which we meet with it in the urine, the purulent matter is for the most part in sufficient quantity to enable us to recognize it without much difficulty, as well from its peculiar physical nature, as from the action of reagents upon it. It is nevertheless true, that we occasionally meet with organic deposits in the urine, which seem to partake of a mixed character between pus and mucus. The latter is capable of undergoing very marked changes, arising apparently in some cases from the effect of certain vital actions, in others from atmospheric exposure.

Thus inflammatory action will alter the character of a mucous secretion, rendering that opaque which, in a healthy condition, is semi-transparent; and modifying also its chemical habitudes. Mere exposure for some time to atmospheric influences will also cause a change of a sufficiently striking nature to take place. Thus mucus which was originally semi-transparent and ropy, becomes in part opaque, and appears to undergo a species of coagulation, putting on many of the characters, both physical and chemical, of coagulated albumen. A ready and unequivocal mode of distinguishing these two principles from each other has long been a desideratum among medical men; and although the remarks which are about to follow will not be found applicable to pus and mucus in whatever part of the body they may be found, still as far as the urine itself is concerned (that being the main object at present), they may, perhaps, be found adequate to the distinction.

Characters of Pus.

It will, however, be necessary, before noticing purulent matter as constituting a urinary deposit, to make some general remarks upon the nature of pus. This peculiar fluid, which is not unfrequently one of the products of inflammatory action, is somewhat viscid in character, of a pale yellow colour, with a slight tinge of green; opaque and homogeneous. It possesses no appreciable odour when cold, but when warm, and in considerable quantities, it exhales a vapour of a somewhat mawkish odour. When fresh it neither reddens litmus or turmeric paper. According to some authorities it speedily becomes acid by keeping; this, however, will depend much upon the length of time it has been kept, and the temperature to which it has been exposed. It is stated by some that this acidity is by no means permanent, soon giving place to an alkaline reaction, owing to the formation of ammonia. I have certainly found that when pus has been diluted with water, and kept uncovered in a cool place, that it has not given any signs either of acidity or alkalinity, even for days.

Pus has been considered by some as blood deprived of its red particles; and this opinion appears to me to be borne out by certain experiments, the truth of which being admitted, will form an excellent ground-work whereon to build many means of distinguishing this peculiar substance from mucus. Every one who has decolorized the cruor of blood by means of chlorine, must have been struck with the close resemblance which the colourless, or nearly colourless, fibrinous particles of the blood suspended through the water, have with those particles which are suspended through a purulent fluid diluted with water; but this resemblance is rendered still more obvious, if the fibrinous particles just alluded to be diffused through a serous fluid, such, for example, as pure serum of blood, rendered somewhat less viscid than ordinary by dilution with a little water.

Now if it can be shewn that the chemical habitudes of pus are similar to those possessed by decolorized red particles of blood suspended through a serous fluid, then I think it may be fairly conceded that the former differs only from blood in its being destitute of colouring matter, and having a less concentrated form than the latter fluid. I shall now proceed to shew that the above assumption of chemical analogy between the two fluids is not at variance with the result of direct experiment.

The two obvious ingredients of healthy blood are, its serum, or more fluid part, and its cruor, crassamentum, or more solid part. The former we know by analysis to contain a large portion of water, some albumen, and certain salts, chiefly of an alkaline nature, with a little fatty matter and extractive; these

are the most striking and important constituents of serum, and the only ones it is necessary here to enumerate. The latter consist of a substance called fibrine, colouring matter, and certain salts, chiefly of an earthy nature, with a very important metallic oxide, at least chemically considered, viz. peroxide of iron, together with some fatty matter. If pus be allowed to remain at rest for some time, the opaque particles subside, and a clear fluid remains at top. This last, when carefully removed and examined, is found to be coagulable by heat even below that of boiling water. The stronger acids also coagulate it. If the watery part be separated from the coagulated portion, it is found to yield by evaporation and subsequent incineration a saline residue, in no respect differing from that which the serum of blood yields under similar treatment. A little fatty matter is also capable of being separated from the serous part of pus. In all these respects it bears a strict analogy to the serum of blood, the only difference being that one is a much more concentrated albuminous fluid than the other. That portion which is opaque and insoluble differs in no respect, when collected and dried, from decolorized red particles, and may, therefore, be called the fibrinous portion of pus. Water possesses no solvent action over it; neither do the diluted acids. Caustic potass is capable of dissolving it with the assistance of heat; and it yields by incineration a fixed residue, containing a small portion of oxide of iron and earthy phosphatic salt, similar to that found in the fibrine of blood. We are also able to remove a little fatty matter, by boiling the insoluble particles in alcohol before incineration. It is clear, then, that not only is the serous, or fluid albuminous portion of pus, very similar in chemical constitution to that fluid when separated from the other solid parts of blood, but that the solid and insoluble particles are not different from the red particles of blood which have undergone decolorization.

It is not my intention in this place to make any reference to the numerous experiments, microscopic and chemical, that have at different times been instituted on purulent matter. Darwin, Pearson, and Everard Home, are the principal writers on this subject in this country; but their experiments, however clear and convincing they may appear at a first perusal, have not, when repeated by others, afforded similar results; besides, it is my intention to confine my remarks as much as possible to the consideration of purulent matter as found in the urine.

The following action of reagents on pus is different from that observed in the case of healthy mucus, and apply to pus and mucus generally:—

1. Strong acetic acid, when added to pus in the proportion of four parts acid to one part pus, does not appear to produce any more effect on the purulent fluid than the same bulk of mere water.

2. The addition of the same quantity of acid to healthy mucus causes it to contract in bulk, still retaining much of its transparency, and appearing like a membranous substance, or semi-gelatinous mass, diffused through a perfectly pellucid fluid. Mucus, however, which has been exposed for some time to atmospheric influences, as well as that which has been altered by inflammatory action, is not capable of undergoing this characteristic change by means of acetic acid.

3. If an equal bulk of concentrated sulphuric acid be added to pus, an imperfect solution of a deep brown colour, bordering upon purple, is produced. Upon dilution with water the colour is entirely discharged, and the fluid remains troubled from the diffusion of opaque particles through it, having an appearance precisely like pus diluted with a considerable quantity of water.

3. If mucus be treated in a similar way with concentrated sulphuric acid, the colour produced is a pale brown, very much less deep than in the instance above noticed. Dilution with water discharges the colour. The resulting fluid is not opaque, however, as is the case with pus.

4. If pus be boiled with three or four times its bulk of strong nitric acid, a yellow coloured solution is obtained: if this solution be evaporated, so as to get rid of the redundant nitric acid, a yellow viscid mass is obtained. The heat should then be continued so as partially to char the syrupy mass. Water should then be added, and a drop or two of nitric acid, so as to render the fluid somewhat acidulous. The whole should then be evaporated to a small bulk, and filtered. The fluid which comes through is still yellow, and somewhat acid. The addition of a drop or two of a solution

of ferrocyanate of potass causes the whole to become of a deep bluish-green colour, from the formation of prussian blue by the action of the salt of potass on the per-oxide of iron, removed from the pus by the agency of the nitric acid.

5. A given bulk of the yellow acid fluid, obtained as above described, was treated with two drops of ferrocyanate of potass.

6. The same bulk of pure water was rendered very acid by means of nitric acid, much more acid, indeed, than the yellow fluid just noticed, and two drops of a solution of ferrocyanate of potass added.

7. The same bulk again of pure water was rendered yellow by means of carbazotic acid (the yellow principle obtained from indigo), so as to resemble in colour the yellow fluid, 5. Two drops of ferrocyanate of potass were also added.

In Exp. 5 the fluid became instantly of a deep bluish-green colour; and after a short time yielded a precipitate of a distinct blue colour.

In Exp. 6 the fluid was not altered until some time had elapsed, and then it became of a pale blue colour.

In Exp. 7 no alteration was produced in the fluid at first; it afterwards became of a light green colour, yielding, however, no precipitate.

These experiments were made for the purpose of removing any source of fallacy that might have arisen from the nitric acid, in Exp. 5, producing a blue colour with ferrocyanate of potass, independently of any iron which might have existed previously in the fluid; for the stronger acids, when added to ferrocyanate of potass, are capable of partly decomposing the latter, and causing, after a time, a precipitation of prussian blue. Exp. 6, however, shews that the acid produced no such effect until after some time, whereas in Exp. 5 it was produced immediately.

Exp. 7 also shews that a fluid equally yellow, and more acidulous, did not produce the same effect with the same quantity of ferrocyanate of potass, even after some time. It is clear, therefore, that the fluid in Exp. 5 must have contained per-oxide of iron, and that that metallic oxide was obtained from pus. Since mucus when treated in a similar way gives no evidences of the presence of iron, I think that where we can obtain a sufficient quantity of matter for operation, that this mode of examination would be found, if not a decisive, at least an important auxiliary test for distinguishing pus from mucus. I shall now conclude the subject of urinary deposits by describing the appearance of the purulent deposit, as it is found in the urinary secretion.

Tests of Purulent Urine.

Urine containing pus will almost invariably be found of a pale colour; it will be found sometimes acid, at others alkaline; and although somewhat, is not nearly so prone to undergo decomposition as is observed in some of the worst forms of the phosphatic deposit. If urine of this character be allowed to remain at rest, the purulent matter subsides after some time, having a greenish yellow tint, which is highly characteristic of it. This deposited matter is ropy, and capable of being, in some cases, drawn out into threads. When thrown upon a filter, it appears in the form of a perfectly opaque coagulum, of a greenish-yellow colour, very different from ordinary mucus, as found in the urine. When treated with three reagents, which were noticed when speaking of pus generally, it behaves precisely in the same way towards them.

The urine from which this deposit has taken place will be found to be invariably albuminous, coagulating by heat and by nitric acid. Tincture of galls, corrosive sublimate, and alum, all cause either a precipitate, or manifest turbidity. This albuminous impregnation of the urine is depending, I believe, on the serous portion of the purulent matter being diffused through the urine, the deposit consisting of the insoluble opaque particles, mixed with a larger or smaller proportion of mucus.

ON SECONDARY UTERINE HÆMORRHAGE.

By John Roberton,

One of the Surgeons to the Manchester Lying-in-Hospital.

The use of the term "secondary" in this connexion is warranted by such circumstances as these:—A woman who perhaps has had a favourable confinement, the placenta having been naturally thrown off, accompanied with no

undue discharge of blood, is seized suddenly and unexpectedly, after the lapse of one, two, three, or even four weeks, with uterine hæmorrhage. Under this head I do not include the menorrhagia lochialis, or constant draining of blood; neither that description of hæmorrhage which, commencing at the time of, or very soon after, the expulsion of the placenta, returns repeatedly at short intervals (generally attended with more or less after-pains) for days or even for weeks; as little do I include floodings caused by portions of the placenta suffered to remain in the uterus.

Although secondary uterine hæmorrhage is not of frequent occurrence, I imagine few in extensive practice but must have seen it. It has, however, obtained less notice in works on midwifery than might have been expected. Even in Mr. Ingleby's Treatise on Uterine Hæmorrhage, (the latest and the most complete on this subject we possess,) it is alluded to chiefly in an incidental manner. Under the head of "hæmorrhage with a firm contraction of the uterus," the writer intimates having witnessed, in several instances, blood discharged "in quantity sufficiently great to reduce life to immediate danger at so late a period as three weeks after delivery, even when the uterus is apparently *small and well contracted**." Again—"I have used the plug with the best effect in an hæmorrhage imminently dangerous, as late as fourteen days after delivery, the uterus being *firmly contracted*†." In another place he remarks—"I could add several striking cases in confirmation of the efficacy of superacetate of lead in uterine hæmorrhages arising at late periods after delivery‡." In the elaborate work of A. C. Baudelocque, on the Internal Hæmorrhages of the Uterus, I cannot discover that what I have defined as secondary uterine hæmorrhage is at all distinctly noticed§.

Perhaps it is this description of secondary hæmorrhage that Burns refers to in the following passage: "It may happen that from some agitation of mind, or morbid state of body, the uterus may not go regularly on in its process of contraction or restoration to the unimpregnated state. In this case the cavity may be filled with blood, which forms a coagulum, and is expelled with fluid discharge. The womb may remain stationary for a considerable time, and the coagula be successively expelled with slight pains and no small degree of hæmorrhage. These symptoms very much resemble those produced by the retention of part of the placenta, and cannot easily be with certainty distinguished from them, &c." p. 481. Here, however, nothing is said as to *the length of time which has elapsed after delivery*, when the uterus begins to expel coagula and fluid blood. In the above work of Baudelocque there are some interesting remarks on a kind of bloody discharge, which this writer supposes to arise, partly from the womb after delivery chancing to lie too obliquely to permit the due escape of the lochia, and partly from deficient uterine contraction, owing to which clots form in the uterine cavity; the effect of such accumulation being severe after-pains, which often annoy the patient for many days, until the uterus is emptied. To prevent this state of things, Baudelocque advises the accoucheur to carry two fingers into the womb after delivery, "pour s'assurer de l'etat de sa cavité, et la débarrasser des corps etrangers qui peuvent y être restes." Adding, "cette pratique est celle que suit M. Deneux depuis plusieurs annees, et a laquelle je ne manque jamais d'avoir recours."—Pp. 263 and 264.

Of two or three of the earlier cases of this species of flooding which came under my care I do not possess notes: but of a few I have memoranda, scanty indeed, yet perhaps copious enough to convey some notion of the nature of the accident, which is further and better illustrated by notes of cases kindly and liberally furnished me by several of my friends and colleagues.

CASE I.—April, 1829. Mary Noble, a patient of the Lying-in-Hospital, had a favourable labour, attended by a free but not alarming discharge of blood. On the fourth day after delivery she felt herself so well as to be induced to remove her binder, and begin attending to her household affairs. From this period till the end of the fortnight, things seem to have gone on favour-

* Ingleby on Uterine Hæmorrhage, p. 227.

† Ibid. p. 247.

‡ Ibid. p. 255. Allusion is made to this kind of hæmorrhage in Blundell's Lectures on Midwifery, London, 12mo. p. 102; in Campbell's Introduction to the Study and Practice of Midwifery, p. 308; in Denman also, p. 442.

§ Traité des Hæmorrhagies internes de l'Utérus, &c. Bruxelles, 1832.

ably; but at this time, as she was seated by the fire, a copious discharge of blood took place, producing syncope. On reaching her shortly after I made a careful examination *per vaginam*, without, however, detecting any circumstance calculated to explain the cause of the hæmorrhage. The uterus did not strike me as being larger than it ought to be, and the os uteri was soft, and as far as I could judge natural. The treatment consisted in strict confinement to bed, small repeated doses of ergot of rye, and laxatives. There was no return of the hæmorrhage. From a later entry I find that this patient became dropsical; and that she ultimately recovered.

Case II.—Nov. 5th, 1829. —— Mellor, aged 23, a fortnight confined of her first child, very imprudent in the liberties she has allowed herself as to both diet and exercise. Last night a discharge of blood took place from the vagina that nearly extinguished life. To-day there has been a further gush. The pulse is feeble and rapid, and the mind is barely coherent. On examining by the vagina I find the os uteri open enough to admit the finger, and the uterus itself larger than is usual so late after delivery. Ergot of rye was given every fourth hour, in doses of 6 grains; a sponge plug was passed, a laxative was ordered, and the recumbent position strictly enjoined. The patient slowly recovered, without having had a return of the hæmorrhage.

Case III.—May 1st, 1833. — Kerney, aged 26, seven days delivered of her second child. The labour had been favourable, followed by some, but not a profuse discharge. Was doing well till 5 o'clock this morning, when, in getting out of bed to make water, a large hæmorrhage took place, which the attendants estimate at three quarts, but which I have reason to believe was under a third of that quantity. The patient appears feeble and reduced. She informs me that a similar accident occurred in her former confinement, which happened about a year ago, and then the flooding occurred nearly a month after delivery. The os uteri is a little open, but I cannot say that the womb is expanded. Subsequently the hæmorrhage returned twice, but less profusely than in the first instance. The super-acetate of lead was freely administered, and the patient (how far owing to this medicine I will not say) had a favourable recovery.

Case IV.—Nov. 25, 1833. Mrs. W., the mother of several children, enjoying good general health, but who is lame from rheumatism, was brought to bed on the 29th of October, that is, 27 days ago; the labour unattended with any degree of hæmorrhage. Her recovery had been so far favourable, and she had left her bed-room. Yesterday, while seated by the parlour fire, she discharged a clot of blood the size of an egg, followed by several other clots, when she became faint, and in this state was removed to bed. There was no return of hæmorrhage.

Case V.—Dec. 11th, 1835. — Inglesby, aged 27, delivered of her first child nine days ago. The labour was in every respect natural, and she was doing well till to-day, when, after a shivering fit as she sat by the fire, a considerable hæmorrhage issued from the vagina, chiefly in the form of clots, producing faintness and vomiting. She was immediately put to bed. Some degree of hæmorrhage continuing, four ten-grain doses of ergot were given at intervals of half an hour, seemingly with effect; for the patient, who was not much weakened, soon recovered.

The following case occurred in the practice of Mr. Fawdington: these are his notes.

Case VI.—Mrs. S. H., 31 years of age, spare in person, and delicate in health, was delivered of her first child, September 3d, 1835, after a natural and rapid labour, the placenta being expelled without any assistance in about half an hour, unattended by any sanguineous discharge worth notice.

Up to the 10th day after delivery, viz. September 14th, my patient had done very favourably. Her strength and spirits had been good, the lochia natural, the secretion of milk moderate; and she had latterly sat up in her bed-room a few hours daily. But in the course of this day she began to complain of a sense of bearing down, and of not feeling herself quite well; in consequence of which she was desired to keep her bed. In the course of the following night coagula were expelled from the vagina, amounting, it might be, to about

a pound. The next day, although she kept strictly in her bed, there was some trifling hæmorrhage, but the ensuing night it came on with alarming violence. During the ten following days, that is till the 24th of September, although various means were adopted to stay the discharge, it continued to return at intervals, until my patient was in a state of the most deplorable exhaustion—nearly pulseless, the extremities cold, the mind at times wandering, and she incapable of enduring the slightest change of position, such as in voiding her urine, without syncope. Indeed, for several days her urine had to be statedly drawn off.

After the cessation of the hæmorrhage, three weeks elapsed before my patient could leave her bed. She, however, slowly, but perfectly recovered.

Mr. Windsor furnishes me with Cases 7 and 8.

Case VII.—June 28th, 1835. Mrs. H., the mother of several children, was seized on the sixteenth day after delivery (she was confined on the 12th of June) with profuse hæmorrhage from the uterus. Previously to this time she appeared to be doing very well; the labour had been propitious, and there had been no trouble with the after-birth. The lochia and lacteal secretion were natural. She complained at times of pain in the uterine region, not of the nature of after-pains.

Mild opiates and aperients were prescribed, and cloths wrung out of vinegar and water were applied to the external parts; by which means the hæmorrhage abated for a time.

But two days after, having moved rather too much, the discharge returned so copiously as to induce alarming syncope, with its usual attendants; so that her life was apparently in great danger. It happened that I was in the house just as the hæmorrhage came on. The means used on the former occasion were now repeated; and added to these was the introduction of a sponge plug, moistened with vinegar and water. The pulse being scarcely perceptible, and the countenance pale and exsanguineous, wine was freely administered, with decidedly good effect. Afterwards she had ergot, in doses of fifteen grains, repeated every six hours.

By this plan the patient gradually recovered, and the hæmorrhage did not return, at least to any considerable extent. The plug was changed every, or every other, day.

July 28th.—Is gradually recovering; has a frequent pulse, some headache, and slight œdema and pain of legs. The sponge plug being now only a little stained, it was finally withdrawn to-day—five weeks and two days after her confinement.

August 28th.—Has had no return of the catamenia; œdema gradually diminishing; general health pretty good. The infant has to be suckled by a wet nurse.

Case VIII.—Dec. 31st, 1835. Mrs. B. was confined of her second child after a few hours' labour. The placenta was discharged naturally, without any unusual flow of blood.

On the 10th of January (till which time she had been doing well), she was seized rather suddenly, after going into the next room to see a sick sister, with severe uterine hæmorrhage. I visited her not long after, and found there had been a profuse discharge of blood, and large coagula were lying about her. The os uteri and vagina were in a very relaxed state; she was almost pulseless, pale, and exsanguineous. A plug was used, and the same means as in the former case. The use of the plug was persevered in only for a few days. She recovered more rapidly than Mrs. H., without any troublesome sequelæ, and is now perfectly well.

Case IX.—(The notes furnished by Dr. Pendlebury.) Mrs. H., aged 20, lusty, but not robust, delivered of her first child on the 13th of August, 1835, after, as I was given to understand, a favourable labour, of about six hours duration. The after-birth, too, I also learnt, was naturally expelled, without hæmorrhage, and the lochia, and the secretion of milk, were duly established.

It was not till the tenth day after delivery that any unpleasant symptom appeared. On this day she had sat up talking too much with her friends, and had also suffered much anxiety respecting her baby, which was drooping, and not likely to live. In the evening, before returning to her chamber, she suddenly discharged a quantity of coagula, became faint, and had immediately to be put to bed. In the course of the night the discharge returned repeatedly; and

once, as her husband, who was her accoucheur, informed me, she fainted away.

On the following day (the eleventh from her confinement), I saw her for the first time. A degree of hæmorrhage continued, returning at short intervals; and this state of things varied little throughout the two following days, blood being discharged, sometimes profusely, both fluid and in clots. The countenance was blanched, the pulse weak and rapid, and on one occasion the mind was observed to wander.

On laying my hand on the lower abdomen, I thought that the uterus was too much expanded. The treatment consisted in keeping the patient in the recumbent position in bed; in giving, in the first instance, brandy and laudanum; and in causing cloths, dipped in cold vinegar and water, to be applied over the abdomen. The ergot also was ordered, in small repeated doses. From this period the case, upon the whole, went on favourably; only, at the end of another fortnight, she parted with a few clots. Her health is now established, and she is again pregnant.

Case X.—(The notes furnished by Mr. Mellon). Mrs. M., 28 years of age, stout and leucophlegmatic, was confined of her first child December 25, 1835. The labour was rather tedious, but natural; and the placenta was thrown off in about twenty minutes, without flooding. The uterus afterwards appeared to be well and firmly contracted. The secretion of milk was abundant, and the lochial discharge quite moderate. In a few days she could with comfort sit up in bed, and on the eighth day she sat up in a chair for about five hours. So far, there had been no unpleasant occurrence. In the evening of this day, however, she complained of a weight in the sacrum, and a sense of coldness. Suddenly, about three o'clock the following morning, she was taken with alarming hæmorrhage: at a guess, I should say, from the quantity of clots I was shewn on my arrival, that she must have lost at least a couple of pounds. The pulse was small, and scarcely to be counted; the face extremely pallid, and the surface cold. I had the pelvis raised, the temperature of the room lowered, and I gave an opiate and a mixture containing the sulphate of magnesia. The utero-abdominal tumor was to be felt somewhat enlarged. At the end of an hour I left the patient: there was still a slight discharge, but no considerable return happened; only two days thence she discharged a pale clot. Her recovery was slow, and she has since had profuse leucorrhœa.

Concerning the foregoing ten cases of uterine hæmorrhage, it is worthy of remark, that in none had the labour preceding been severe; nor does it appear that in any the placenta was extracted by the hand, or that there was much hæmorrhage accompanying, or immediately following, the expulsion of the placenta. In one case the hæmorrhage occurred on the seventh day after delivery, in one on the ninth, in two on the tenth, in one on the eleventh, in two on the fourteenth, in one on the sixteenth, and in one on the twenty-seventh. In six of the ten cases there was a single attack only of hæmorrhage; in the remaining four, hæmorrhage occurred oftener than once.

My observations in the way of comment shall be few. The form of flooding now under consideration is sometimes troublesome to manage, as may be gathered from several of the preceding cases. In a case which was under my care a number of years ago, but of which I am not in possession of notes, the hæmorrhage returned again and again for several weeks, until I despaired of the patient's life. When, by rest and other means, I flattered myself that the risk of flooding was over, no sooner did she begin to sit up than it was renewed. I have never myself witnessed an instance that ended fatally, but my very experienced friend, Mr. Windsor, has favoured me with such a case, in the following brief note:—"About fifteen years ago, I was hastily summoned to a woman (not a patient of mine), in Bradford-street, who I learnt had been confined about a month. I immediately went to visit her, but on my arrival found she was dead; which was owing to a sudden and great hæmorrhage from the uterus, that came on as she was walking across the floor."

When death occurs, as in this instance, it is of course impossible to tell, unless from the evidence of a post-mortem inspection, whether the hæmorrhage may not have depended on local

disease of the uterus. The following case, with which I am favoured by one of my colleagues at the hospital, Mr. Clough, illustrates this remark, and will, I venture to think, be read with interest:—

"Mary Powall, æt. 25, of a delicate habit of body, was delivered of her third child on November 4, 1834. The case was, I believe, a hand presentation. Turning was easily effected, but, in extracting the child, the head was incautiously allowed to catch upon the brim of the pelvis; the face being directed rather forwards towards the pubes, the chin resting upon that portion of the bone above the left obturator foramen. This position of the head was, after a time, rectified, when it almost immediately descended into the pelvis, and was expelled rather quickly by the natural efforts. The placenta was expelled without any unusual symptoms. The patient had not much uterine discharge succeeding her labour, but she soon began to complain of pain and tenderness on pressure in the left hypogastric region, for which she was ordered between forty and fifty leeches. Her pulse was never much excited, nor had she any vomiting. The tenderness and pain of the abdomen did not entirely subside, but was confined to a small part of the left hypogastric region. There was no distention of the abdomen, and the bowels were pretty regular. After the first four days she seemed to be recovering well, when on the 8th day she was seized with severe vomiting, and towards evening a sudden and very copious flooding came on. This continued until morning, large clots escaping: the quantity lost I should think could not be less than two or three pounds. I saw her early the morning after (the ninth day after her confinement); the hæmorrhage had then ceased; her extremities were cold; there was death-like paleness, and the pulse was hardly perceptible. The hæmorrhage had completely soaked through the bed, and escaped on the floor. The belly was not tumefied, but there was some degree of tenderness, and there was vomiting. Brandy was administered, and in the course of an hour or two she seemed a little revived; the hæmorrhage, however, returned in the evening. She took the acid infusion of roses, with the tinct. opii, and was kept strictly quiet. On the 10th day she seemed a little improved, but in the evening the flooding again returned to an alarming degree. On the 11th day very low. Ordered pulv. secal. cornut., to take wine, and to have ol. ricini. 12th day, vomited the powders; pulse small, and feeble. She lived until the 18th day. There still was occasional hæmorrhage, great prostration, sordes on the teeth, furred tongue, and mucus in the throat: no pain of abdomen for the last few days, except when pressure was made, but the vomiting continued more or less until she died. Super-acetate of lead, and opium, were given, without any effect.

Postmortem inspection.—The peritoneal surface of all the viscera perfectly healthy, but very much blanched. The bladder somewhat distended with urine, and the uterus not so much contracted as is usual. Near the neck, on the left side, and between the folds of the broad ligament, there was some appearance of extravasation: and a sac partly filled with bloody pus was opened, which sac communicated by a large aperture with the general cavity of the uterus. On laying open the interior of the uterus, there was the appearance of a deep excavation or ulceration, capable of readily admitting a finger or two, leading to this sac. The other parts of this organ were perfectly healthy. The uterus was shewn to some of my professional friends, one of whom considered the excavation in question as an abscess; but two or three others, besides myself, looked upon it as a partial rupture of the organ, the opening communicating with the general cavity of the uterus being too large for an abscess to have existed there.

Madame Boivin mentions a patient who, after a difficult labour, caused by deformity of the pelvis, was taken with hæmorrhage on the twenty-second day, and on the thirtieth died, from a return of the same. On inspection, it was found that an abscess, collected in the neighbourhood, had pierced the vagina; and in so doing opened a varicose sac formed by one of the lumbar veins, and thus established a communication directly with the vena cava*.

This form of uterine hæmorrhage, it is to be remembered, is comparatively of rare occurrence, happening, probably,

* Mémoire sur les Hemorrhagies Internes de l'Uterus, page 139.

not to more than one in several hundred puerperal women. Perhaps the following remarks may tend to elucidate its nature.

The uterus pours forth a periodical secretion, which is not blood, but a fluid *sui generis*; nevertheless, it is nearer blood in its qualities, and is produced far oftener mixed with real blood, than any other secretion whatever. Hence in certain women the transition from menstruation to a bloody discharge is easily occasioned by a variety of causes. I remember a female servant, habitually liable to copious menstruation, who could not lift a heavy weight, nor work the pump to draw water, without bringing on the menstrual discharge, or perhaps I should rather say a degree of flooding; yet she was healthy, for she married, and has had a number of children. Other instances similar to this I have seen, and they are far from uncommon: they tend to show how readily, in some women, blood is poured out by the uterus,—a fact which leads me to remark, that secondary uterine hæmorrhage is probably allied in its nature to menorrhagia, only the discharge is in general more copious, on account of the comparatively greater size of the uterine vessels, and the greater readiness with which the uterine cavity admits of expansion in the puerperal than in the unimpregnated state. I doubt if it will do to ascribe this form of hæmorrhage solely to a relaxed uterus,—that is to say, to the uterus contracting too tardily in its transition from the puerperal to the ordinary condition; for where is the convincing evidence that *prior* to the hæmorrhage such relaxation really exists? After the hæmorrhage, which mostly consists both of clots and fluid blood, has burst forth, the uterus, it is true, is commonly to be felt enlarged in volume; but similar, although slighter enlargement, is often to be detected after profuse hæmorrhage from the uterus not succeeding parturition, such increase of bulk being, in the latter instance, and generally, I am inclined to think, in the former also, the effect of distention caused by the blood having been more or less accumulated in the uterine cavity. As little will it be satisfactory to attribute the occurrence of secondary hæmorrhage to the imprudence of patients in forsaking too early the recumbent position; for in truth nine out of ten of the wives of the operative class are up and stirring about by the seventh or eighth day after labour, and that (be the other ills which result from such a practice what they may) without the production of hæmorrhage.

I regret that the state of the circulation preceding the hæmorrhage did not receive more attention in taking down notes of the cases, for this is an important particular. Respecting two of the cases, it is noted that there was a feeling of chilliness previous to the attack, indicating, probably, a febrile state. Upon the whole, to dismiss the matter, I incline to think, although I desire to be understood as not speaking with much confidence, that this form of uterine hæmorrhage depends in a considerable degree on an hæmorrhagic diathesis, whose manifestation, in the discharge of blood from the uterus, is determined by the peculiar condition of that viscus in the puerperal state *.

With reference to methods of treatment, perfect rest in the recumbent position is essential; compression of the abdomen ought to be made by means of a well-adapted flannel binder; in the outset opium is useful; the plug is sometimes necessary; and the ergot of rye, in small repeated doses, I regard as the most important remedy of all. The diligent administration of alterative and aperient medicines is of very great importance, not in this kind of hæmorrhage only, but generally in the hæmorrhages attendant on abortion. As a tonic, I prefer to every other the sulphuric acid. In order to perfect recovery, such patients need a temporary removal from family cares and duties, the benefit of a pure air, judicious dieting, and in most cases exemption, wholly or in part, from the drain of lactation.

Manchester, Feb. 26, 1836.

* Since writing this paper, I have perused Dr. Montgomery's very instructive communication, in the 23d number of the Dublin Journal, "On some peculiar forms of Relaxation of the Uterine Tissue." This writer's cases and remarks refer chiefly to an extremely relaxed state of the organ resulting from abortion, which exposes the patient to profuse hæmorrhagic and leucorrhœal discharges. It occurs "most frequently after early abortions, sometimes after delivery at the full time, when there has been profuse hæmorrhage, and sometimes as the result of protracted undue lactation." How far several of the cases of hæmorrhage I have detailed are to be attributed to such a condition of the uterus as Dr. Montgomery describes, I will not now attempt to discuss. I with pleasure, however, direct the reader's attention to the communication in question, which deserves a careful perusal.

STATISTICS OF ST. MARYLEBONE INFIRMARY, FOR THE YEAR ENDING DEC. 1, 1835;

With Observations.

By John Clendinning, A.M. & M.D.

The Marylebone Infirmary has been within the last ten or twelve months the subject of several communications to the Medical Gazette. But the author of those communications, and of this, has reason to believe that, in a statistical point of view, a review of the whole year will be found to present advantages and interest sufficient to justify its publication, even at the risk of partially repeating matter already in print. He conceives that the facts of the late year, contrasted with and illustrated by those of several preceding years, for which he is now in possession of numerous tabulated data, must possess some interest for such as concern themselves about medical statistics. The author, therefore, feels happy to be at length enabled to redeem his pledge, given in his summer's Report of the Diseases of Marylebone, to furnish a medico-statistical account of several recent years' experience at the Marylebone Infirmary. The author purposes first to state the facts of the past year, and afterwards to offer such facts of former years, or from the experience of other institutions, as he may possess, and think proper for proof or illustration.

At the Infirmary the total of "Doctors' orders," or applications for medical relief from out of doors, and exclusive of the Workhouse population, for the year ending December 1, 1835, amounted to 3918: of these 751 were admitted into the wards, and the rest prescribed for by the resident surgeons-apothecaries, at the Infirmary; or visited by them at their own homes, if unable to attend at the out-patients' room. Add to the preceding, 1300 admissions from the Workhouse, and the total of sick, in and out, for the year, is 5218.

To the preceding I may add, that the Workhouse contains 214 beds, in fifteen convalescent or infirm wards, occupied at present by 226 persons—aged, epileptics, imbeciles, paralytics, or lying-in women. But I have not been able to procure any further data respecting that portion of the parochial establishment.

The admissions into the Infirmary for the year ending December 1, 1835, have been 2051; of these, 1256 were entered of the physicians' wards, and 795 of the surgeons'. The particular ages and diseases of the patients admitted will be found below, with the exception of about 150 cases of children admitted into the physicians' wards, and of about 30 admitted into the surgeons' wards, whose diseases or ages have not been recorded. Of those, the majority might be considered rather as transitory inmates than patients, their ailments having been too slight to require hospital assistance in most instances; some also having had no ailment whatsoever, being children thrown on the parish, and *in transitu* to the Workhouse schools, but placed in the Infirmary for previous medical examination. All those cases are unavoidably omitted in the table of admissions, and most of them in the table of ages, but they are all included in the other calculations.

The greatest number in the house at one time, during the year, was in February, in the second week, when there were 110 physicians' cases, and 122 surgeons'. The least number in the wards happened in September, the second week, when there were but 65 medical, and 71 surgical patients. The average daily population was 203; viz. 108 physicians', and 95 surgeons' cases.

It will be observed that the author's *annus medicus* begins with December 1, rather than the first of the civil year. His reason has been, that the solstices and equinoxes, the middle points of the seasons, physically speaking, though not strictly coinciding with the means of the medical seasons, yet on the whole differ from them not very widely, and principally by falling somewhat later in the year; so that without any important error, the 1st days of January, April, July, and October, may be assumed for medico-statistical purposes, as the middle points; and the last days of February, May, August, and November, as the *termini* of the seasons. Such an arrangement, whether correct or not, at all events accords sufficiently well with what he knows of medical statistics.

Admission Table, from 1st December, 1834, to 1st December, 1835, Marylebone Infirmary.

	Spring.		Summer.		Autumn.		Winter.		Males.	Female.	Total.
	M.	F.	M.	F.	M.	F.	M.	F.			
Fever	18	29	30	30	39	28	27	41	114	128	242
Inflammation of lungs	10	12	9	10	8	12	10	10	37	44	81
Consumption	16	12	9	16	17	13	26	23	68	64	132
Pulmonary catarrh	5	9	3	6	4	14	25	18	37	47	84
Insanity	4	16	10	13	5	4	9	10	28	43	71
Apoplexy	8	8	3	7	5	3	5	5	21	23	44
Palsy	0	1	3	0	2	1	2	2	7	4	11
Epilepsy and convulsions	3	1	0	1	4	1	2	3	9	6	15
Erysipelas	5	3	2	2	3	4	3	1	13	10	23
Rheumatism	14	13	3	5	11	4	14	9	42	31	73
Diarrhœa	1	3	1	6	4	1	7	4	13	14	27
Inflammation of bowels	2	4	1	3	1	2	5	2	9	11	20
——— brain	0	4	1	3	1	1	1	1	3	9	12
——— peritoneum	0	2	1	2	0	1	0	4	1	9	10
Morbus chron. cordis	9	9	3	4	8	8	5	4	25	25	50
Asthma	1	1	0	0	2	1	3	4	6	6	12
Inflammation of pericardium									2	1	3
Morbus chron. cerebri									2	4	6
——— ventriculi									11	7	18
——— renum									1	0	1
——— uteri									0	6	6
——— ovarii									0	1	1
——— mesenterii									0	1	1
——— intestinorum									2	2	4
——— chord. spinalis									2	1	3
Hemoptysis									5	0	5
Hæmatemesis									0	1	1
Exanthemata									5	3	8
Dropsy									7	11	18
Colic									3	0	3
Cholera spasmodica									8	9	17
Constipation									1	2	3
Dysentery									0	3	3
Indigestion									0	1	1
Tympanitis									0	1	1
Delirium tremens									4	2	6
Concussio cerebri									2	1	3
Hysteria									0	2	2
Tetanus									0	1	1
Mania puerperalis									0	2	2
Catalepsy									1	0	1
Ague									0	1	1
Jaundice									2	4	6
Intoxication									2	0	2
Old age									1	2	3
Scrofula									10	4	14
Pertussis									5	2	7
Gout									1	0	1
Œdema									0	1	1
Cynanche									7	4	11
Urticaria									3	1	4
Aphtha 1 2 Abortio 0 1									1	3	4
Syncope 1 0 Gangrene of lung 1 0									2	0	2
Leprosy 1 0 Psoriasis 1 0									2	0	2
Purpura 2 0 Poison 0 1									2	1	3
Congestion of brain									0	1	1
Ovarian dropsy									0	1	1
Convulsions									2	0	2
Total of physicians' patients									529	561	1090

Add cases under puberty, disease and ages not recorded 1260

Surgical Cases admitted.

	Male.	Female.	Total.		Male.	Female.	Total.
Abscessus	27	23	50	Orchitis	3	0	3
Abscessus lumbalis	1	0	1	Ophthalmia	72	16	88
Amaurosis & Cataract	2	1	3	Pernio	95	7	102
Aneurism	1	1	2	Prolapsus uteri	0	1	1
Burns	8	3	11	Prolapsus recti	0	2	2
Cancer	2	1	3	Porrigo	57	36	93
Caries	0	1	1	Psora	62	118	180
Contusio	9	14	23	Retent. urinæ	2	2	4
Cut throat	2	0	2	Sphacelus	1	3	4
Fistula lacrymalis	0	2	2	Sprains	1	1	2
Fractures and Dislocations	17	11	28	Stricture of urethra	4	0	4
Hernia	3	2	5	Tetter	3	4	7
Hydrocele	4	0	4	Tumors	2	0	2
Inflamed and dislocated Joints	3	7	10	Ulcers	39	37	76
Inflam. externa	18	5	23	Venereal	20	32	52
				Wounds	5	1	6
							794

Add 27 cases diseases not recorded—Total 822

Admission Table for the Year ending December 1, 1835, arranged in decennial periods.

Under 10 years,	412	60 to 70 years,	201
10 to 20	126	70 to 80	85
20 to 30	216	80 to 90	25
30 to 40	193	90 to 100	2
40 to 50	160		
50 to 60	180	Total	1900

Not including 151 cases under the age of puberty, whose ages have not been recorded.

Number of Days' Residence of the Patients admitted into the Marylebone Infirmary, for the Year ending 1st December, 1835.

Under 10 days,	674	patients.	From 100 days to 120 days,	31	patients.
From 10 days to 20 days,	190	....	.... 120 ...... 140	17	....
.... 20 ...... 30	286	....	.... 140 ...... 160	10	....
.... 30 ...... 40	198	....	.... 160 ...... 180	5	....
.... 40 ...... 50	118	....	.... 180 ...... 200	9	....
.... 50 ...... 60	71	....	.... 200 ...... 250	4	....
.... 60 ...... 70	49	....	.... 250 ...... 300	8	....
.... 70 ...... 80	34	....	330	1	....
.... 80 ...... 90	28	....			
.... 90 ...... 100	18	....		2051	

Average stay of each .... 24 days.

[*Mortality Table*, &c.

Mortality Table, from 1st Dec. 1834, to 1st Dec. 1835, Marylebone Infirmary.

	Spring.		Summer.		Autumn.		Winter.		Male.	Female.	Total.
	M.	F.	M.	F.	M.	F.	M.	F.			
Consumption	8	14	20	16	6	7	16	14	50	51	101
Inflammation of lungs	9	10	3	5	2	1	5	2	21	18	39
Apoplexy and palsy	2	6	3	2	1	0	5	4	11	12	23
Morbus cordis	1	3	0	3	6	5	5	3	12	14	26
Fever	0	1	1	3	3	2	1	3	5	9	14
Inflammation of bowels									4	3	7
——— brain									2	6	8
——— peritoneum									0	3	3
——— pericardium									1	0	1
——— absorbents									1	0	1
Morbus chron. cerebri									1	9	10
——— ventriculi									3	5	8
——— uteri									0	3	3
——— hepatis									3	1	4
——— renum									2	0	2
Dropsy									2	2	4
Dysentery									2	2	4
Ulceration of bowels									1	3	4
Diarrhœa									0	1	1
Cholera									1	1	2
Convulsions									2	2	4
Delirium tremens									1	0	1
Epilepsy									0	1	1
Concussion of brain									1	0	1
Croup 0 1 Poison 0 1									0	2	2
Scrofula 1 0 Purpura 1 0									2	0	2
Diseased mesentery 0 1 Diseased rectum 1 0									1	1	2
Old age 0 2 Tetanus 0 1									0	3	3
Erysipelas 3 0 Burns 2 4									5	4	9
Fractures 1 1 Aneurism 2 1									3	2	5
Cancer 2 1 Cut throat 1 0									3	1	4
Sphacelus									2	4	6
									142	163	305

Ages of Patients Dead in Marylebone Infirmary Wards, during the year ending Dec. 1st, 1835, in decennial periods.

Under 10 years of age	35 patients.	From 60 to 70	48 patients.
From 10 to 20	16	.... 70 .. 80	43 ....
.... 20 .. 30	33	.... 80 .. 90	9 ....
.... 30 .. 40	37	.... 90 .. 100	2 ....
.... 40 .. 50	31		
.... 50 .. 60	51		305

Observations.—The preceding differ considerably in several respects from the results of former years. The admissions are less, and the average population, and average stay in the house, are less, but the deaths are more, than the corresponding averages of the last five years, ending January 1, 1835. For that period the annual average of admissions was 2260; viz. 1260 medical, and 1000 surgical cases; and the average daily population was 213. The average annual mortality was 289—the average stay in hospital was 34·2 days—and the proportion of deaths to days of sickness : : 1 : 261; so that, for the year ending December 1, 1835, the mortality has surpassed the five years' average by more than 1-7th, and the average stay exceeded the corresponding five years'

average by nearly 1-3rd; while the days of sickness fell short of the five years' average by 2-5ths, being to the deaths : : 150 : 1 instead of : : 261 : 1. The mortality of the past year was then, on the admissions, 15 per cent., instead of 12·50 per cent., the average for the last five years. That considerable increase of mortality seems owing *indirectly* to the circumstances that the number of physicians' patients has not materially diminished, and that the decrease has principally happened in the surgical population; and *directly*, to an absolutely increased number of some of the more intractable diseases in the wards of the physicians. The mortality of the surgical wards was, for the last year, 25; or about 3·59 per cent.; so that the diminution on the surgical population of about 1-5th, as compared with the average of the five years ending the 1st of January last, has diminished the gross mortality by 1-50th part only; while there has been a considerable increase of the items "phthisis" and "morb. chron. cordis" on the medical side; the annual average for five years for the former having been, according to the house-surgeon's journal, admissions 127, deaths 86; and for the latter, admissions 38 per annum, and deaths 16. Whereas for the year ending December 1, last, the admissions for "phthisis" were 132, and deaths 101; and for "morb. chron. cordis," the admissions were 50, and deaths 26. No doubt something may depend on changes of officers, and of official opinions and nomenclature, more especially with respect to "morb. chron. cord.," and, in fact, I observe that within the last few years the item "*dropsy*" in the official journals has gradually been subsiding, and, together with *asthma*, giving place rapidly to the formidable item, "morb. chron. cord.," or to "bronchitis chron." Yet I think, after having made all proper allowances, something still remains to be placed to the account of the disease last mentioned; while, with regard to "phthisis," the most obvious, and long, and universally-known *rational* signs, sufficiently distinguish its advanced stages, in a vast majority of cases, to the least discriminating observer; so that I can have no hesitation in placing to the credit of 1835 the whole of the difference between the average mortalities of the last five years from "phthisis," and the mortality of that year, viz. 15, which alone will reduce last year's mortality rather below the annual average since 1831. But though in this way the difference between the gross mortalities may be accounted for, the differences between the times of residence in hospital, and the ratios of deaths to days of sickness, remain apparently unexplained, or at least not clearly accounted for; for in former years the gross mortality was less, and the average daily population, and average stay, and ratio of sick days to deaths, were greater; so that there must have been something additional in the diseases or other circumstances of the sick, to cast the scale against the late year. One cause may possibly be the diminution of the Workhouse population. Those have the "entree" into the Infirmary without "Doctors' orders," or overseers' permission, and have often gained admission, although in reality not labouring under dangerous ailments. That diminution appears, from an official statement before me, to have amounted to about one-tenth (from an average of 1058 to that of 940) since the full practical establishment of the new Vestry Bill; and that diminution in numbers in the Workhouse has increased, not diminished, the supply of sick to the Infirmary, as I apprehend, in this way. The really necessitous have submitted to the improved discipline,—the idle and able-bodied only have retreated. The result has been an increase in intensity of the mortality of the adult admissions from the Workhouse, as compared with other years, or with the mortality of the admissions from out of doors, or the parish at large. Another cause is, I have no doubt, a larger admission of children with slight ailments, such as catarrh, ophthalmia, lepra, scabies, &c. contracted in the play-ground, &c. of the Workhouse; for on referring to the return last alluded to, I find that for five years the proportion of boys and girls in the Infirmary, or persons between extreme infancy and puberty, has been pretty steadily increasing: so that from an average of less than 40 daily, in 1831, they have now reached 80 and upwards.

[To be continued.]

INJURY OF THE SPINE:

LIGAMENTS RUPTURED — VERTEBRA DISPLACED—NO PARALYSIS—RECOVERY.

To the Editor of the Medical Gazette.

SIR,

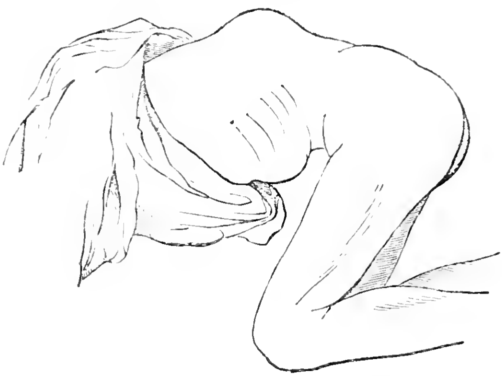
THE case of injury of the spine related by Mr. Stafford to the Royal Medical and Chirurgical Society, and reported in the last number of your Gazette, resembles a case which occurred some years ago in the Middlesex Hospital, and of which I have preserved the notes and a drawing. Perhaps these may be interesting to your readers.

I have the honour to be,
Your obedient servant,
ALEXANDER SHAW.

10, Davies Street, Berkeley Square,
29th Feb. 1836.

John Barnes, aged 32, a bricklayer, was admitted into the hospital, 18th August, 1828, having fallen from a scaffolding about thirty feet high. He was insensible for some time: when he recovered he was bled to sixteen ounces; and was brought to the hospital an hour and a half after the accident.

He lies in bed with his body much bent: at the lower part of his back there is an angular projection, which, on examination, is found to be produced by the displacement of the last dorsal and first lumbar vertebræ; the tumefaction is very considerable over the bones, yet a depression, to the extent of about an inch, can be felt between the most projecting spinous processes; and the transverse processes stand so much out, that they press against the skin. He has perfect power in moving his lower extremities; and the sensibility of the skin is entire. He complains chiefly of the pain occasioned by several severe cuts



and bruises on the face, and is observed to move his legs up and down, expressive of suffering.

August 27th.—The progress of this case has been carefully watched, although the daily reports have not been kept. No symptoms of paralysis have appeared. On the second day, being fatigued with lying in one position, the patient turned himself round to the other side. He was cautioned against doing this; but was seen the next day turning round as before. The tumefaction has subsided; and the irregular projections of the vertebræ can be distinctly seen as well as felt. He has been purged, has taken salines and antimonials, and has had leeches repeatedly applied to the back, and cold lotions.

October 4th.—The patient remained free from symptoms of the spinal marrow being injured; and he left the hospital a fortnight ago. On visiting him at his house, I found him dressed, conversing with his neighbours. The position in which he was placed was remarkable. Instead of sitting, he stood by the side of a low table, and leaning forwards, with his body much bent, he kept his chest flat upon the table; thus resting the upper part of his body upon the table, to relieve the spine of the weight. This position, he said, was the easiest that he could find, except lying in bed. To show the way in which he walked, he began, before leaving the table, to prepare himself; and for this purpose, he grasped the front

part of both his thighs, a little above the knee, with his hands; and then, fixing his shoulders, and supporting his weight on his arms, with his knees bent, and taking short steps, he walked across the floor till he reached another table, where he proceeded to place himself as described above. On examining the spine, the bones were prominent as when he was in the hospital. He attempted to raise himself into the erect position, and he succeeded to a remarkable degree; but although the effort did not give him pain, the spine was so weak at the seat of the injury, that he quickly sank down again into the bent position.

August 11, 1830.—It is now two years since the patient met with the accident. I was therefore interested to learn what was his condition. I found his wife only at home, but she informed me that for a year and a half her husband has been in constant employment, in his former occupation, as a bricklayer. Although he lives in Paddington, he walks to Lincoln's-Inn-Fields, works during the day, and walks again home in the evening. His back is still flexible at the part where it was injured, and the bones project so much that the skin is occasionally fretted over them. He complains continually of the weakness and pain in his back, and is relieved by lying down.

Remarks.—The circumstance which gives the principal interest to this case is, that the patient had none of the symptoms indicating that the spinal marrow was injured. When we consider the complicated manner in which the vertebræ are locked into each other, and the powerful ligaments that bind them, and then estimate the force necessary to separate them, as in the above case, we cannot but be surprised that such a delicate organ as the spinal marrow, lodging within them, should escape injuries and retain its functions. The instances, however, are not uncommon, of accidents similar to that which I have related, where the seat of the injury was the same, being unattended with paralysis. I may refer to an interesting example of this kind, in Sir Charles Bell's "Surgical Observations," p. 140. The question thus arises—what is there peculiar in the anatomical structure at the lower part of the spine, which may explain the occasional occurrence of these favourable cases?

In the foregoing case, it was the last dorsal vertebra and first lumbar vertebra which were found displaced. In the other cases to which I have referred, the injury was at the same part, or only the distance of one vertebra higher up. Now it is at this part of the column that the spinal marrow begins to diminish in size, and to be surrounded by the leash of nerves that compose the *cauda equina*. When it reaches the second last dorsal vertebra, the spinal marrow, after forming a slight bulging, gradually decreases in its thickness, tapering and getting finer for some inches, until it terminates opposite the second lumbar vertebra. Hence, between these two points, it is enveloped in the numerous long and thick roots of nerves which it gives off close together, and which are destined for the supply of the lower extremities. These roots forming the cauda equina are, in point of firmness of texture, nearly the same as the common nerves distributed through the exposed parts of the body. It would appear that as the lumbar vertebræ not only sustain the greatest weight of all the spine, but enjoy considerable freedom of motion, and are, accordingly, liable to sudden and severe shocks, it was provided that the spinal marrow should cease thus high up, in order to save it from injury.

Whether this be correct or not, it is obvious that these loosely suspended roots of nerves, which occupy the lumbar region of the spine, cannot suffer in the same manner from the accidental twists or concussions of the bones at this part, as would the spinal marrow itself, if it had been so prolonged in its canal. It appears also, that where the spinal marrow is surrounded, at its termination, by these nervous cords, it must be defended by means of them, to a certain extent, from the effects of violence. Such may be the explanation why accidents, so severe in their nature as to rupture the ligaments, or even fracture the vertebræ across*, sometimes fail to produce immediate mechanical injury of the spinal marrow; and consequently if subsequent thickening of the membranes do not cause paralysis, the pa-

* See the case of Anton, in Sir Charles Bell's Surgical Observations, p. 138. The body of the eleventh dorsal vertebra was broken. There was no paralysis. The patient died from suppuration within the sheath of the spinal marrow. The spinal marrow itself did not appear to have suffered mechanically.

tient may remain powerful in his lower extremities, even when the spine is incapable, without artificial support, of sustaining the weight of his body.

REMARKS ON THE PATHOLOGY OF HYDROCEPHALUS.

With Illustrative Cases.

By Daniel Noble, Esq.
Of Manchester.

The present remarks relative to the pathology of hydrocephalus, refer, mainly, to the equivocal character of the symptoms commonly regarded as those of serous effusion within the head; and, to the frequent association of this event with inflammation of the lungs, in all probability as the immediate consequence of such inflammation.

If a reference be made to the works of any writer of the last century, or, indeed, to the generality of works of more modern date, it will be found that certain symptoms, such as stertorous breathing, fixity of the pupil, strabismus, and others of a like character, are regularly recorded as certain indications of some compression of the brain. And if depression of some portion of the cranium be not discovered, or if circumstances have not led to the conclusion that there exists some extravasation of blood, or some morbid growth, exerting such compression, the pathological inference deduced from the symptoms has always, until a recent period, been, that a serous effusion had taken place either into the ventricles, or between the membranes, of the brain. But of late years it has been most satisfactorily shewn, by the cultivation that has been bestowed upon morbid anatomy, that such symptoms as sensorial paralysis, fixity of the pupil, and stertorous breathing, may all, or each, exist, without the presence of any undue quantity of fluid within the head, or any other cause of cerebral compression; and also, that serum, pus, blood, or tumors, may all, or any of them, exist within the head, with a complete absence of those external signs. For abundant evidence upon these points, I need only refer to the works of Andral and Abercrombie, where these assertions are most fully borne out by the cases therein narrated. And, with these preliminary observations, I will now proceed to offer some remarks upon the pathology of hydrocephalus, more especially as it is observed in children; and then detail the particulars of two interesting cases of the disease, which I had the fullest opportunity of tracing from the onset to the termination, and which, in my own opinion, most completely corroborate the conclusions arrived at by modern investigators,—affording an exemplification of the association of hydrocephalus with pneumonia, as its probable consequence.

No doctrine in pathology can rest upon more satisfactory grounds, than that the train of symptoms, to which I have just alluded, afford no certain evidence of cerebral compression, but that they must be regarded as the result of certain conditions of the brain with whose nature we are but imperfectly acquainted; that, although these conditions most commonly precede, coincide with, or succeed to, the causes of compression, this last may nevertheless exist without their occurrence at all. Hence, in speaking of hydrocephalus, or serous effusion within the cavity of the cranium, we may, with strict propriety, assume the possibility of its existence, without the manifestation of its ordinarily associated symptoms. I think it right to reiterate this proposition, as, in practice, it is but too often disregarded. That the conditions of the brain originating the train of symptoms before alluded to, are nearly allied to those which give rise to serous effusion, there can be no doubt; and hence it is within the scope of my present design to refer to the various pathological states which the sure evidence of facts points out as furnishing the causes of this phenomenon. And, in the first place I may observe, that upon referring to the history of the doctrines taught upon this subject, we shall find the greatest contrariety of sentiment to exist. For example, many writers, amongst whom may be classed Goelis and most of his contemporaries, have considered hydrocephalus to be the result of inflammatory action in the brain, or its investments; whilst others, amongst whom Whytt seems to have occupied the most distinguished place, have gone to the opposite extreme, and have stated it to be associated with a diminution of vital energy, and the effusion to occur as the consequence of a laxity of the cerebral exhalants; and,

lastly, some have regarded the affection as specific in its nature, resembling inflammation in some respects, but differing from it essentially in others. But although this variety of sentiment as to the pathology of hydrocephalus may have existed, there will yet, I think, be no great difficulty in bringing all these opinions into an accordance with the facts, as they stand revealed to us by experience: for it is pretty evident that these various writers have not, in point of fact, been referring to the same disease. The source of error has mainly existed in their having regarded the serous effusion as the essence of the disease. And, as in those days the symptoms were almost universally regarded as constituting the disease, and the external signs of water in the head being considered to be uniform and unequivocal, no surprise need be felt that differences should have existed as to the true nature of the malady. But if, in accordance with the spirit of an improved and more philosophical pathology, we consider the hydrocephalus itself only as a symptom of antecedent changes—and if, moreover, we bear in mind that these antecedent changes vary in their characters, all having one of the symptoms in common, and that a serous effusion—I think we shall have a key to the elucidation of all the perplexity and contrariety that have arisen in discussions of this subject.

If we understand that the brain is subject to the same laws as every other structure in the animal economy—if we regard its investments as possessing no immunity from the susceptibilities of other membranous tissues—we shall be aware that their capillary exhalants may effuse their serum in undue proportion, under various morbid conditions. We shall know that a state of active inflammation, or a *sthenic* condition of the organization, may precede, and cause an aqueous effusion; that a diminution of energy, or an *asthenic* state, may lead to the same thing; and that some irregular action of the vessels, dependent altogether neither upon an increased nor upon a diminished energy, but on some *depraved* condition, may also occasion a like result. And as in these days we regard an appreciable change in function and organization as constituting the essence of disease, and as, in most instances, changes are so far appreciable as to enable us to discriminate between *sthenia*, *asthenia*, and *depravation*, and as these conditions differ most completely in their nature, so must cases of disease be estimated and regarded, according as the organization presents these various conditions; and not as any one symptom may happen to be absent or present.

It does not form a part of my present intention to detail in what possible manner an undue arterial excitement may lead to an exhalation of some of the serous constituents of the blood, nor how a diminution of the vascular energy, especially when combined with an undue serosity of this fluid, may bring about the same thing; nor yet how certain irregular states of the tissues may produce the result; for all these are matters of common observation. We all know, for example, how the peritoneum, in a state of active inflammation, will, in many instances, lead to an acute dropsy of the belly; under other circumstances, as after great exhaustion from uterine hæmorrhage, to a dropsy of a totally opposite character; and also, that frequently a strumous condition of the same tissue will induce an aqueous effusion within the sac. And similar phenomena ensue in like pathological states of other secreting textures, especially the membranous; and, under these circumstances, the nature and character of the disease is estimated, not according to the presence, or to the amount of the effusion, but according to the antecedent changes in the structure whence the fluid is derived.

Now it is quite clear that the membranes of the brain are subject to the same pathological changes as analogous structures in other parts of the system; and such being the case, it is obvious that the conditions to which the term "hydrocephalus" is applied, must be variable; and that consequently, the diagnosis, prognosis, and treatment, must vary according to the probable condition of structure preceding, and coincident with, the serous exhalation. And I would go farther, and assert with some confidence, that the question of mere presence or absence of effusion is a matter of but secondary importance. I know that in general the occurrence of this phenomenon is regarded as a most formidable event, and, in most cases, as the sure precursor of inevitable death; but I can only consider that such views have had their origin in the prevalent notion

that the presence of certain symptoms, to which I have before alluded, and which in a very large proportion of cases do certainly precede the fatal termination of cerebral disease;—I can but consider, I say, that these views have resulted from the prevalence of the idea that such symptoms and cerebral compression were necessarily coincident. But such opinions being unquestionably demonstrated to be fallacious, it follows that every inference that has been deduced from them must be equally so. It is true that in most cases in which we have effusion within the head, the danger to life is most imminent, but not, I contend, in consequence of the effusion, but of antecedent changes in the structure of the brain, or its membranes. I reiterate, that of the serous exhalation itself, we have no essential symptom manifested by irregularity of the functions; and I submit, that in every case our prognosis and therapeutical proceedings must be guided by the nature and extent of the cerebral disease with which it happens to be associated. The two cases the particulars of which I shall detail in the sequel, most fully corroborate the position which I have taken in the whole of these remarks; one presenting, in the highest degree, every external sign during life, commonly regarded as indicative of hydrocephalus, and yet the effusion being but inconsiderable; and the other, where the serum was most abundant, offering scarcely one of the symptoms formerly considered as the necessary consequence of hydrocephalus. These views are most fully propounded and illustrated by many modern writers, and hence it may be deemed a work of supererogation that I should submit them, in a new dress, to the members of this Society*; but believing, as I do, that they are often disregarded in practice, I do not think it useless that our attention should be specially directed to the subject—a result which may be expected to ensue from its deliberate discussion upon the present occasion.

Having thus far dwelt upon the pathological conditions associated with hydrocephalus, I will now proceed to the consideration of the second point, to which I referred at the outset of this paper—to the common occurrence of effusion within the head, as the consequence of pneumonia. That hydrocephalus is, in a great number of instances, immediately induced by the presence of pulmonary inflammation, especially in children, I am thoroughly convinced; and the conviction I believe to arise from an attentive observation of facts, capable of being combined, so as to accord with all our general notions of the laws of morbid process. In consulting any of the standard authors who have written upon hydrocephalus, we find very general allusion to its occurrence as an affection dependent upon some remote sympathy, as upon disorder of the liver, the mucous membrane of the alimentary canal, the mesenteric glands, or, indeed, of any of the abdominal viscera. Disease of the heart has also been enumerated as one of the exciting causes of cerebral affection. But, with the exception of a passage occurring in a tract on Measles, by the late Dr. Armstrong, casually alluding to the phenomenon, I have no where met with a recognition of the intimate relation which I believe to subsist between water in the head and inflammation of the lungs. My attention to this was first engaged by a clinical remark made by Dr. Whiting, physician to the Surrey Dispensary (whose practice, when a student in the metropolis, I was in the habit of attending), who, on one occasion observing an infant labouring under pneumonia, directed my attention to the turgid state of the veins of the head and neck, and stated his fears lest hydrocephalus should supervene; and, from that time to the present, I have never lost sight of the phenomenon afforded by the connexion; whose foundation, in fact, many cases, attentively observed, have, in my own mind, most satisfactorily established.

I do not mean to affirm that in every case where we have pneumonia we must expect hydrocephalus to supervene; but I do say, that if any predisposition exist, it becomes a matter of importance to attend to the best means of preventing such an occurrence. It may be asked, in what way I attempt to explain this association, in conformity with the known laws of pathology? And to this inquiry I will reply, by a brief exposition of the views that suggest themselves to me, in elucidation of the relation.

* The Manchester Medical Society—where the present paper was originally read.

It is a well-known fact, that whenever any obstruction exists to the due progress of the venous circulation, a tendency to serous effusion is induced in the radicles leading to the great trunks, where the obstruction exists. Thus sometimes we have œdema of the hands and wrists, from the glands in the axilla, in a state of enlargement, pressing upon the axillary vein. A like occurrence will also take place from undue pressure upon the femoral veins. A tuberculated or scirrhous condition of the liver, interrupting the freedom of the portal circulation, is a recognised cause of peritoneal dropsy. Now, in noting the phenomena dependent upon pneumonia, do we not discover what is precisely analogous? The blood which is sent into the lungs from the right ventricle does not circulate with its ordinary freedom: an obstruction to a greater or less extent almost always exists; and this obstruction must operate upon the whole current from behind, until we arrive at the venous radicles generally, and must influence, I apprehend, in an especial manner, the current in the veins by whose union the descending *cava* is formed. This obstruction in the pulmonary circulation, extending to the internal jugulars, and onwards to the sinuses of the brain, must tend, as a matter of course, to excite what is called venous congestion of that structure; and hence, if a predisposition to any morbid action in the brain exist, either of a *sthenic*, *asthenic*, or *depraved* character, it is highly probable that, under such circumstances, a serous effusion will take place. Indeed, this view, in connexion with the pathology of these affections, appears to me so obvious, that I can only feel surprise that it has not received more general attention.

As I have before stated, I have made many references, in expectation of meeting with a distinct recognition of the frequent association of pneumonia and hydrocephalus; but, with the exception above mentioned, I have no where seen particular notice taken of the former immediately leading to effusion within the head. On reading the chapter on hydrocephalus, in Underwood's popular work on the Diseases of Children, I find that, amongst the premonitory symptoms, "cough" and "difficult respiration" are enumerated, though he himself does not connect these phenomena with primary disease of the respiratory apparatus; but I feel little doubt that, had auscultation been practised at the time this author was an observer of disease, a crepitation would have been revealed upon application of the ear to the chest.

With regard to the character of the affection likely to be excited in the brain by the presence of pneumonia, it will, of course, depend upon the peculiar predisposition of the patient at the time. We all know that, in the great majority of instances, the brain is an organ which, in children, is at all times very much open to the influence of morbid agency; and hence, if a child be of a sanguine plethoric habit of body, with a vigorous circulation, we should expect that any cause, operating upon the brain in a way to produce disease, would excite an action of a *sthenic* character, and effusion might ensue as the probable consequence of the prolonged existence of this condition. If a child had been much exhausted by long-continued evacuations, with almost total destruction of the powers of digestion, and this had led, as a matter of course, to a diminution of the tone of the vessels, and to an undue serosity of the blood, special derangement of the cerebral circulation might be expected to induce effusion, the result of an *asthenic* condition of the brain. And the existence of a strumous diathesis, in connexion with pulmonary obstruction, would not improbably excite a *depraved* action, tending to hydrocephalus.

These various predisposing states of the system, and special conditions of the brain, might of course exist as the natural constitution of the patient; and in many cases, no doubt, as the result of modes of management. Neglect of depletion, in some instances of pulmonic inflammation, and the administration of stimulants, would evidently tend, if the foregoing views be correct, to arouse disease, of an active character, in the brain; and if the opposite course had been unduly pursued, the hydrocephalus of debility might be expected to ensue, and so on.

These remarks have their origin in attentive reading, and the careful observation of a fair proportion of cases; and as illustrating, most distinctly, the views that have been expressed in the whole of this paper, I will now proceed to detail the particulars of the two cases

of hydrocephalus supervening upon pneumonia, of which I have before spoken.

Mrs. Wilkinson's child, Newton-street, Ancoats, aged fourteen months, of plethoric habit, fair complexion, and of sanguine temperament, had been attacked with pneumonia, when about three months old, from which it had recovered under ordinary treatment. After this attack, the child improved gradually, both in health and appearance, and in every respect seemed likely to do well. When about six months old, a second invasion of the pulmonary affection occurred, from which a recovery again took place; but from this time the child's health was never completely re-established. Its bowels were disordered, with alternate relaxation and constipation; and there was rarely an immunity from nightly paroxysms of remittent fever. In the month of August, 1835, the child being a year old, diarrhœa and vomiting came on, accompanied with continued fever. The severity of these symptoms, however, in about ten days abated, but still disorder of the bowels existed more or less until the month of October, in the early part of which another attack of pneumonia occurred. This was opposed by small and frequently-repeated doses of calomel, salines, and the application of a few leeches to the chest; the warm-bath was also employed at the onset. Under this treatment the symptoms were alleviated, but after the lapse of a few days, owing to an astringent mixture being given by mistake, instead of a saline aperient, the bowels became locked up, and the febrile symptoms increased, with very great restlessness. On the substitution of the intended medicines, the symptoms again improved, when tartar emetic, in large doses, was administered, and the expectoration became abundant; after this the pneumonic symptoms almost entirely abated, but the child seemed reduced to the lowest pitch of inanition. Simple medicines, such as hydrarg. c. creta, were then given according to circumstances, and for a few days beef-tea and milk were taken with apparent relish, and a decided improvement took place; but the appetite failed, and slight paroxysms of fever occasionally occurred; there were, besides, general pallor, languor, drowsiness, and every visible indication of thorough exhaustion. At this time, fearing secondary affection of the brain, I directed cold applications to the head whenever the febrile paroxysms gave rise to increased temperature of the scalp. Besides the general symptoms of exhaustion, a harassing tenesmus existed for several days, which, however, yielded to starch injections. This state continued for about a week, when convulsions, of a slight character, came on from time to time; but these, though mild, were of long duration. Except when convulsed, the child continued perfectly sensible; the breathing was free and unembarrassed; there was no stertor, nor strabismus, nor immobility of the pupil; and according to the statement of the friends, it possessed perfect consciousness to the last, having spoken intelligibly, from its slender vocabulary, only half an hour before its death, which took place on the 28th October, 1835, as if from sheer exhaustion.

The day after its death a post-mortem inspection was obtained. From the appearances observed within the head, there can be no doubt that disease had been going on for some time. The dura mater was almost universally adherent to the calvarium. The arachnoid membrane presented the well-known milky aspect to a considerable extent; and the vessels of the pia mater were much engorged. The substance of the brain was of proper consistence, but its minute vessels were distinctly injected. A quantity of fluid, that must have amounted to many ounces, was found between the membranes and in the ventricles. This was not measured, but the quantity was certainly most unusual. That the serous effusion had only taken place just at the last, I cannot for a moment believe; I can but consider that in this instance it was the gradual result of the combined influence of diminution of vital energy, and of mechanical obstruction to the pulmonary circulation, — the former predisposing the brain to *asthenic* hyperæmia (in the language of Andral), and the latter acting as exciting cause of such condition, and the consequent effusion. The probability of the cerebral affection being, in this instance, of an *asthenic* character, was strengthened by the purple appearance of the blood contained in the arteries.

On opening the chest, pleuritic adhesions were very general, but there was

no effusion; congestion of the lungs was universal; hepatization of some parts had occurred, and in some places the consodidation seemed to be of no very recent formation.

The abdominal viscera, though indicative of long-continued derangement, presented no appearance worthy of especial note.

In this instance, then, we had successive invasions of pneumonia, which reduced the child to the lowest point of depression. All the symptoms arising in consequence seemed only of a character such as we generally observe after prolonged and gradually induced exhaustion. No external sign of special disease of the brain was present, nor any of the symptoms commonly set down as denoting effusion; and yet, on opening the head, it was found that most extensive changes had been induced, and, above all, that many ounces of serum had been poured out by the cerebral vessels.

The next case which I shall relate furnishes an instance of cerebral disease in connexion with pneumonia, occurring in a little girl of four years of age, whose family seemed to be predisposed to affections of the head; inasmuch as two or three of its members were remembered to have died of what was said to have been "water in the head." The patient was of sanguine nervous temperament, and of somewhat strumous diathesis. She was attacked with all the ordinary symptoms of pneumonia, about the middle of October, 1835. Leeches were applied to the chest, and the warm-bath was employed. After the bowels had been freely opened, calomel and salines were prescribed, in small and often-repeated doses; but, as I soon ascertained, the medicines were given very irregularly, and the symptoms increased: afterwards, the medicines were taken as directed, but no improvement occurred. The treatment was then changed, tartar emetic being administered; but no nausea, or excretion of mucus, was effected. In about ten days from the first invasion, I saw the girl labouring under every symptom denoting serious disease of the brain. Convulsions of a very severe character occurred from time to time; the breathing was rapid, laborious, and somewhat stertorous; the eyes were fixed and the pupils dilated, with insensibility to light, and the cornea presented the glazed appearance so often observed in such cases. The child died in a few hours after being seen by me in this state.

On removing the calvarium, the membranes of the brain presented every character of active inflammation; the vessels being engorged, and filamentous adhesions existing in numerous points between the two surfaces of the arachnoid. A small quantity of fluid had been effused, chiefly found in the lateral ventricles. The substance of the brain was somewhat softened, and very generally injected. The dura mater also adhered very strongly, in some places, to the inner aspect of the cranium.

On opening the chest, no pleuritic adhesions were discovered; but the lungs themselves, in some portions, evinced the signs of recent consolidation, and in others only congestion: some parts seemed quite free from the indications of previous disease. The minute ramifications of the bronchi were very generally obstructed by a thick tough mucus.

This case differs from the preceding in several very important particulars. In the first place there was, in this last, a very evident predisposition to cerebral disease; as evinced by the patient's own appearance and history, and by the fact of several members of the family having died of disease of the brain. Again: the pneumonic symptoms never abated, but their whole course was such as to induce very speedy obstruction to the pulmonary circulation, which appeared very distinctly to excite the affection of the head, which was accompanied, in a high degree, by the symptoms esteemed of old to be certainly diagnostic of serous effusion; and yet, on examining the interior of the head, the serum was found in no proportion to the severity of the symptoms; affording, in this respect, a striking contrast to the preceding case, in which the fluid must have existed to ten times the amount, with a complete absence of the equivocal symptoms.

I have related some of the particulars of the above cases, because they illustrate the position I have taken in these remarks more fully than any it has ever been my fortune to witness, and to note so carefully. They do not, however, by any means comprise the extent of the facts which I have observed, leading to similar inferences; especially as re-

gards the supervention of hydrocephalus upon pneumonia; and were not these remarks already extended sufficiently, I could still further illustrate the relation between affections of the brain and the lungs.

The practical deductions, however, which I would gather from all these considerations are, that, in all cases of thoracic disease in children, it is of the highest importance to keep a strict watch upon the state of the head; and that whenever, in such cases, any disturbance of its functions ensues, it should be met by treatment the most prompt; varying, of course, in its character, according to the particular circumstances of the case. Indeed, I would go farther, and assert, that even in the absence of any notable derangement of the head, it is highly proper, and indeed necessary, that it should receive the utmost attention in the first instance, inasmuch as we cannot be sure that serious lesions may not occur, and that, too, without the presence of any very striking symptom. It has become my own practice, in almost every case of infantile pneumonia, to direct at once that the cap be removed from the head, and that this latter be kept in an elevated position; and on the occurrence of any undue heat and dryness of the scalp, that sponging with cold vinegar and water be assiduously employed: and to these views, and to this practice, I have been led, not only from reading and clinical instruction, but also from the especial interest with which I have directed my own attention to cases of this description.

Manchester, Feb. 24, 1836.

CASE OF FATAL HÆMATEMESIS.

To the Editor of the Medical Gazette.

SIR,

As the following case appears to possess some interest for pathologists, perhaps you will give it a place in your valuable journal.—I remain, sir,

Your constant reader,

ROBERT HUNTER TEMPLE.

Islington, Feb. 26th, 1836.

William Carter, æt. 78, a pauper in the Islington workhouse, who was not under medical treatment, was taking down some articles from a shelf, when he was suddenly seized with vomiting of blood, which burst forth in large quantities from his nose and mouth. I attended in five minutes after the attack, but the man was dead. His nose, mouth, and face, were covered with blood, which was also spilt upon the floor, and about a quart was collected in a basin, into which he had vomited.

A post-mortem examination was made the next day, twenty-two hours after death, in the presence of my father and another medical gentleman, when the following appearances presented themselves. On opening the chest, the lung on the right side was found very large, and extended over to the left side. It was of a dark venous colour, and did not collapse on the removal of the sternum. On its anterior and inferior surface a dark blackish patch appeared, about the size of a hen's egg, an incision into which showed it to be composed of dark, bloody, congested matter; it was an effusion of blood into the cellular texture of the lung, not defined by any well-marked boundary, and resembling, except in the last-mentioned circumstance, the disease called by Laennec apoplexy of the lungs. Two other similar congestions were seen in other parts of the right lung. The left lung was extremely small, collapsed, almost impermeable to air, and strongly bound by adhesions to the costal surface of the pleura. The substance of this lung was soft and friable, and was torn in the endeavour to remove it from the chest. On cutting into this lung, the edges only were found permeable to air; the lower portions presented a firm, yellowish, semi-cartilaginous texture, while the upper and middle portions appeared broken down and extensively ulcerated; and the arteries were found crossing the ulcerated cavities, in the form of strong fibrous cords. No phthisical tubercles or ulcers were observed in any part, but the cavities appeared to be formed by the mere rotting of the pulmonary substance. On introducing the finger into the left pulmonary artery, the passage was found to be impervious, and on further examination, its canal was found to be closed up by the growth of a firm cartilaginous substance around its walls. This patient must have been attacked at some period of his life with pneumonia and pleuritis, terminating in hepatization and adhesions of the left

lung; and the case shows remarkably that nature can perform the function of respiration with only one lung, when the other is diseased and atrophied. The stoppage of the left pulmonary artery, which prevented the circulation of blood through the lung, was probably one of the consequences of the pneumonia. The heart was found somewhat large; the two auricles and the right ventricle were healthy, but the left ventricle was found dilated and hypertrophied, the dilatation being slight, but the hypertrophy considerable; the septum ventriculorum was very much thickened. The aortic semilunar valves were found hardened by a copious deposition of calcareous matter, but there seemed to be sufficient space for the passage of the blood. The aorta was degenerated along its whole course; patches of albuminous matter, in a hardened state, were thickly strewed along its course, and atheromatous deposits were situated below the lining membrane. This degeneration of the arterial coats is by no means rare, and is well known to occur in most old persons. The trachea was healthy, but on cutting into the œsophagus, a large quantity of blood was seen to issue from the wound.

The stomach was now examined. On a superficial view, it appeared large, and distinctly constricted in the middle. On partially opening it, we found it filled with a large quantity of dark, grumous, coagulated blood, mixed up with mucus and half-digested food: the constriction in the middle was of a size sufficient to admit a man's finger, and appeared formed by a constriction of the muscular coat, and a puckering of the mucous lining. The stomach, with a portion of the œsophagus and the pylorus, was now removed, laid completely open, and washed. The mucous membrane was rather soft, and was also red and injected; a layer of jelly-like, bloody, transparent coagulum, was removed from it with some little difficulty. The stomach being the organ whence the fatal hæmorrhage proceeded, it was examined with great care, but no lesion of its texture was in any part observable, nor was any vessel found ruptured, though the cœliac axis and its arteries were found degenerated in the same manner as the aorta. The liver was pale, and presented the appearance of what is called the *nutmeg liver;* and the spleen was small, soft, dark-coloured, and very friable. The cause of death in this case, therefore, was a spontaneous hæmorrhage from the minute vessels of the stomach, in such abundance as to impede the action of the nervous, circulating, and respiratory systems, and causing death by suffocation.

I do not presume to say that the case now related is an extremely rare one, but I do not remember to have seen or read of an instance, in which hæmatemesis was so rapidly fatal, and where there was no rupture of any considerable vessel to account for its occurrence.

PLAN OF RENDERING THE REGISTRATION OF DEATHS EFFICIENT FOR MEDICAL AND STATISTICAL PURPOSES.

To the Editor of the Medical Gazette.

SIR,

THE following rough notes were written on the spur of the moment, for the use of an M.P., when it was announced that the question of registration was coming before the House. Should you consider that the mode of registration I have recommended, and which is found highly serviceable abroad, would contribute to the enlightenment of medical practitioners, as men of science, and to their profit as men of business, you will perhaps have the kindness to publish my observations; they will probably give rise to the suggestion of improvements on the part of your readers.

I do not know the exact nature of the bill now being drawn; but the exclusion of the medical officer will be no less detrimental to the community at large than to the profession.

I remain, sir, in haste,

Your obedient servant,

HENRY BELINAYE.

17, George-Street, Hanover Square,
March 2, 1836.

I consider the registration of deaths as important in three points of view:— 1st, Regarding the insurance of life. 2d, As influencing the improvement of medical science and statistics. 3d, Relating to the criminal laws and police.

The first object mentioned, as contem-

plated in the registration I would recommend, is one that concerns particularly the *English* community, one more superadded to those uses for which it was designed on the continent, and to which it is found so well to respond.

Life insurance is by far the greatest improvement, and the noblest feature of civilization--that by which England surpasses all the olden communities, however great their civilization, as well as all contemporary nations. Setting aside all the commercial facilities it affords, it imparts to fathers of families a peace of mind which passeth all understanding of unmarried men. It improves and exalts the moral feeling of a nation, gives every man a stake in the well-being of the commonweal, and affords a pledge to the governing power of the state. There are no institutions, therefore, that statesmen, legislators, and philanthropists, should so much encourage as life assurances. Those that at present exist cannot meet the wants of society—cannot come within the scope of men of very small revenues, who are the persons to whom it is most important to afford their security. Those offices which demand the least premium enjoy no credit; the others exact premiums twice the amount necessary; and, although there is an equitable division of profits ultimately, the yearly payments are an absolute bar to married men of very limited revenue engaging in these assurances, particularly professional men, as there is no country in the world where appearances and external shew are so necessary to success in life. The proper registry of deaths, as I should wish it to exist, would compass the following, as well as many other objects:—Prevent life-assurance societies from being the losers by suicides, which have even been sometimes committed by persons to provide for their families—by murders, which last may also be effected by poison, and other means easily disguised, to obtain the sum the murdered individual has insured.

But these are the least important objects I contemplate. It would allow of establishing a new computation; a new and correct basis for life societies; enable these societies to reduce their premiums; to insure persons according to their age, and in relation to their being male or female, married or single, pregnant or not, diseased or not diseased, and according to the nature of their disease. That they should insure persons, although diseased, (within a certain morbid range) is of the highest importance; for if they do not, what numbers of persons they must exclude. How few individuals arrive at 25 years of age in our artificial state of society without the lesion or debility of some organ! Health is not a positive, but a comparative state of being: the difference betwixt the sanatory state of one man and another, is one of degree only. It is not rational to suppose that the wonderfully intricate and delicate mechanism of perishable man can be worked even for a few years without some slight impairment. Some person has had a watch for 10 years, without its ever having erred; to-day it stops, and the watch-maker, on inspecting it, finds that it can never go well again. So a man of health proverbially good, dies to-day of apoplexy, or of an accident, and his body being opened, his lungs or his liver are found to have been deeply diseased for many years. By the aforesaid mode of registry, people may assure themselves, according to their station and means of existence, their professions and avocations; and I need not say how absurd it is to make a calculation for assurance establishments based on the mortality of all classes taken together: the lowest classes, who do not insure, affording by far the greatest mortality.

I cannot see why charities, such as savings-banks, should not afford even the lower class means of life assurance. To give an idea of what importance the establishment of life insurance reaching the industrious classes, (of course the lowest it can scarcely touch) I will observe that in Paris an immense proportion of those classes die in the hospital; out of 261,360 persons who died in Paris from the year 1821 to 1830, 213,503, or 83 in 100, died in the hospital, leaving behind not even enough to bury them. Conceive the mental sufferings of those persons previous to death; their own position, and that in which they leave their families; think of the poverty, degradation, and prostitution of their sons, wives, and daughters! The mortality of children under age from this cause is known to be terribly extensive. I must, therefore, say once more, that if

by another mode of registration of deaths one could only compass the insurance of the majority of the persons now excluded, it would be an incalculable advantage to the moral and political structure of society. If one had to give an example of the triumphs of moral improvement in the Christian world, one would show, on the one hand in, the Chinese nation, with its boasted civilization, beginning in the remotest time, the parent ridding himself of the burthen of his children by murder; and on the other hand, the English father curtailing his dearly loved comforts to maintain his children even after his death. This at once gives a measure of the highest altitude of Christian civilization.

As regards the advancement of medical science, the mode in which these registrations have been hitherto managed has not only met these views, but all reasonings established upon them, either by foreign or native writers, have been altogether erroneous from their remarkable deficiencies and inaccuracies. It has been constantly the practice of parish clerks to omit for three or six months, even at the time of the cholera, sending in their returns, and then reporting them as belonging to the hebdomadal bills of mortality just elapsed. There can be nothing more extraordinary than the way in which these returns have been managed, except the still more surprising supineness and indifference of the great medical bodies to the subject, and this amidst the wants and difficulties inextricable which beset all men who wished to guide themselves and medical science, by reference to these reports. The conduct of governments and medical bodies on the Continent being different from ours "toto cœlo" in this respect, and their writers having consequently published on the basis of data thus obtained, works of the highest use and merit, which have been translated into the other contemporary languages, no reasoning is necessary to prove the use of these reports to the medical philosopher. Were they only a subject of an elevated scientific curiosity, they ought to be attended to; since it is known how constantly, one might add how infallibly, researches made for the gratification of the *hobbies* of philosophers have ultimately led to solid and important advantages. But there is no better means (setting aside all other important points of view) of studying and endeavouring to counteract *en grand* the diseases, and alleviating the sufferings, of the lower *strata* of the population, whose mortality is so enormous, when compared to that of the upper classes in England. By these reports, amongst other things, would be known the propriety of certain treatments in vogue, the prevalence of certain complaints, the use of certain prophylactics like vaccination, and the mortality of localities, which may be remedied by drainage; preventing the accumulation of overworked artisans in manufactories, of paupers in narrow streets and small tenements, &c. &c.; of delinquents in prisons and penitentiaries; of the sick in hospitals.

I will add but one observation more: that for twenty-three years that I have had my attention engaged in medical pursuits, there has never been a greater revolution, to my mind, in the constitution of disease; diseases having now changed their characters, and for the most part taken up chronic and insidious forms. As to the upper classes, if to their treatment be applied strictly the medical lore which is acquired in schools, fatal effects must ensue: for instance, that disease formerly so common—inflammation of the lungs, pure pneumonia—which, where it exists, requires abundant vascular depletion, is now comparatively rare in this sphere of life. For three years I have not seen, in London, a case in private practice where repeated copious bleeding had been employed, except where some young or unobservant medical practitioner, having bled largely the patient, and deprived thus the extremities of their irritability, the blood has congested upon the lungs, on the principle of "ubi stimulus, ibi fluxus." Here you must often bleed again, to withdraw the local congestion; and the patient, when he reaches the first step of convalescence, is reduced to the lowest ebb of life.

It is highly probable that the extraordinary diminution of certain diseases, particularly of the contagious, is attributable to their supervention of cholera, through whose instrumentality the depressed and diseased members of the community have been swept away; their abodes and haunts, which afforded a refuge and nidus for contagion, cleared out: contagion, like a thief, seldom har-

bouring but under rags, in filth and darkness, and almost always attacking first the weak and unarmed—the unprotected members of the community. This, however, in absence of good statistical reports, is mere probability, but if it were proved, would show once more that the exercise of Christian charity, and the application of "mèsures gouvernementales" to the hygiene of the lower classes, are the only means of security for all classes of society.

As regards criminal law and police, I must observe, that the mode of death, as well as the disease which caused it, cannot be known by such means as are at present adopted. The least important result of this is, that suicides are not brought to light. I have shown, under the previous point of view, the consequence. But I have not only heard (I could express myself more positively) of several instances of concealment of suicides, but I have also heard of cases of death, under the most suspicious circumstances, passed over without notice; one particularly of the most sinister complexion. Here the medical man could not act without taking upon himself the character of an informer, against the advice of his brethren and friends, and where, if he had put himself forward without the case proving one of murder or homicide, and his name had got into the papers, he would have been scouted by every body. Nor can it be expected that medical men will ever volunteer a demand for investigation under doubtful circumstances, as long as the law does not make it an unavoidable duty for them to do so.

Instead of the "*Officier Civil*," and the "*Médecin Vérificateur*," as on the continent, establishing the fact of the death of any member of the community, two old crones generally perform that office in England in each parish,—a trifling pecuniary compliment to them from the family of the deceased, and that of a glass of spirits, being the only result of their inspection. That these veteran ladies should be invested with an office of so much importance and gravity, proves that many of their *semblables* occupy higher offices. It shows an absence and want of feeling of propriety and of interest in the well-being of society,—an ignorance or a blindness inexcusable and inexplicable on the part of the governing power. There will always be an objection to legislation on this subject, on account of the natural repugnance of Englishmen to the invasion of their privacy by persons invested with any thing like an approach to an *inquisitorial* power. But setting aside the great advantages—nay, the necessity—of such an enactment as we wish to enforce, would it not be infinitely preferable that the office of visiting the houses of any deceased member of the community should be in the hands of educated and judicial persons, than in those of the persons previously described as at present so singularly trusted to? Is it not absurd that a coroner's inquest should be held upon persons who die of the most palpable causes, whilst doubtful cases escape investigation?

The following are an enactment and a mode of registration of death, having for their object—1st, the detection of suicide, of murder (particularly by poison and suffocation), infanticide, death after artificial abortion, &c.; 2dly, the improvement of the system of life assurance, and its protection; 3dly, the advancement of medical science, police, and statistics, and public health. This plan would likewise prevent persons being buried alive, as also the bodies of friendless and isolated paupers remaining uninterred until they have contaminated their neighbours.

> "No person to be buried without the inspecting medical officer (to be named for each district) being apprised of the circumstance, and having given permission,—which permission he is only to deliver after having called at the house of the deceased, with the clerk of the parish, visited the body, and in the presence of two witnesses, if possible, acquainted with the defunct, filled up and signed the following paper; which is likewise to be drawn up according to the same form by the medical man (if any) who has attended the deceased in the last illness. The medical superintendent to recommend a post-mortem examination of the body to be made by the medical attendants of the family. Information of death having occurred to be sent by the head of the family, within twenty-four hours after its occurrence, to the inspecting officer or clerk of the parish."

Name of the Deceased.	Date and Place of Birth, (if known.)	Sex.	Married or Single.	Year, Day, and Hour of Decease	Residence, in what District, Street and Number, and in what Story.	Nature and Duration of last Illness	Predisposing Causes.	Profession, Avocation, Habits.	OBSERVATIONS, in which the position and state of the Corpse, when discovered, may be recorded; besides the suggestions of Inspector.

TIC DOULOUREUX *A SYMPTOMATIC* DISEASE.

To the Editor of the Medical Gazette.

SIR,

IN your number for the 27th of February, there is a detailed report of a clinical lecture, by Sir Charles Bell, on tic douloureux, in which he exemplifies the useful agency of a medicine actively purgative in several cases, states his opinion that *tic* is a sympathetic pain, depending on distant and *visceral* irritation; and he claims a priority of right to this view of the disease, by the following expressions:—"It has appeared to me surprising that authors have omitted to found on the anatomy of the nerves, which leads so directly to the satisfactory explanation of the symptoms of this disease." p. 874.

Now I am far from wishing to diminish the claims of Sir Charles Bell to the esteem of the profession on account of his incessant and successful labours for the advancement of science; yet it may not be improper, nor, I trust, unreasonable in me to observe, that *the same view of the cause of tic douloureux has been placed on record sixteen years ago*, in a thesis printed at Edinburgh in 1820, by which my degree of M.D. was obtained at that University.

The thesis was not that of a young gentleman who must write about something at the close of his attendance on the classes, and is often indifferent on the choice of a subject; but it was a *practical essay*, the result of experience, accumulated in the treatment of a disease of rather unfrequent occurrence, during my practice in London as a surgeon-apothecary, or general practitioner, for twenty years; and contained, as I believe, a novel view of the causes of that complaint, by asserting,

Firstly, that I considered the pain of the face to be *symptomatic*—viz. that the morbid sensibility of the nerve was excited by irritation at a distance from the painful part.

Secondly, that the seat of irritation was in the *abdominal viscera*. Thirdly, by explaining the association between the nerves of the face and those of the viscera, and showing the direct line of communication, assigning as a reason why the division of the painful nerves failed in affording relief, *that the exciting cause was out of the reach of the knife*, as expressed in the following words:—

"Unde, procuddubio, intelligendum est, cur sensatio, pruritus, nimirum, dolor, irritatiove cujusvis generis, prout causa excitans sit, a visceribus ad faciem, cursu directo, volat; necnon docet, cur sectio nervorum minimè prodest ægrotantibus, si neuralgia facialis spasmodica symptomatica sit, junctio, enim, cum sympathetico magno profundior est, quam scalpellus chirurgi peritissimi pertingere queat."

The plan of treatment recommended was founded on two points. 1st. A perfect clearance of the alimentary canal. 2d. The employment of medicinal agents *afterwards*, which should bring the alimentary surface and the alvine secretions into a more healthy condition.

The tendency of the painful affection to periodical returns, without any cause sufficiently obvious, had not escaped my observation; and as cinchona, in powerful doses, had succeeded best in several cases with me, I preferred it, to fulfil

the *second* indication, but I have never employed it without discrimination, nor relied on it as *an antidote*, as may be collected from the following sentence, page 40:—"Actio curativa cinchonæ, febribus et hæmicraniis intermittentibus, ubique terrarum agnoscitur, et hujus effectus salutifer ægrotis, *neuralgiâ faciali spasmodicâ vexatis*, haud minus manifestabitur, si detur conditionibus *idoneis*, jam memoratis: *his, autem, prætermissis, aut malè ordinatis, minimè adjuvat.*"

In offering these remarks, I beg to be distinctly understood that, at present, I merely wish to notice the fact of identity of view with Sir Charles Bell; that I have, sixteen years ago, described tic douloureux as a *symptomatic* disease, caused by irritation in the abdomen, and endeavoured to point out the line of communication between the splanchnic nerves and those of the face, particularly the three branches of the fifth pair, and to vindicate my fair claim to priority. It is my intention to publish a treatise on this extraordinary and painful affection, taking for its basis the Thesis alluded to, and annexing the result of subsequent experience.

A copy of the Thesis was sent, in the autumn of 1820, to Sir Henry Halford, Bart., President of the Royal College of Physicians, and to each of the Censors, viz. Drs. Powell, Cooke, Macmichael, and P. M. Latham, and to many personal friends in the profession. A number of these were interleaved, bound in morocco and lettered, to form library books of reference, and not likely to be put aside like a loose pamphlet. A copy had been given to each of the medical students (more than 120) who graduated at Edinburgh in that year; it had been, therefore, widely diffused, and being an *original paper*, which had never been through the hands of any private tutor or grinder, the information conveyed was likely to be of more interest. It would seem, however, that Sir Charles Bell was not aware, or had forgotten, in 1836, that any writer on *tic douloureux* had noticed the association, and reasoned on the effects of a communication, between the nerves of the abdominal viscera and those of the face.

I have the honour to be, sir,
Your obedient servant,
ROBERT MASTERS KERRISON.

12, New Burlington-street,
March 1, 1836.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue à abréger."—D'ALEMBERT.

Flora Metropolitana; or, Botanical Rambles within Thirty Miles of London. With a List of the Land and Fresh-water Shells of the Environs of London. By DANIEL COOPER. 1 vol. 12mo. London, 1836.

WE are induced to bestow a passing notice on this little work, both because it is well calculated to supply a remarkable deficiency in the catalogue of books proper for the out-of-door studies of the metropolitan botanist, and because we regard with pleasure the appearance of a book which may be considered as the first fruits of the introduction of botanical science into the curriculum of medical education in London.

Being in some sort a catalogue, we cannot devote much space to it; but yet would wish to notice with commendation one feature in its construction. The environs of the metropolis are in it divided into districts, and an arranged list of the plants found in each of these, furnished for the guidance of those who may visit it; and under each locality the geological character of the spot is stated. We are glad, therefore, to see the recommendation of Dr. Daubeny, which he made a few years ago, in his "Specimen of a Proposed Index of the Oxfordshire Flora," adopted in the present case. Some have doubted, or denied, the connexion of the characters of the soil with the vegetation which clothes it; and although this is not the place to discuss the question of the correctness or unsoundness of the doctrine, we may be allowed to state that it is believed in, and advocated by the illustrious Haustmann, and made the subject of a treatise by Crome, of Hanover, from whose work we quote the following striking fact:—"When the proportion of clay in the soil of any place does not exceed 60 per cent., the *Trifolium arvense* may be found; but if the clay reach 80 per cent., you will search for it in vain."

We strongly recommend Mr. Cooper's book to the botanical students of the metropolis.

MEDICAL GAZETTE.

Saturday, March 12, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

CICERO.

MEDICAL CORONERS.

SOME persons are remarkable for the unlucky talent they have of always proving too much. When they have a point to carry, they overlay it with what they conceive to be argument, but which turns out to be only so much rubbish, wearying and disgusting those who give it any attention.

We often meet with examples of this sort; and by the way, the ingenious advocates of the skull-doctrines, calling themselves *phrenologists*, may be instanced as furnishing perhaps the purest specimen of the unhappy zeal we allude to: but we have been specially struck with the peculiarity in question, upon reading a pamphlet recently published, by Mr. Rogerson, of Liverpool*. In a brochure of about fifteen or twenty pages, he has so superabundantly proved the fitness of medical practitioners for the office of Coroner throughout the kingdom, that, if we had our doubts about it before, we are now strongly persuaded that the position is a false one. This we will show more fully presently.

Mr. Rogerson begins his address to the "Reform Corporations," by tendering to them, in the shape of copious extracts from the statute of Edward I., information respecting the nature of the Coroner's court. "This court," he sums up, "is, then, one of inquiry, and all its inquiries are for the purpose of deciding questions of *facts, not of law*." We shall here only stay for a moment to remind the author that the Coroner, in his capacity of judge, may have some *little* difficulties of law to disturb him occasionally—for instance, certain questions, which it is for him to decide, with respect to what is, and what is not, to be admitted as *evidence*; and the *law of evidence*, Mr. Rogerson must know, is not one of the simplest in our English jurisprudence.

The following passage shows the author's notions of what duties devolve on the Coroner, in his judicial capacity.

"A view of the external surface of the body merely will lead a practised eye to trace the hand of violence, or suspect some "foul deed." A more minute examination will explain if the death has been caused by the infliction of a wound, blow, or other injuries, either *from number, extent* (?), or on a part of the body essential to life. The fatal result will be equally exposed, if it has arisen from the natural operations of disease, or if it has been hastened by any rude act. This inquiry, primary and important as it really is, and as our laws have wisely made it, can be executed only by those who *intimately understand the laws of the animal economy*, and are profoundly acquainted with the numerous organs and functions of the human constitution,—with the different states in which they appear during health and disease, during life and death. Hitherto the inquiry is strictly *medical*, and the requisite information is afforded by anatomy, pathology, and surgery. By negligence, self-design, or guilt, destruction of life is frequently effected by drugs and poisons; of which the Coroner's inquiry can only be efficiently and justly prosecuted by discovering *the nature and properties of destructive substances, and their effects on living bodies*. This part of the inquiry is also strictly *medical*, and can only be attained by the aid of materia medica, chemistry, and toxicology. The various branches of a difficult, learned, and deeply interesting science, requiring years of labour and study fully to comprehend, are embraced in the elucidation of these questions, and are *absolutely necessary to forward the ends of justice*."

Mr. Rogerson evidently has a high opinion of the attainments of those medical men who are willing to become

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* A Letter to the Reform Corporations, on the Necessity of Electing Medical Coroners. By George Rogerson, Surgeon, of Liverpool.

candidates for coronerships. But does he really mean to say that the medical *practitioner*, in the actual state of the profession in this country, is possessed of all this profound knowledge and extraordinary skill? or that, being so, the *judge* is himself to make the investigations which are at present usually instituted by the medical witnesses?

"The science of medicine," says Mr. R., "the various branches of which must be known to institute an efficient examination, has been cultivated and pursued exclusively by a distinct class of society, who alone are able to investigate and discover the *causes of death*, and to duly estimate all evidence bearing on them."

Surely the author here exaggerates not a little. That part of medical education, enabling the *practitioner* to make efficient examinations into the causes of death, is only of comparatively recent introduction into the schools: it is there even still little attended to, and but partially encouraged: and it is a special department of medical science—an application of all the branches—not attainable in ordinary practice, though, upon Mr. Rogerson's own showing, indispensable for the medical coroner. Either, then, the ordinary medical practitioner, of several years' standing, is ineligible to the coronership, or the only competent medical candidates are those who have lately come from the schools. This the author very clearly *proves*—though he certainly does not intend it.

We rather doubt that the question is fairly stated between the lawyer-coroner and the medical. "Is a lawyer," says Mr. R., "whose business *cramps* his mind, in the study and *perversion* of precedents, acts of parliament, judges' versions of them, and in *mechanically* copying forms of law, best qualified to preside in such a court? Decidedly not, will be the immediate answer." We admit the propriety of the answer, but deny that of the question: for it is what the lawyers call a *leading* question, and we, an uncandid one. An ignorant, prejudiced, dishonest coroner, every body must allow to be a nuisance which ought not to be tolerated for an instant. But must every person installed in the coroner's office necessarily be a man of this stamp, because he is not proficient in medicine?

Few readers, we imagine, will be satisfied with the authorities adduced by Mr. R. in support of his argument. They are, indeed, a remarkable trio—the late Mr. Hunt (of "matchless" notoriety), Mr. Joseph Hume, and Mr. T. Wakley; the first telling a very apocryphal story of an affair that happened while he was in Ilchester jail, and the last, quoted as a most *disinterested* witness, being a (rejected) candidate for a coronership, "on the expressed principle of that office being essentially medical!" Is it possible that the author could bring forward no *respectable* vouchers?

But to come to the gist of the matter, Mr. Rogerson says he has *proved* (no doubt to his own satisfaction) that the coroner ought to be a medical man, in order to appreciate the professional evidence given at inquests, and to explain it clearly and justly to the jury. But, with our author's leave, he might have gone a little further: on the same principle, and by virtue of several other *proofs* of the same kind stated *passim* in the pamphlet, he could readily shew that *the jury* ought also to be medical; nay, that judges and juries of all sorts, who hear and determine cases involving medical evidence, ought to be *bonâ fide* medical men: commissioners of lunacy decidedly should be so, and even the Lord Chancellor himself! Now this is what we call proving *too much*. We must, however, have done with Mr. Rogerson's production for the present.

The question may be put to us, whether we think coroners ought to be of

the medical profession? If by the profession be meant actual practitioners, we say decidedly not. But we will explain. The office of coroner, in order to be discharged in a manner suited to its great importance, ought to be filled by a man of sound judgment, strict impartiality, and very extensive acquirements. Those acquirements, should, unquestionably, embrace a large portion of medical and medico-legal knowledge; so large, indeed, as can scarcely be attained but through the medium of a regular medical education.

This, however, does not necessarily imply that he should be a *practitioner;* on the contrary, we hold that a person standing in such a relation to the public ought *not* to be entrusted with the duties of the office in question. It can be readily understood how the functions of the two avocations are incompatible. In most cases of emergency, where personal injuries likely to prove fatal have been received, medical men are the first called in, and become referees and witnesses of the circumstances that come under their observation. Are these the persons, thus mixed up in the transaction, who are afterwards to sit as judges concerning the matters of fact? Are they, who are themselves the chief witnesses, to sit in a judicial capacity in gathering the evidence, and then to direct the verdict of the jury?

It happens that this is no ideal case. Mark what has occurred within the last ten days. At Winchester, on Friday, the 4th instant, a criminal trial took place on a capital charge of child-murder. We shall not present the details here, as probably most of our readers have seen them reported in the newspapers; suffice it, that they were of a very remarkable and deeply interesting nature. But the fact to which we wish to call attention is this:—The surgeon who was called in, in the first instance, when the body of the infant supposed to be murdered was discovered, and who examined medico-legally both the accused mother and the dead child, *sat afterwards as coroner* in the case, and directed a verdict of WILFUL MURDER against the prisoner. Nor was this all. At the trial, which came on at the Assizes on the day above mentioned, who should come forward as principal witness for the prosecution, but the very identical surgeon, medical jurist, and medical coroner, who had already given his professional, and pronounced his *judicial*, opinion on the question!

Such an example as this,—and no doubt the same concurrence of accidents is by no means rare where there are medical coroners,—must prove, better than all Mr. Rogerson's arguments put together, that there is at least a *chance* of similar gross anomalies occurring wherever the same individual acts at once as a practitioner and a judicial functionary. We wish to be distinctly understood: our objection is not to the fact of coroners being men of medical education, but to their being simultaneously medical practitioners; for we hold that not only is medical practice *per se* incapable of qualifying a man efficiently to discharge the coroner's duties, but that it directly disqualifies, inasmuch as it is calculated both to distract and embroil him.

We do not think the law ever contemplated that the coroner and his jury should necessarily be very accomplished persons, or profoundly versed in the mysteries of science. It seems rather as if this court was instituted for plain inquiry, preparatory to further and more searching investigations; in which case, as it appears to us, the judge of the said court need not be a *very* learned or scientific personage, but rather one suited to act as a kind of middle man, or interpreter, between the homely understandings of the jury and the occasionally scientific testimony given by

professional witnesses. The technical explanations of the latter are to be conveyed to the dull ear of the uninitiated, through the medium of a person of good plain sense and general acquirements. This is what we conceive the legislature intended from the first, respecting the office of coroner. No doubt the progress of knowledge renders it desirable that the functionary should now be more largely gifted; and we have admitted the full propriety of this, in saying that he ought to be endowed with much medical and medico-legal knowledge.

In short, our opinion on the subject may be briefly summed up in this way:—Medical knowledge, in the present state of things, is of much importance to the coroner. But let no mere practitioner, therefore, fancy himself fitted to be a candidate; for in doing so, he will commit a great mistake. Ordinary medical practice is not sufficient to render a man competent to the investigations required of a medical witness. What, for instance, has the common routine of the physician or surgeon to do with the tests of live or still-births, in questions of infanticide? What has it to do with detecting the proofs of poison? It is plain, consequently, that even within the province of medical science, the practitioner has considerable acquirements to add. Nor let it be supposed, as is too often the case, that the knowledge of law required of the coroner is slight: we have already alluded to the law of evidence with which he ought to be familiar, and which is admitted on all hands to be a subject of no small intricacy; so it is with various other legal points belonging to his essential qualifications. The coroner, besides, it must not be forgotten, has several duties to perform in addition to holding inquests on dead bodies—duties, unquestionably, rather fitted for the lawyer than the medical man. We must, therefore, in conclusion, say, that medical men are certainly adapted to make excellent coroners—perhaps the best—provided, 1st, that they superadd to their ordinary practical attainments a competent knowledge of legal medicine; 2dly, that they make themselves perfectly qualified for the discharge of *all* the duties of the office, by being duly versed in the requisite law forms (for in this respect they will be closely and jealously watched); and 3dly, that they absolutely and unconditionally resign the practice of their profession. If they comply not with these provisions, we are persuaded that their meddling with the coronership at all will only be likely to bring disgrace on themselves and detriment to the community.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, March 8, 1836.

H. Earle, Esq., F.R.S., President, in the Chair.

The Secretary, Mr. Partridge, commenced the business of the evening by reading over the minutes of the last meeting, the general annual one, held on the 1st March. We think the following facts, mentioned in the statement, worthy of being extensively known:—Unusual and large as has been the Society's expenditure for the past year (owing to the removal from Lincoln's-inn-fields, the new charter, the fittings-up of library, &c. in Berners-street) the resources of the Society have proved abundantly sufficient to meet its liabilities; so that at the present moment there is not a penny due. The admissions of new Fellows during the year have amounted to the unprecedented number of 35,—the largest number in any preceding year not having exceeded 10. It was announced that the President (Mr. Earle), and the other officers of the society, were re-elected.

The President rose to return thanks for the honour done him, and promised his utmost exertions to promote the interests of the society. He congratulated the

Fellows on the actual prosperity and the gratifying prospects of the institution.

Dr. CLENDINNING then resumed the reading of Dr. Kingston's paper,—introducing it by an abstract of that part which he had read at the last evening meeting. He also read a note received from the author, to announce his regret at being unable, through illness, to be present at the discussion this evening.

We give the following as an abstract of the whole:—

The paper was entitled, *Remarks on Two Forms of Atrophy of the Heart's Valves, which interfere with their function: founded on Cases.* By P. N. Kingston, M.D., Physician to the St. George's and St. James's Dispensary.

The first of these forms of atrophy, which has been hitherto altogether overlooked by pathologists, was defined "a simple shortening of the heart's mitral or tricuspid valve, without any diminution of its natural thinness, pliancy, and transparency; the orifice to which it belongs having at least the ordinary calibre." It has been observed by the author in numerous cases, in eight of which it had proceeded to a considerable extent, the length of the laminæ being in some cases only a quarter of an inch, and in one case the posterior lamina of the mitral valve being only the eighth of an inch at its greatest length.

The second of these (to which the aortic and pulmonary, as well as the auriculo-ventricular valves, are liable) is nearly allied to the preceding in nature and effects. "When so altered, the continuity of the valve is interrupted by apertures, sometimes of large size, and sometimes so numerous as to reduce the structure to a mere net work, while the remainder of the valve is in a state of attenuation, which is here and there often extreme, especially towards the edges of the apertures." In five of the cases a considerable portion of the valve had been thus affected. This cribriform condition of the valve, as connected with attenuation, Dr. Kingston had long believed to be also unnoticed by any author; but some time after the composition of his paper, he found a short passage in Laennec, which appears to refer to it.

Both the lesions, though not very rare, are of a nature easily to elude observation, until the attention has once been directed to them.

A valve, which has become shortened or cribriform, is of course incompetent to close completely the orifice to which it belongs; and hence permits that regurgitation which it was placed to prevent. The regurgitation, when dependent on shortening, will be liable to increase from bodily exertion; for it was shown by Mr. Hunter, that the heart at its diastole dilates much more when the body is in activity than when it is at rest; and it is evident that when an orifice has become unusually dilated at the diastole, its valves, if shortened, will at a succeeding systole be still less able to meet than they were before. Mental emotions, and other circumstances by which the heart's action is stimulated, will promote the operation of this lesion in the same way.

From this disablement of the valve arises a strong tendency to dilatation or hypertrophy of those parts of the heart posterior to the valve affected; to palpitation, venous congestions, anasarca, and effusion into the serous cavities; and where the mitral valve is defective, to dyspnœa and cough, to pulmonary congestion and inflammation, and to some of those symptoms which are apt to arise from deficient and irregular supply of arterial blood to the head and remote parts of the body.

These two species of atrophy sometimes co-exist, or are combined with other valvular lesions; and then, though each of the defects be slight, their influence thus united may be considerable.

A morbid "bruit" may, in some cases at least, be perceived by the stethoscope. Where the tricuspid valve is affected, there is distension, and sometimes pulsation, of the external jugular veins. Where the mitral valve is affected, the pulse is small and weak, in comparison with the heart's impulse; and it is apt to be irregular and unequal.

By a reference to these symptoms, and to the circumstances out of which the complaint arose, it may generally be ascertained whether or not there is valvular disease. The discrimination of these from the other valvular lesions is a point of great nicety, on which the author is not as yet fully prepared to speak.

These defects may now and then be congenital; but in all the present cases they could be proved to have occurred long subsequently to birth: a conclusion which coincides with Meckel's opinion respecting similar defects of the valves of the veins. They had evidently been produced by those species of absorption unattended with suppuration.

In the greater number of cases the valve affected had, from hypertrophy of the corresponding ventricle, over-exertion, &c., been subjected to an increased force of the blood's impulse; which, when from previous morbid affection the nutritive powers of the valves are weak, may exercise on them the same influence which unnatural pressure has been observed to exercise on all other organs and tissues of the body. To this combination of undue pressure and debility, the valvular atrophy was in these

cases referred. In the other cases there was reason to believe that the heart had been subject to gouty or rheumatic affection, which may have so impaired the nutrition of the valves, as to render them liable to be absorbed even under the pressure to which they are naturally subjected; in the same way that the cartilages of gouty and rheumatic joints are absorbed from the friction occasioned by the ordinary exercise of the limbs.

The author concluded by pointing out the errors which may have arisen from an oversight of these lesions; the practical advantage of detecting them during life, (even so far as to perceive that there is some valvular defect obstructing the circulation); the means by which the frequency and extent of the lesions may be diminished; and the circumstances under which it may be hoped either that the valvular structure may be restored to its original dimensions, or that the corresponding orifice and cavities may gradually diminish in calibre, so as to become adapted to the altered size of the valve.

There was no regular discussion of the topics of the paper; but some conversation took place on various anomalous conditions of the heart, principally functional.

Dr. Bostock exhibited to the Society some specimens of very remarkable urine, which he had recently procured from a man labouring under anasarca. The albumen abounded in such quantity as to render the fluid perfectly opaque. Some other urine voided by the same patient alternately with the albuminous kind was quite limpid, or almost wholly aqueous. It is understood that Dr. B. will draw up an account of this case.

Mr. Stafford, with the permission of the Society, introduced the man whose case (dislocation and fracture of the lower dorsal vertebræ) he described in the paper lately read, and of which we gave an ample abstract in our last number but one. With the exception of some stiffness in the back, and irregularity in the appearance of the processes of the vertebræ about the part injured, the recovery seemed to be complete. It will be recollected that the man fell, or jumped, backwards from the window of a first floor, cleared the area railings, and came upon his buttocks.

VARIETIES OF EAST INDIAN OPIUM.

It appears that the English market presents at least two varieties of opium brought from Turkey.

1. The first is what is proper Turkey opium, and the specimens which are regarded as best, instead of being compact, solid, and of dark brown colour, occur in masses about the size of the hand, soft, compressible, and elastic on the surface; not frangible, but sectile, and presenting on internal section a viscid compressible mass, pretty firm and dry at the external crust, though still receiving easily the impression of the nail, and soft, moist, and approaching to semifluid in the centre. The colour varies, being in the external drier parts between deep reddish brown and liver brown, and in the moister and softer portions lighter, between brownish red and chestnut brown, or something of the colour of the mineral grenatite, or between that and cinnamon stone. The smell is not unpleasant, and like that of the bean-husk. In a specimen which we lately examined, the exterior presented the seeds of a species of *Rumex*. Does this indicate that it belongs to the Smyrna variety, to be mentioned below?

It is remarkable, that notwithstanding the statements above noticed, in all the books and dispensatories on the characters of good Turkey opium, experienced druggists invariably prefer Turkey opium, which is soft, compressible, and moist, as now described. Its internal section is much intermixed with leaves, and it is even possible to recognize the petals of the white poppy. This contains about five per cent., sometimes six, of morphia. But the strength and purity of the very best Turkey opium vary so much, that for this no rule can be laid down.

2. The second variety of Turkey opium is that denominated Constantinople opium. This occurs in much smaller masses, being sometimes not larger than the hand of an infant of one year, and weighing from fourteen drachms or two ounces to three ounces and a half, or four; rarely more than that. The exterior of these masses, which are generally flattened and irregularly polyedral, is dry, hard, and of a broccoli brown colour, less perfectly covered with leaves, and showing smaller fragments of vegetable matter than the common Turkey opium. The masses externally are scarcely compressible, but they are not frangible but sectile; and the section is of a deep reddish, sometimes of chesnut brown colour, more uniform than that of the common Turkey opium, drier and firmer, and compressible, and receiving impressions only towards the centre. Some recent portions may be compressed externally, but they generally resume their figure. This opium is always procured from Constantinople, is believed to be reared with peculiar care in the country round the city, chiefly for the use of the

Turks themselves, is richer in morphia than the other species, and consequently brings in the market a higher price. It is occasionally impossible to procure it at any price.

3. It further appears, that in France is another variety of Turkey opium, known by the name of *Smyrna opium*, which is brought direct from that port, it is said, to Marseilles. This is in larger, rounder, and more irregular masses than the Constantinople opium; it has a black colour, especially internally, a stronger and ranker odour, and the cakes are covered with the seeds of *Rumex*, which M. Merat ascertained to be those of the *Rumex Patientia*. This opium is stronger than the common Turkey reddish opium, dissolves readily in water, and is used chiefly to manufacture morphia, which is said in it to be abundant.

The Egyptian opium occurs in small, flat, irregular masses, sometimes less than those of the Constantinople variety. It is always extremely hard, compact, either difficultly compressible or quite incompressible; and though it may be cut, it is rather frangible than sectile. Its fracture is conchoidal and resinous, its aspect shining, and its appearance uniform. It is equally hard at the circumference and centre, but receives the impression of the nail, leaving a lighter streak. It is remarkable, that though this variety agrees with the characters ascribed to the best Turkey opium, and Egyptian opium has been celebrated as the best in the European market by all writers, from Alston, Lewis, and Pomet, down to Dr. Duncan and Dr. A. T. Thomson, yet this is regarded by experienced druggists at the present time as much inferior to Turkey opium, and does not yield more than from 2 to 3 per cent. of morphia. It contains much caoutchouc and gluten.

It has been repeatedly stated by all travellers, from Chardin and Tavernier downwards, that the opium reared in Persia was of the best quality, and much superior even to Turkey opium; and while it is said that the genuine medicinal poppy is a native of Persia, and minute accounts of the effects of collecting opium on the persons engaged in it in that country have been given, it has been also said that all the best opium was brought from Persia. This may have been true about a century or 150 years ago. But at present we are assured by all experienced druggists, that either no opium is brought from that country, or what is brought is of inferior quality, or, in short, is not genuine Persian opium.

It appears, therefore, that the best kinds of opium in the English market are the two varieties of Turkey opium. But even these are so variable in qualities, that the most practised druggists assert that it is impossible to predict, from the external characters and appearance, the probable purity or productiveness of the article as a medicinal agent. It has been commonly supposed that the strength of Turkey opium is about double of that reputed to come from the East Indies; and it is stated by Dr. Anthony Todd Thomson to contain three times the quantity of morphia afforded by the latter.—*Dr. Smyttan, in Transactions of Medical Society, Calcutta.*

ON THE

FORMATION OF ÆTHER.

By Professor Mitscherlich.

The decomposition of alcohol into æther and water is not interesting merely by the production of æther, but is especially so as an example of a particular kind of decomposition, which cannot be so well followed with any other substance, and which is manifested in the formation of some important products; for example, in that of alcohol itself. M. Mitscherlich has endeavoured to elucidate the phenomena of this decomposition by the following experiments:—Take a mixture of 100 parts of sulphuric acid, 20 of water, and 50 of anhydrous alcohol, and heat it gradually until its boiling-point becomes 284° Fahrenheit. Alcohol is then allowed to fall gradually into the vessel which contains the mixture, and the current is to be so regulated that the heat of the mixture remains constantly at 284°. If, for example, the operation be conducted with a mixture of six ounces of sulphuric acid, one ounce and one fifth of water, and three of alcohol, and if the density of each two ounces of product as it is obtained be taken, it will be observed that this density passes gradually from 0·780 to 0·788 and 0·798, and afterwards remains constantly at the last-mentioned density, which is exactly that of the alcohol employed. If the operation be properly conducted, an unlimited quantity of alcohol may be converted into æther, provided that the sulphuric acid does not change. The distilled liquor is formed of two distinct fluids; the upper one is æther, containing a little water and alcohol; the lower one is water, with a little alcohol and æther. Its weight is nearly equal to that of the alcohol employed, and it is composed of

Æther	65
Alcohol	18
Water	17—100

If into six ounces of concentrated sulphuric acid six ounces of pure alcohol are suffered to flow gradually, a product of constant density is not obtained until the sulphuric acid has taken its proportion of water. Take, on the contrary, three ounces of sulphuric acid and two ounces of water, and let alcohol be added, drop by drop; the first two ounces distilled are merely spirit, if wine of specific gravity 0·926, containing scarcely a trace of æther. The density decreases until the quantity of water of the sulphuric acid is reduced to its proportion, and the product of the distillation has acquired the density of the alcohol.

If concentrated sulphuric acid be added to anhydrous alcohol in excess, pure alcohol distils at first; but when the temperature reaches nearly 260°, the first traces of æther begin to appear; the production of æther is at its maximum between 284° and 302°.

It results, from the preceding observations, that alcohol, when in contact with sulphuric acid, is converted into æther and water at a temperature of about 284°. A great number of analogous decompositions and combinations are known, which may be attributed entirely to the influence of the contact of bodies. The most remarkable example of this kind is that of the conversion of oxygenated water into water and oxygen, by the slightest trace of the peroxide of manganese and some other substances. The decomposition of sugar into alcohol and carbonic acid, the oxidizement of alcohol when it is changed into vinegar, are phenomena of the same kind; and so also is the conversion of starch and sugar by means of sulphuric acid. M. Mitscherlich, observing that in the preparation of carburetted hydrogen by means of sulphuric acid and alcohol, water is formed at the same time, attributes this decomposition of alcohol to the influence of mere contact, and not to the affinity of sulphuric acid for water.—*Journal de Phamacie, Juin* 1835.—*Phil. Mag.*

REPULSIVE POWER OF HEAT.

In the *Phil. Magazine* for the present month (March), Mr. H. F. Talbot, F.R.S. gives a short paper on this subject. The following experiments are worth extracting:—

Experiment 1.—*On the Vaporization of Sulphur.*

When a minute portion of sulphur is warmed between two plates of glass, it sublimes, and forms gray nebulous patches, which are very curious microscopic objects. Each cluster consists of thousands of transparent globules, exactly imitating in miniature the nebulæ which we see figured in treatises on astronomy. By observing those particles which are larger than the others, we find their figure not to be spherical, but plano-convex, with the flat side to the glass. Being very transparent, each of them acts the part of a little lens, and forms in its focus the image of a distant light, which can be perceived even in the smaller globules until it vanishes for minuteness. If they are examined again after a certain number of hours, the smaller globules are generally found to retain their transparency, while the larger ones are become opaque, in consequence of some internal change in the arrangement of their molecules. I find that Mitscherlich and others have noticed this property in sulphur of undergoing a spontaneous change. There is a circumstance attending this experiment which deserves particular attention. Although the sulphur has been sublimed by heating it over a lamp between two plates of glass almost in contact with one another, yet the globules are found adhering to the upper glass only; and as their number amounts generally to many thousands, it is evident that the preference which they thus exhibit to the upper glass must have some strong determining cause.

The reason of it is, no doubt, that the upper glass is a little cooler than the lower one; and by this means we see that the vapour of sulphur is very powerfully repelled by heated glass. The plano-convex form of the particles is owing to the force with which they endeavour to recede from the lower glass, and their consequent pressure against the surface of the upper one. I think this experiment is a satisfactory argument in favour of the repulsive power of heat, and I believe it has not been hitherto described.

Experiment 2.—*On the Vaporization of Arsenic.*

When a particle of arsenic is sublimed between two plates of glass, it forms nebulous patches, considerably resembling those formed by sulphur in the preceding experiment. But the microscope detects a great difference. Instead of a globular or semiglobular form, the particles of arsenic are crystallized. The minuteness of some of the crystals almost exceeds calculation. I would suggest the employment of this method to detect the presence of arsenic in minute quantities of matter. The difficulty of demonstrating its presence with sufficient certainty is shown by the number of chemical essays that have been written on the subject, while a particle of the size of a pin's head is amply sufficient to display this microscopic crystallization; and the form of the crystals

being distinct and definite, the observer can soon make himself acquainted with their figure, so as to run little risk of mistaking any other substance for them.

ON THE USE OF SETONS IN CHRONIC DISEASES OF THE CHEST.

M. Rostan recently directed the attention of his pupils to the case of a patient who had been admitted three years previously into the Hôtel Dieu. He was stated to have been affected with phthisis pulmonalis; and, as the case was deemed incurable, M. Rostan had recourse, as a last resort, to the introduction of a seton over the chest. Under the action of this remedy, the patient has now survived three years, and though not cured he has gained flesh, and is in other respects in a condition "*assez satisfaisant.*" M. Rostan proceeded to state, that he had been consulted by a physician, aged 35, (whose name and address he mentioned), who had suffered from cough for a month; his general appearance, however, was satisfactory, and no actual disease of any organ was made out; M. Rostan, therefore, only prescribed some simple remedies, and ventured to encourage him in believing that he would soon be well: some time afterwards the gentleman again returned, in a state of considerable anxiety; but still no very careful examination seems to have been made, and no active means to have been adopted. However, the patient returned yet a third time, and his aspect was now very sensibly changed,—he was now subjected to an attentive examination, and no doubt remained as to the existence of disease in the lungs. M. Rostan then recommended the introduction of a seton into the parietes of the chest. From this time the symptoms abated, and he improved so much as to be able to resume the practice of his profession, which he still continues.

Several other analogous instances were related by M. Rostan, which tend to confirm his opinion of the efficacy of the seton in chronic affections of the chest. The following case we give as rather a remarkable illustration:—

Madame —— was affected with disease of the lungs, which had proceeded so far that caverns were already formed in them. Being called into consultation, the Professor proposed the introduction of a seton, which, however, was violently opposed by the other attendant. "It would be inhuman," said he, "to have recourse to so painful a means, in a case which is so far advanced as to leave no hope." Nevertheless, the seton was used, and the patient rescued from certain death.—*Gaz. des Hôp.*

DIVISION OF THE TENDO-ACHILLIS,

FOR DEFORMITY.

A man, 46 years of age, was affected, from early life, with that kind of club-foot which compels the patient to walk on the toes. The deformity, however, was not congenital; it had come on at the age of six, in consequence of the bite of a dog in the heel. This obliged him to walk on his toes, and the gait, which at first was accidental, became habitual. The ankle-joint was entire; the foot of natural dimensions; the heel raised several inches from the ground. The remarkable success obtained in analogous cases, by M. Stromayer, in Hanover, and by M. Duval, in Paris, by means of dividing the tendo-achillis, and following this up by continued extension, determined M. Bouvier, under whose care the patient was, to perform the operation exactly in the manner of the Hanoverian surgeon. It is extremely simple as to its execution: a long narrow-pointed bistoury is thrust in behind the tendon and parallel to it, about six fingers' breadth above the external ankle. The blade of the instrument, as it were, grazes the posterior surface of the tendon, but the point is not pushed through the skin on the opposite side. A probe-pointed bistoury, as narrow as the other, and with a very convex cutting edge, is immediately introduced into the passage thus made, in such manner as completely to divide all the fibres of the tendo achillis. The operation being completed, the foot, in this case, admitted of being at once restored to its natural position. The wound healed by the first intention, and was cicatrized within a day or two. The foot was kept under the action of a very simple apparatus, by which it was kept always inclined towards flexion on the leg, keeping the heel down, and the toes in their proper place.—*Gazette des Hôpitaux.*

PHLORIDZIN.

Such is the name given to a substance which has recently been obtained from the bark of the apple-tree; more particularly from that of the root. It is said also to exist in the plum, pear, and cherry-trees. Phloridzin chrystallizes, is soluble in water, particularly at an elevated temperature;—soluble in concentrated acids, without decomposition, not solidifiable, and not containing azote as a constituent principle. It has been chiefly used in intermittents, in doses of from 10 to 15 grains. Various other particulars regarding it are detailed in the *Bulletin Méd. Belge.*

ALDERSGATE SCHOOL OF MEDICINE.

DR. CUMMIN.

The conduct of the *Lancet*, in assailing, and attempting to injure, the character of every gentleman supposed to be connected with this journal, is a proof at once of its black malignity, and of its hate and dread of ourselves. We have much pleasure in giving insertion to the following document (the best answer, in the present instance, to the *Lancet's* lies), volunteered to Dr. Cummin by his pupils and several other students belonging to the Aldersgate School. It was presented to him, with a suitable address, by Mr. Henry Landor, and a deputation of the students, on Friday, the 4th instant.

"Perceiving a statement in the *Lancet* of last week, to the effect that it was the intention of Dr. Cummin to resign his connexion with the Aldersgate School, 'in consequence of the exasperated feelings of the students;' we, the undersigned, pupils of that school, come forward to state that no such feeling exists amongst us: on the contrary, we hope that as Dr. Cummin possesses the esteem of all of us, we shall long have the pleasure of meeting him as a lecturer in our theatre."

Fifty-five signatures are attached; and we are informed that the number would have been doubled, had it been generally known that the document was in preparation. We regret we have not room for the names at full length.

THE METROPOLITAN UNIVERSITY.

Several gentlemen have been mentioned as likely to receive appointments either as Commissioners or Examiners in the new University. We refrain, however, from giving them at present, because we believe that none have as yet been actually nominated, and because we feel assured that some of those with whose names rumour has been busy never will be so. By the way, it is scarcely fair, we think, to drag any one before the public in a manner which necessarily becomes rather awkward, if the honour, prematurely announced, be not eventually conferred.

NEW MEDICAL WORKS.

On the Analysis of the Blood and Urine, in Health and Disease. By G. O. Rees. 8vo. 5s. 6d. bds.

A Treatise on the Prevention and Cure of Pulmonary Consumption. By Robert Little, M.D. 8vo. 6s. bds.

Obstetric Tables. By G. Spratt. Part II. 4to. 21s. bds.

COLLEGE OF SURGEONS.

LIST OF GENTLEMEN WHO RECEIVED DIPLOMAS IN FEBRUARY.

William Sall, A.
George Hopper, Scarborough.
Edward Wade, Bristol.
Samuel Hill, St. Thomas's Street, Southwark.
William Turner, Newcastle-upon-Tyne.
Frederick Francis Whitfield, Ashford.
George Robert Fraser, Dublin.
John Bland Wood, Pontefract.
George Kirkham Paxon, Hampstead.
James Jopp, Aberdeen.
James Allanson.
George Brain List, Fishbourne, Isle of Wight.
George Warren Watts Firth, Norwich.
Henry Welby, Farnden, Notts.
James Vaughan, Asthboy, County of Meath.
John Parsons, Goalhurst, Somerset.
George Thompson Gream, London.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

March 10, 1836.

James Rattan.
Decimus Hands, 23, Duke Street, Grosvenor Sq.
Thomas Ridout Tuck, London.
John Dean Bishop.
James Johnson.
Thomas Lowe.
Edward Richardson, Preston.
Matthew H. Cory, Kettlestones.
Robert Mortimer.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Mar.* 8, 1836.

Abscess	3	Hooping Cough	5
Age and Debility	62	Inflammation	31
Apoplexy	7	Bowels & Stomach	3
Asthma	32	Brain	3
Cancer	3	Lungs and Pleura	4
Childbirth	5	Insanity	2
Consumption	73	Liver, diseased	10
Convulsions	29	Measles	10
Croup	3	Mortification	9
Dentition or Teething	9	Paralysis	1
Dropsy	16	Scrofula	1
Dropsy on the Brain	15	Small-pox	8
Dropsy on the Chest	3	Spasms	2
Fever	3	Thrush	1
Fever, Scarlet	5	Tumor	2
Fever, Typhus	1	Unknown Causes	22
Gout	2		
Hæmorrhage	1	Casualties	4
Heart, diseased	3		

Increase of Burials, as compared with the preceding week . . . 105

METEOROLOGICAL JOURNAL.

March, 1836.	Thermometer.		Barometer.	
Thursday . 3	from 35	to 49	29·57	to 29 69
Friday . . 4	31	47	29·65	29 34
Saturday . 5	38	44	29·41	29 32
Sunday . . 6	26	48	29 32	29·18
Monday . . 7	26	49	29 33	29·36
Tuesday . . 8	32	41	29 32	29·46
Wednesday 9	25	44	29·43	29 28

Prevailing winds, S.W. & S.E.

Except the mornings of the 5th and 7th, generally cloudy; with frequent showers of rain.

Rain fallen, 325 of an inch.

Charles Henry Adams.

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 19, 1836.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XXV.

I PROCEED in this lecture to examine—

NITRIC ACID.

History and synonymes.—This acid was known to Geber in the seventh century. The nature of its constituents was shewn by Cavendish, in 1785, and their proportion afterwards determined by Davy, Gay-Lussac, and Thomson. It has been known by various names, such as *Glauber's spirit of nitre, aqua fortis,* &c.

Native state.—It is never found in nature pure. Combined with potash, soda, lime, and magnesia, it is met with on the surface of the earth in various parts of the world. Traces of it have been detected in rain-water, after thunder-storms. It is found in some vegetables; for example, *borage,* the *nettle,* the *sunflower,* and *pellitory of the wall,* in *cissampe'os pareira,* &c.

Preparation.—In the London Pharmacopœia, equal weights of protohydrate of sulphuric acid and nitrate of potash are ordered to be distilled in a glass vessel. These proportions are about equal to two atoms of the protohydrate, and one of the nitrate.

The *theory* of the process is the following:—The potash of the nitrate uniting with the real acid of the protohydrate, forms bisulphate of potash; the nitric acid of the nitrate, with $1\frac{1}{2}$ atoms of water of the protohydrate, forms 1 atom of liquid nitric acid which distils over, while half an atom of water remains in combination with the bisulphate. In the following formula I have doubled the number of atoms, to obviate the difficulty of employing half atoms.

Reagents.	Results.
$2\ \dot{K}\ \overset{\therefore\therefore}{N}$	$2\ \overset{\therefore\therefore}{N} + 3\ \dot{H}$
$4\ (\dddot{S} + \dot{H})$.	$2(\dot{K} + 2\ \dddot{S}) + \dot{H}$

The residual salt, when crystallized, constitutes the *supersulphate of potash* of the Pharmacopœia, or *sal enixum* of the shops. Manufacturers seldom use glass vessels, but an iron or earthen pot, with an earthen head; the latter is connected with large earthen two-necked bottles. The acid, when pure, ought to be colourless, but it is generally more or less coloured; for in the first part of the process, the excess of sulphuric acid deprives the nitric acid of its water, and in consequence of this the latter is decomposed into nitrous acid and oxygen: so also, in the latter part of the process, nitrous acid is again generated from the preponderance of the sulphuric acid. Diluting the sulphuric acid with water will counteract the formation of this nitrous acid, by saturating the affinity of the sulphuric acid for water.

Properties. — When pure, it is colourless; but owing to the presence of nitrous acid, is sometimes yellowish, brownish, or even greenish. In this state it constitutes the *nitrous acid* of the shops. We may readily generate this compound by passing the binoxide of nitrogen into colourless nitric acid: the latter is partially deoxidated and converted into nitrous acid: the binoxide also becomes nitrous acid by oxidation. This coloured acid may be rendered colourless by gently heating it, to drive off the nitrous acid. It fumes in the air, owing to its affinity for water: by exposure to solar light, it is partially decomposed into nitrous acid and

oxygen, and becomes coloured. By the action of combustibles (as charcoal, phosphorus, sulphur, and some of the metals) it is decomposed, with the evolution of binoxide of nitrogen; and by the action of the hydracids it is also decomposed.

Characteristic tests.—This acid is best recognized by the following tests:—(*a.*) Drop into it a piece of copper (as wire or foil); effervescence takes place, owing to the escape of the binoxide of nitrogen, which becomes brown in the air, by the formation of nitrous acid gas, while a greenish blue solution of the nitrate of copper is formed. (*b.*) A little morphia, brucia, or strychnia, dropped into the acid, renders it red; and this colour is deepened by supersaturating with ammonia. (*c.*) If a little pure hydrochloric acid be added, the compound has the power of dissolving leaf gold. (*d.*) Boiled with sulphate of indigo, it destroys the colour of the latter. (*e.*) Lastly, saturated with an alkali (as potash), the resulting salt has the power of deflagrating, when thrown on a red-hot cinder, or charcoal.

Composition.—Pure or anhydrous nitric acid is composed of

1 atom of nitrogen	= 14
5 atoms of oxygen (8×5)	= 40
	54

In the liquid form, however, it is always combined with water, and the strength of it in this state is determinable either by taking its specific gravity, or by observing its saturating power. The acid prepared according to the Pharmacopœia has a specific gravity of 1·500; and Mr. Phillips thinks it is the strongest procurable, though several chemists assert they have obtained a denser acid. When of this gravity, nitric acid is said by Mr. Phillips to be thus composed:—

Anhydrous nitric acid	75	...or 1 atom	54
Water	25	..or 2 atoms .... (9×2)	18
	100	or 1 atom of bi-hydrate of nitric acid	72

However, the experiments both of Dr. Ure and of Dr. Thomson seem to show that acid of this specific gravity does not contain so much water. Dr. Ure states that it consists of

Anhydrous nitric acid..79·7	which would give	1 atom of anhydrous acid..	54
Water................20·3	very nearly	$1\frac{1}{2}$ atom of water $(9\times1\frac{1}{2})$	13·5
		or 1 atom of sesqui-hydrate of nitric acid	67·5

so that on the latter supposition the strong liquid acid is a *sesqui-hydrate.*

Impurities.—A little nitrous acid is the only impurity likely to be present, and which is of no practical importance. Sulphuric acid may be detected by a barytic salt, and muriatic acid by nitrate of silver. Always dilute the acid before testing for the impurities.

Physiological effects.—(*a.*) In the *concentrated* form the acid acts as a powerfully corrosive poison, which property it derives in part from its affinity for water, but more especially from the facility with which it gives out oxygen; so that the appearances caused by its action on some of the tissues are different from those produced by sulphuric acid. Thus the permanent yellow stain which it communicates to the cuticle is peculiar to it. Iodine, indeed, stains the skin yellow or brown, but a little caustic potash readily removes the colour, whereas the yellow stain produced by nitric acid becomes orange on the addition of an alkali or soap. Bromine also stains the skin yellow, but when recently produced, the colour may be removed by potash. In Dr. Roupell's work on Poisons is a drawing representing the tongue, pharynx, &c., of a boy poisoned by this acid; and it will be observed that in those parts where the cuticle is left adherent, it has acquired a citron colour. The same kind of blackening is frequently noticed in the stomach as when the sulphuric acid has been swallowed. The *symptoms* caused by the ingestion of this acid in the concentrated state are analogous to those described in the last lecture when speaking of sulphuric acid. The yellow, citron, or orange spots, sometimes observed on the lips, chin, or heads, will, when present, at once indicate the kind of acid swallowed. Sometimes the binoxide of nitrogen is evolved by the mouth.

(*b.*) Properly *diluted*, it produces effects similar to those caused by the dilute sulphuric acid already described. It is said, however, to act less evidently as a tonic, and to be more apt to disagree with the stomach, so that it cannot be employed for so long a period. In some cases it has excited ptyalism, and from this circumstance, as well as from the occasional benefit derived from its use in the venereal disease, it has by some writers been compared, in its operation, to mercury, a com-

parison founded rather on theoretical than practical considerations.

Use.—(*a.*) *Externally*—it is employed both in a concentrated and diluted form. In *sloughing phagædena* the *strong acid* is applied as recommended by Mr. Welbank, with the best effects, as I have on several occasions witnessed. The best mode of using it is by a piece of lint tied round a small stick or skewer. When the slough is very thick, it is sometimes necessary to remove part of it with a pair of scissors, in order to enable the acid to come in contact with the living surface. *Largely diluted*, (as 50 or 60 drops of the strong acid to a pint or quart of water) it is a favourite application of Sir Astley Cooper to sloughing and other ill-conditioned sores. In the form of *ointment* (the *pommade d'Alyon*, or *oxygenated ointment*), it has been used in various skin diseases, more particularly porrigo and the itch.

(*b*) *Internally*—this acid has been very extensively employed in *venereal diseases*, but with variable success. That it has been, and is frequently serviceable, no one can doubt who reads the immense body of evidence offered in its favour by Scott, Kellie, Albers, Prioleau, Rollo, Cruikshanks, Beddoes, Ferriar, and others. But on the other hand it is equally certain that on many occasions it has been useless. The same remark, indeed, may be made of mercury, or of any other remedy, but the frequency of beneficial influence of this acid, will not bear a moment's comparison with that of mercury. However, we frequently meet with syphilitic cases in which the employment of this mineral is not admissible, or is useless. Thus it will be found that mercury can rarely be employed with advantage in scrofulous subjects, or in persons whose idiosyncracies render them peculiarly susceptible to the influence of this metal: again, in sloughing sores, it would be inadmissible. Now these are the cases in which we employ nitric acid with benefit; and I believe the best mode of administering it is in conjunction with the compound decoction of sarsaparilla. Under the head of "nitric acid," in Mr. Samuel Cooper's *Dictionary of Practical Surgery*, you will find a good historical account of the uses of this acid in the venereal disease.

In *skin diseases* (especially in impetigo and pityriasis) it is spoken favourably of by Rayer, given to the extent of half a drachm daily in barley-water. In *lithiasis* it has been used in the same cases as the sulphuric acid. As a *disinfectant* it has sometimes been employed in the form of vapour, which is obtained by adding strong sulphuric acid to powdered nitrate of potash, and applying heat.

Administration.—For facilitating its employment, a dilute acid (*acidum nitricum dilutum*) is kept in the Pharmacopœia. It is prepared by mixing one fluid ounce of the strong acid with nine fluid ounces of water. This liquid has a specific gravity of 1·080. It is given in doses of from 10 to 50 or 60 minims.

I have already alluded to an *ointment of nitric acid.* It is prepared in imitation of Alyon's ointment, and hence it is frequently termed *pommade d'Alyon.* In the Dublin Pharmacopœia is a formula for it. It is there prepared by adding $5\frac{1}{2}$ fluid drachms of nitric acid to a mixture of 1℔. olive oil, and 4 ounces of lard. Binoxide of nitrogen gas is evolved, showing that the fatty matter receives oxygen (hence the compound is called *oxygenized fat*). This preparation has a firm consistence, and a yellow colour. It has been employed in various cutaneous diseases. It is said to be more efficacious when recently prepared.

Antidotes. — Poisoning by nitric acid requires precisely the same treatment as that already described for sulphuric acid.

HYDROCHLORIC ACID.

History and synonymes.—Liquid hydrochloric acid was probably known to Geber. Glauber, however, first procured it by the process now usually followed, and hence its name, *Glauber's spirit of sea salt.* It has also been called *marine acid*, and usually *muriatic acid.* Scheele, in 1774, may be regarded as the first person who entertained correct notions of its nature, and we are indebted to Davy for establishing the fact that this acid is composed of hydrogen and chlorine.

Native state.—It is found in both the organized and the inorganized kingdoms. Thus it is one of the products of volcanoes, and is found in the neighbourhood of Mount Ætna, Vesuvius, &c., in a gaseous state. It seems to be an essential constituent of the gastric juice, and hence, probably, one of the uses of common salt as an article of food, is to furnish this acid.

Gaseous Hydrochloric Acid.

Preparation.—It is obtained by the action of strong liquid sulphuric acid (*protohydrate of sulphuric acid*) on dried common salt, (*chloride of sodium*) contained in a tubulated retort, the gas being collected over mercury; or it may be generated in an oil flask and collected in bottles, by means of a tube curved twice at right angles. The *theory* of the process is this:—The water of the hydrated sulphuric acid is decomposed into its elements, hydrogen and oxygen, the latter of which unites with the sodium of the salt to furnish soda, with which the sulphuric acid combines to form sul-

phate of soda; while the hydrogen of the water, with the chlorine of the salt, forms hydrochloric acid gas.

Reagents.	Results.
Na Chl	H Chl
$\dddot{S}\ \dot{H}$	$\dot{Na}\ \dddot{S}$

Properties.—It is a colourless invisible gas, fuming in the air, in consequence of its affinity for aqueous vapour. It is rapidly absorbed by water. Its specific gravity is, according to Dr. Thomson, 1·2847. It has a pungent odour and acid taste. Under strong pressure (40 atmospheres) it becomes liquid. It is neither combustible nor a supporter of combustion. When added to a base, (that is, a metallic oxide), water and a chloride are the results.

Characteristic tests.—Its fuming in the air, its odour, its reddening moistened litmus paper, and forming white fumes with the vapour of ammonia, are part of the means by which it may be recognized. Dissolved in water, it may be detected by nitrate of silver, as I shall presently mention, in speaking of the liquid acid.

Composition.—It is composed of—

1 atom of chlorine . . .	36
1 atom of hydrogen . . .	1
	37

Or by measure of equal volumes of its constituents, which combine without condensation.

Constituents.	Results.
Chlor. =36	Hydrochloric acid gas =37
Hydr. =1	

This composition is determinable both by synthetic and analytic means. Thus hydrogen and chlorine may be made to combine by light, heat, or electricity, and the result of the combination is hydrochloric acid. On the other hand, we can decompose this gas by electricity, or by various reagents, as the metals.

Physiological effects. — (*a.*) *On vegetables.* Mixed with 20,000 times its volume of atmospheric air, this gas is said by Drs. Christison and Turner to have proved fatal to plants, shrivelling and killing all the leaves in twenty-four hours. But in the 10th volume of the MEDICAL GAZETTE you will find some experiments, detailed by Messrs. Rogerson, from which it would appear that this gas is not injurious to vegetables when mixed with 1500 times its volume of air. Dr. Christison ascribes these different results to Messrs. Rogerson having employed glass jars of too small size. We have good evidence of the poisonous operation of this gas on vegetables in the neighbourhood of those chemical manufactories in which subcarbonate of soda is procured from common salt. The fumes of the acid which issue from these works have proved so destructive to the surrounding vegetation, that in some instances the proprietors have subjected themselves to actions at law, and have been compelled either to pay damages, or to purchase the land in their immediate neighbourhood.

(*b.*) *On animals* this gas acts injuriously, even when mixed with 1500 times its volume of atmospheric air. Mice or birds introduced into the pure gas struggle, gasp, and die, within two or three minutes. Diluted with atmospheric air, the effects are of course milder, and in a ratio to the quantity of air present. In horses it excites cough and difficulty of breathing. When animals are confined in the dilute gas, in addition to the laborious and quickened respiration, convulsions occur before death. Messrs. Rogerson state, that "In a legal suit for a general nuisance, tried at the Kirkdale Sessions-house, Liverpool, it was proved that horses, cattle, and men, in passing an alkali-works, were made, by inhaling this gas, to cough, and to have their breathing much affected. In the case of Whitehouse *v.* Stevenson, for a special nuisance, lately tried at the Staffordshire assizes, it was proved that the muriatic acid gas from a soap manufactory destroyed vegetation, and that passengers were seized with a violent sneezing, coughing, and occasional vomiting. One witness stated, that when he was driving a plough, and saw the fog, he was obliged to let the horses loose, when they would gallop away till they got clear of it." It acts as an irritant on all the mucous membranes.

(*c.*) *On man* this gas acts as an irritant poison, causing difficult respiration, cough, and sense of suffocation. In Mr. Rogerson's case, it caused also swelling and inflammation of the throat. Both in man and animals it has appeared to produce sleep.

The action of hydrochloric acid gas on the lungs of animals is injurious in at least two ways: by excluding atmospheric air, it prevents the decarbonization of the blood; and, secondly, by its irritant, and perhaps also by its chemical properties, it alters the physical condition of the bronchial membrane. The first effect of attempting to inspire the pure gas seems to be a spasmodic closure of the glottis. Ap-

plied to the conjunctiva, it causes irritation and opacity.

Use.—It has been employed as a *disinfectant*, but it is admitted on all hands to be much inferior in this respect to chlorine. The Messrs. Rogerson go so far as to deny that it possesses any disinfecting properties at all. It is perhaps equally difficult to prove or disprove its powers in this respect. The experiments of Guyton-Morveau, in purifying the cathedral of Dijon, in 1773, are usually referred to in proof of its disinfecting property. I would recommend you always to prefer chlorine, or the chlorides of lime or soda; but if you are so placed that you cannot obtain these, why then you may resort to this acid. In neutralizing the vapour of ammonia it is certainly powerful.

Application. — In order to fumigate a room, building, or vessel, with this gas, pour some strong sulphuric acid over dried common salt, placed in a glass capsule or iron pot, heated by a charcoal fire.

Antidote.—Inhalations of the vapour of ammonia may be employed to neutralize those of hydrochloric acid gas. Symptoms of bronchial inflammation are of course to be treated in the usual way.

Liquid Hydrochloric Acid.

Preparation.—In the Pharmacopœia, this is ordered to be prepared by distilling in a glass retort 2 pounds of dried common salt, 20 ounces by weight of sulphuric acid, and 1½ pints of water.

Manufacturers, however, do not follow this process. They usually employ an iron vessel, and put the water into the receiver instead of mixing it with the sulphuric acid. In a large manufactory near London, an iron cylinder, placed horizontally over a fire, is used for the retort. The salt is put in at one end, which is then closed up by a round iron plate, perforated to allow the introduction of the leg of a leaden funnel, through which the sulphuric acid is added. The muriatic acid gas is conveyed by a pipe into a double-necked stoneware bottle, half filled with water, and connected with a row of similar bottles. The *theory* of the process is the same as that for procuring the gas.

Properties.—When pure, the liquid acid is colourless, fumes in the air, and possesses the usual characteristics of an acid. It has the odour and taste of the gaseous acid. Its specific gravity varies with its strength. It is decomposed by some of the metals (as zinc and iron), hydrogen gas being evolved, while a chloride is formed in solution. It is decomposed by those oxyacids which contain five atoms of oxygen—namely, nitric, chloric, iodic, and bromic acids: the oxygen of these acids unites with the hydrogen of the hydrochloric acid to form water.

Characteristic tests. — (*a.*) A solution of nitrate of silver causes a white precipitate in dilute hydrochloric acid. This precipitate is the chloride of silver, and is recognized to be such by the following properties:—it is soluble in a solution of ammonia, but is insoluble in nitric acid, whether cold or hot; secondly, it readily blackens when exposed to the solar rays; and, lastly, it fuses at about 500° F., and forms a horn-like mass: hence its name, *Luna cornea.* The facility with which the chloride dissolves in ammonia, distinguishes it from the bromide and iodide: its insolubility in nitric acid distinguishes it from carbonate, oxalate, and cyanuret of silver. (*b.*) The addition of a few drops of nitric acid to the hydrochloric acid, enables it to dissolve leaf gold. (*c.*) Liquid muriatic acid produces white fumes when brought near ammoniacal vapour.

Strength.—The strength of the solution is determined by observing its specific gravity, or by examining its saturating power. Prepared according to the College formula, its specific gravity is 1·160; and according to Mr. Phillips, it consists of—

Hydrochloric acid gas .	32·4
Water	67·6
	100·0

By reference to Dr. Thomson's table, you will observe that acid having the above-mentioned gravity may be regarded as consisting very nearly of—

1 atom real hydrochloric acid	37
8 atoms water (9 × 8) . . .	72
1 at. octohydr. of hydrochlor. acid	109

Atoms Acid.	Atoms Water.	Acid in 100 of Liquid.	Specific Gravity.
1	6	40·659	1·203
1	7	37·000	1·179
1	8	33·945	1·162
1	9	31·346	1·149
1	10	29·134	1·139
1	11	27·206	1·1285

Impurities.—The liquid hydrochloric acid of the shops has usually a yellow colour. This generally arises from the presence of some perchloride of iron, and occasionally from free chlorine. Dr. Thomas Thomson suspects that bromine may be present, partly because this substance is found in common salt, and partly because

a small quantity of this substance will give a similar colour to hydrochloric acid. The *chloride of iron* may be recognized by saturating the acid with soda or potash, and then adding the ferrocyanuret of potassium, which causes the formation of a blue colour (Prussian blue), if iron be present; an excess of caustic ammonia will also distinguish it, by precipitating a light brown peroxide of iron. *Free chlorine* is recognized by the power of the solution to dissolve leaf-gold. *Sulphuric acid* may be detected by a solution of the chloride of barium causing a white precipitate (sulphate of barytes), insoluble in acids and alkalies. In applying the last test, let the suspected acid be diluted with five or six times its volume of water, otherwise a fallacy may arise from the crystallization of the chloride of barium.

Physiological effects.—(*a.*) The *local* effects vary with the strength and quantity of the acid employed. In the *concentrated* form it acts as a corrosive poison; but is less powerful than either the sulphuric or nitric acids. The particular nature of its chemical action on the tissues is not so well known as either of the before-mentioned acids. Soon after swallowing it, thick white fumes of a pungent odour are evolved by the mouth. Like the other mineral acids, when thrown into the veins it kills, by coagulating the blood. When *diluted* it ceases to be corrosive, but acts as an irritant: hence, when swallowed, it excites a sensation of warmth in the throat and stomach.

(*b.*) Its *remote* action is very similar to that of sulphuric acid, already described. It has, however, been supposed to possess an exciting property with respect to the nervous and vascular systems.

Uses.—(*a.*) As a *local* agent, it has been employed in a dilute form as a gargle in aphtha, and other affections of the mouth, and also in ulcerated throats, especially of scarlet fever. Diluted with water, it has been applied to frost-bitten parts, to chilblains, &c. In the proportion of from 8 to 12 drops in three or four ounces of water, it has been employed as an injection in gonorrhœa. It has been employed both in the concentrated and dilute forms, in sloughing phagædena, but it is inferior to nitric acid.

(*b.*) Its *remote* uses are so similar to those of the dilute sulphuric acid, that it is hardly necessary to particularize individual cases. Suffice it, therefore, to say it has been used in fevers, (especially of the kind at one period called putrid), in gout, lithiasis, diseases of the stomach and liver, in venereal affections, &c. It does not so readily disorder the stomach as nitric acid, and in this respect agrees with sulphuric acid; it generally, however, causes a relaxed state of the bowels. In a very dilute form, and in the absence of common salt, it might be employed as an antidote in poisoning by the nitrate of silver.

Administration. — The most agreeable mode of exhibiting it internally is in the infusion of roses, substituting this acid for the sulphuric acid.

Antidote.—In a case of poisoning by liquid hydrochloric acid, the treatment would be precisely similar to that of poisoning by sulphuric acid. Chalk, whiting, magnesia, or soap, are the best antidotes. In the absence of these, oil and albuminous liquids of any kind might be given. The gastro-enteritis is, of course, to be combated in the usual way.

NITROHYDROCHLORIC ACID.

History and synonymes.—This liquid was formerly called *aqua regia*, or *nitromuriatic acid*. Its nature was first explained by Davy.

Preparation.—It is readily prepared by mixing 2 parts of hydrochloric acid with 1 part of nitric acid. In the arts, however, muriate of ammonia, or common salt, is often substituted for muriatic acid; or nitrate of potash for the nitric acid. By the re-action of the nitric and hydrochloric acids on each other, we obtain nitrous acid, chlorine, and water.

Reagents.	Results.
H Chl.	Chl.
$\overset{\cdot\cdot\cdot\cdot\cdot}{N}$	$\overset{\cdot\cdot\cdot\cdot}{N}$
	$\dot{H}$

Properties.—It has a yellowish or reddish tinge. Its most remarkable property is that of dissolving gold and platinum; metals that are insoluble, in either nitric or hydrochloric acids, separately.

Characteristic tests.—It is recognised by its colour, by its power of dissolving gold, by its precipitating a solution of nitrate of silver; the precipitate being insoluble in nitric acid, but soluble in ammonia; and by the production of two salts, a chloride and a nitrate, when an alkali is added to it.

Physiological effects.—It is a powerfully corrosive poison, acting in a similar manner to the nitric acid.

Uses.—It has been employed *internally* in the same cases as nitric acid, more especially syphilis, diseases of the liver, and some of the exanthemata. *Externally* it has been used as a bath, either local or general, in syphilis and hepatic affections. In India the whole body (the head excepted) is immersed, but in this country pediluvia only are usually employed, or merely sponging the body. The

nitro-muriatic bath is prepared in wooden tubs, by adding this acid to water, until the latter becomes as acid to the taste as vinegar. The patient should remain in the bath from 10 to 30 or 40 minutes. It excites tingling and pricking of the skin, and is said to affect the gums and salivary glands, causing plentiful ptyalism; indeed, we are told that without the latter effect, every trial is to be regarded as inconclusive. In the passage of biliary calculi this bath is said to be remarkably effective.

Antidote.—Poisoning by this acid is to be treated in the same way as that by nitric acid.

ON THE DIVISION OF STRICTURE IN STRANGULATED HERNIA, WITHOUT OPENING THE SAC.

To the Editor of the Medical Gazette.

SIR,

If you consider this communication of sufficient importance, I shall feel obliged by your affording it a place in your valuable journal. The subject is, the operation of dividing the stricture without opening the sac in strangulated hernia; an operation which at present is, in my opinion, by no means duly estimated by the profession, nor by any means so frequently performed as it ought to be. It was first, I believe, recommended by Jean Louis Petit, early in the eighteenth century; and it has subsequently met with the advocacy of several eminent practitioners. Mr. Lawrence, in his valuable Treatise on Hernia, 4th edit. p. 271, says, "the advantages of operating without opening the hernial sac are so great, in cases where the tumor exceeds a *moderate size*, that I strongly recommend its adoption in all such instances." But, at the present day, the merit of more particularly directing the attention of surgeons to it belongs to Mr. Key; who has not only pointed out, at some length, the advantages attending it, but has also pretty fully answered the objections which have been at different times brought forward against it. Still, however, surgeons seem to regard it as a dangerous innovation, and indicate the greatest disinclination to leave the older and more beaten track: more evidence on the subject, therefore, than has been hitherto adduced, appears to be required.

I fully concur in opinion with Mr. Key, that if the operation were more generally performed, it would be the means of preserving many lives. I consider that, if performed properly in all its steps, and at a sufficiently early period, it would divest the operation for strangulated hernia of a great part of its danger. It would moreover afford the surgeon, in many cases, the opportunity of operating under more favourable circumstances, as there are many, very many, patients who would submit to an early operation, if they could be assured that no danger would arise from it. So convinced am I of the safety of the operation, of the facility of performing it, as well as of the great danger of delay in cases of strangulated hernia, that if I were the subject of the complaint myself, I should beg, after the taxis, with the aid of the warm bath, and venesection, had been once fairly employed, unsuccessfully, by an experienced surgeon, to be operated on without the loss of another moment.

In the case the particulars of which I am about to narrate, although the operation was performed under very unfavourable circumstances, yet it was attended with complete success.

I was requested on October 4th, 1834, by Mr. Boote, surgeon, of Bedford Row, to visit a patient of his, who was suffering from strangulation of a scrotal hernia, on the left side. The patient was in his 80th year: the hernia was of many years' duration; and the symptoms of strangulation had existed for about 20 hours. The usual remedies had been employed, but without affording any relief. At the time I was called in the bowels were obstinately constipated, there was frequent vomiting of matter, much more like the contents of the small intestines than any thing to which we could compare it; the abdomen was tumid and tense; and the hernial tumor, which was of large size, was likewise tense, and extremely tender to the touch, particularly about the external ring. The pulse was not very much accelerated, but was very feeble and irregular, and the powers of the whole system were in an exceedingly depressed state. The countenance was shrunken, the complexion of a leaden hue, and the eyes glassy. Such symp-

toms co-existing with strangulated hernia, in a patient of such advanced age, were, I think it will be admitted, sufficient to exclude from the mind of even the most sanguine, any reasonable expectation that an operation, be it what it might, could afford relief. But, having to attend the hospital, I embraced the opportunity of mentioning the case to Messrs. Lawrence and Stanley, who agreed, that notwithstanding the great age, and desperate condition of the patient, it would be right he should have the chance of relief which an operation would give him. I therefore, fortified by their opinion, returned to my patient, determined, if I could gain his consent, and found that no particular change had taken place during my absence, to proceed at once to an operation. I found him very much in *statu quo*, and hopeless of recovery; but at the same time I experienced very little difficulty in persuading him to submit to the operation, as a means that might possibly relieve him, and which I informed him would be unattended with danger. I had made up my mind to operate by dividing the stricture without opening the sac; and as the hernia was of such long standing, I had no doubt of being able to accomplish my object. This, therefore, I proceeded, with the assistance of Mr. Boote, at once to do.

I commenced the incision of the skin about an inch above the external inguinal ring, and extended it downwards two inches and a half in front of the tumor. I then carefully dissected down to the tendon of the external oblique muscle, and to the sac, along the whole course of the wound. Having done this, I passed a slightly curved director under the edge of the ring, which, with a probe-pointed bistoury, I divided directly upwards, as far as seemed necessary to remove the source of strangulation. I then attempted to reduce the hernia, but found that there was still an impediment, which, on examination, proved to arise from the compression occasioned by two fine bands of unyielding fascial, or tendinous-like fibres, about a quarter of an inch from each other, extending across the neck of the sac, and to which they were so closely adherent as to be scarcely distinguishable from the sac itself. I therefore carefully divided them, having previously, and not without some difficulty, insinuated the flat end of a probe underneath them. I then made gentle pressure with both my hands on the tumor, which now, I had the satisfaction to find, readily yielded, the intestinal part of its contents returning with a gurgling sound into the abdomen. The patient was instantly sensible of relief. There still, however, remained some omentum down, which, as I found it was not easily to be returned, I resolved to leave in the sac without further interference. There was also evidently some fluid in the sac. The wound was closed with sutures and adhesive plaster in the usual way, the scrotum was supported, and the patient laid on his back in bed. A fomentation flannel was ordered to be kept constantly on the abdomen, a dose of calomel and opium was given at night, and a common clyster was directed to be administered every fourth hour, till the bowels were freely opened. The patient slept well, the bowels acted copiously in the course of the night, there was no recurrence of sickness, and in the morning he was able to take nourishment, and appeared almost as well as he could have been if the intestine had been returned without an operation. Not a bad symptom subsequently occurred: and on the third day I was able, with very little difficulty, to return the omentum into the abdomen; after which the parts were easily kept up by means of a compress and bandage. The wound healed by the first intention, and the fluid in the sac became quickly absorbed, so that on the Saturday following all was well, except that the small ulcers produced by the ligatures had not quite closed. But, on the subsequent Tuesday (the eleventh day from the operation), they had likewise healed, and I took leave of my patient, as well as he was before the operation. Since then he has constantly worn a truss, and not long ago he called on me to shew me that, notwithstanding his great age, he is able to walk about without inconvenience.

In dividing the stricture while the sac was entire I experienced no difficulty: indeed, though I proceeded most cautiously, the whole operation was completed in little more than three minutes.

That, if in this case the sac had been opened, as in the ordinary operation for

strangulated hernia, a fatal result would have taken place, I cannot have any doubt. I feel justified in this opinion, because in all the experience I have had in hospital and private practice, in cases treated by the most experienced surgeons, I never knew a patient under equally unfavourable circumstances recover. The rapidity too of the recovery in this case, unattended as it was by a bad symptom, may be, I consider, fairly attributed to the comparatively much slighter injury which by this mode of operating is inflicted on the system. That, however, patients often recover without the occurrence of a bad symptom after the sac has been opened, I am well aware. Indeed, of two patients in St. Bartholomew's hospital on whom I had operated for strangulated hernia, a short time before the above mentioned case, one recovered, if I may use the expression, almost without a symptom. The period of recovery, however, was much more protracted, occupying many weeks, and yet the operation was performed under circumstances much more favourable, and on a patient many years younger—ætat. 55.

In the other case just adverted to as occurring in the hospital, I operated by dividing, in the first instance, the stricture externally to the sac, which I accomplished, as in the first-mentioned case, without the slightest difficulty.

It was a case of large scrotal hernia, of long standing. The patient was ætatis 50, an habitually hard drinker, and an unhealthy subject. When I arrived at the hospital, I found that the strangulation had existed about eighteen hours; the abdomen was much distended, and very tender, as also was the hernial tumor; and the symptoms were altogether extremely urgent. The taxis, and all the usual means for the reduction of strangulated hernia, having been employed in vain, I at once proposed an operation to the patient, to which, as his sufferings were so great, he readily consented. But previous to the operation, I stated to the students that I considered the chances were very much against a favourable result, knowing that hard drinkers generally endure any serious injury very badly. I therefore determined to endeavour to remove the strangulation without opening the sac, believing that if this could be effected, the danger of the operation would be greatly diminished. It was evident that a large portion of intestine was contained in the hernial tumor.

In performing the operation, I commenced the incision some way above the external inguinal ring, which having fairly exposed, with such portion of the tendon of the external oblique muscle, as well as of the hernial sac, as corresponded with the wound in the skin, I freely divided its upper boundary, so as completely to remove all constriction made on the tumor by that part. I then endeavoured to return the hernia, but failed, in consequence of the unyielding condition of the neck of the sac. The sac was of its natural texture, except at its neck, where we thought it appeared slightly thickened. Under these circumstances I proceeded at once to open the sac; which having done, it was found that the external stricture had been wholly removed, and that the only impediment to reduction was in the neck of the sac; and here a mere touch, as it were, of the knife, effected all the division that was necessary; the unyielding and constricting part consisting of what appeared to be little more than a thread, or, if I may use the term, a mere ligature of fine tendinous fibre. The hernia was found to consist of about two feet of intestine, and a considerable portion of omentum: moreover, there were in the sac several ounces of serum. The intestine was of a dark chocolate colour, such as I have often observed when the mucous coat has suffered more in proportion than the serous. There were no adhesions; the parts were returned without difficulty; and the whole operation occupied a very short time—very little more than must have been the case if the sac had been opened in the first instance.

The case was subsequently treated in the customary manner, and for three days every thing went on more favourably than was expected. The bowels acted copiously, and the stomach retained what was taken into it. The distention, and some tenderness of the abdomen, however, continued; and subsequently the stomach became very irritable, and the bowels extremely irregular, being always in one extreme or the other—either greatly constipated, or too much open. These symptoms were combated by depletion local and general, by calomel and opium, by tepid

bathing, and by other means, such as appeared to be indicated, and always with temporary relief; but relapse after relapse occurred, till the patient, who had survived the operation about three weeks, was completely worn out. It should be observed that the wound in the sac did not unite by the first intention, but suppurated; which, of course, as the sac was very large, became a great additional source of constitutional disturbance: any serous cavity, of equal magnitude, highly inflamed, and in a state of suppuration, would, indeed, of itself often produce very serious symptoms.

From all I observed in the progress of the preceding case, I do not hesitate to state that, in my opinion, a different result would have probably followed, if it had been possible to effect the reduction of the hernia without opening the sac. The suppuration of the latter would then, undoubtedly, have been prevented.

Whether, after the stricture had been divided, I could have employed more force in the application of the taxis, if I may use the expression, or have had recourse to any other means with safety, to accomplish the reduction, I cannot say. It was the first time I had performed the operation which I am considering; I did not like the aspect of my patient; I was anxious that the operation should not acquire any disrepute in my hands; and I therefore determined to act with the greatest caution. The sac being so tense, and containing so large a quantity of fluid, obviously prevented me, so long as it was entire, and the constriction at its neck continued, from exerting much influence over the protruded intestine. But the great obstacle to reduction was undoubtedly the unyielding condition of the neck of the sac, and which, at the time of the operation, it certainly appeared impossible to remove without opening the sac. But this is a point to which I shall have occasion incidentally to revert presently.

The number of facts in connexion with this operation, which have been recorded, are so few, that it may be said to be yet quite in its infancy; and the course which should be pursued is by no means sufficiently established.

If another case, similar to the one which I have been just relating, should come under my care, I should not hesitate, after having completely removed all external stricture that was discoverable, and the texture of the sac appearing healthy, to place the patient in bed, in a position favourable for the return of the hernia, and be content for a time with employing very much the same means as if the reduction had been effected.

Of course I do not mean these observations to apply to every case of hernia that may occur. Among hospital patients, in particular, the practice must often be quite out of the question, in consequence of the long existence of the strangulation before they are brought in. A short time ago I had to operate on a patient in the hospital, in whom the strangulation had continued between ten and eleven days. The intestine, as was anticipated, was found in a state of gangrene. But it is a question with me, whether it would not be a more judicious mode of practice to adopt in many cases of hernia, than the course which is usually pursued; whether, in fact, it would not be better to be content at first with freely dividing the stricture, so as to place the protruded parts under circumstances favourable for their circulation being restored, and consequently in a state much fitter and easier to be subsequently returned into the abdomen. If the symptoms of strangulation continued, we might then, with very little pain to the patient, divide the neck of the sac.

I should in all cases of hernia make a point to employ only a very moderate force in my endeavour to effect reduction. When there is a large portion of swollen and inflamed intestine protruded, its reduction, even when it is exposed, is by no means an easy matter, unless the aperture in the tendon is extremely large. I have often seen the intestine greatly injured by the long-continued manipulation to which it has been found necessary to subject it before it could be returned. I have more than once known the intestine (one of the ablest and most experienced surgeons being the operator) feel quite cold before its return could be accomplished. The difficulty sometimes arises from a tympanitic condition of the abdomen, but much more frequently, I believe, from the increased volume of the protruded parts. The thickening of a strangulated portion of intestine is produced, as it appears to me, in great

measure, by an infiltration of serum into its cellular tissue, which, when the strangulation is removed, would in all probability become quickly absorbed, a great diminution consequently take place in the bulk of the hernia, and thus its reduction be much facilitated.

By pursuing the course which I am advocating, much of the danger and difficulty attending cases of strangulated hernia would, I conceive, be obviated. I should always, as I have stated before, freely divide the stricture — so freely that the protruded parts, if they had acquired no adventitious adhesions, might in general, after waiting a little, be readily and easily replaced. Thus the injury that so often arises from exposure and manipulation would be avoided. The chance of a radical cure (that is, of such a complete cure that there would be subsequently no necessity to wear a truss) is always so slight, that it should never influence our mode of operating, nor ever deter us from so thoroughly releasing the intestine that it may be returned without force. Whenever the operation has been performed, there is no condition of the ring itself that will prevent the future recurrence of the hernia. Its subsequent descent can, I believe, only be prevented by adhesions contracted about the neighbourhood of the ring, or in some part of the cavity of the abdomen. There is no probability that any new structure could form, by which any of the apertures through which herniæ protrude could be so firmly and effectually closed, as to be rendered capable of withstanding the pressure exerted on the viscera by the abdominal muscles. I know, therefore, of no good reason for not dilating the ring to the fullest extent required. The sac I should never attempt to return; it is almost certain that it will have contracted adhesions to the contiguous parts; nor, the intestine being replaced, should I be very solicitous about any portion of omentum that remained. Indeed, if with the return of the intestine only, the symptoms of strangulation had subsided, I should consider the patient in a condition almost as free from danger, *cæteris paribus*, as in a case in which the reduction had been effected entirely by the taxis. If ulceration, or spacetation of a portion, however small, of intestine had taken place at the strictured part, there would most likely be such adhesions within the ring, that it would be quite impossible to effect reduction without greater force than it is at all probable any surgeon of the present day would feel justified in employing. I should, therefore, have no apprehensions on this head.

A portion of intestine may remain in a hernial sac irreducible, or simply incarcerated, for years, without the production of any bad symptoms: the stomach, notwithstanding, will continue retentive, the bowels act regularly, and every thing go on well. In a case of hernia, therefore, in which the strangulation has been completely removed, before the intestine has undergone any irrecoverable change, there would, in my opinion, be much less danger from the continuance of its mere displacement for a short time, than from its exposure during long-continued manipulation, however judiciously and carefully employed, in the attempt to reduce it. If left to itself, in the manner I have before described, and all impediment had been removed, it is very possible that in a short time the reduction might be effected by the "vis medicatrix naturæ," or call it what you will; perhaps it would be more correct to say, by its own unassisted and unimpeded action, which no doubt would be pretty vigorously exerted as soon as the circulation became completely restored. Whoever has witnessed, in a living animal, the great force exerted by the peristaltic action of the intestines, can hardly doubt its power to replace within the abdomen a portion of protruded intestine, if free from constriction and adhesions. It is not uncommon, in cases of large hernia, after urgent symptoms of strangulation have long existed, to witness the reduction, spontaneously, as it were, take place. A short time ago I was sent for to the hospital, to operate in a case which had been long strangulated; but when I arrived, I found the hernia had just gone up as the patient was being carried into the operating theatre. Cases, too, are recorded, in which portions of omentum, which could not be returned during an operation, in consequence of adhesions, have in a few days spontaneously receded into the abdomen. And in cases in which there have been no symptoms of strangulation, it is by no means very uncommon to fail in the reduction of a hernia one day, which on another will

be accomplished with the greatest facility. I feel, therefore, that under the circumstances I have stated, there would, in the generality of cases, be no dang r in allowing a short time to elapse after the division of the stricture, before any attempt was made by manipulation to effect reduction: but having failed to relieve my patient while the sac was entire, I of course should open it; and having examined its contents, act according to circumstances. I do not consider, as I stated before, that if all constriction had been previously removed, any mischief could arise from the little delay.

It should have been stated, that in performing the operation in question, we ought always to be most careful, to clear as far as possible, all external matter—condensed cellular substance, tendinous, like fibres, &c., from the neck of the sac. Without this precaution the operation would, I believe, often fail, and fail solely from this cause: too much attention, therefore, cannot be paid to this point. A person who had not witnessed it would scarcely believe how completely a few delicate fibres embracing its neck, but hardly distinguishable from the sac itself, will prevent the reduction of a hernia. A cause slight as this certainly operated prejudicially in the second case which I have related in this communication. From the investigation I have made on the subject, it is possible, I believe, in many cases, to remove the constricting or condensed matter from the neck of a hernial sac, without injury to the sac itself. I have seen this to be the case even when such substance has existed in the form of a band or bands, and been so closely adherent as to appear at first that it was an alteration in the tissue of the peritoneum itself. The proceeding will of course protract the operation a little; but the additional time will have been well spent, if it save the necessity of opening the sac: and if the sac should be wounded in doing it, the patient will be no worse off than if it had been intentionally divided in the first instance. And here let me ask, why should surgeons be on all occasions so anxious to see the contents of the sac in a case of strangulated hernia? What surgeon is there who would hesitate to return by the taxis, if it were in his power, any hernia, however protracted the period of strangulation? And why should the mere division of the stricture render it indispensable that the hernia should be seen?

I have heard it said, and very lately, that the opening of the sac does not add much to the patient's danger. But, in answer to that I would inquire, is it a matter of trifling importance, not only to expose, but to roughly manipulate, as is too often the case, a large portion of inflamed and exquisitely tender intestine? Again, let me inquire, would a surgeon, having a clean cut wound, of two or three inches in length, through the parietes of the abdomen, *the peritoneum being uninjured*, consider himself in danger? And, on the contrary, who would not feel that he was in the greatest danger, if at the same time his *peritoneum were wounded* to any thing like the same extent?

Perhaps some will say, that in returning the hernia unseen there may be greater danger than would arise from dividing the sac. I would reply to that by saying, that in the one case the danger is contingent — in the other certain.

I shall not on this occasion discuss the subject further: indeed I have only gone thus far into it, because I know, from observations I have heard them make, that there are many able surgeons who still decidedly object to the operation, and object to it entirely, I believe, because they have not given it sufficient consideration.

I am, sir,
Your obedient servant,
E. A. Lloyd,
Assistant-Surgeon to St. Bartholomew's Hospital, and Surgeon to Christ's Hospital.

Bedford-Row, March 11, 1836.

NON-EFFECT OF MARRIAGE ON THE DURATION OF LIFE.

To the Editor of the Medical Gazette.

Sir,

The present age seems to be distinguished from former times by an inordinate appetite for discovering what is new and surprising; and this, aided by the modern habit of printing and circulating all such novelties without much consideration, becomes an obstacle to the steady progress of knowledge, which

must alway depend on knowledge already acquired,—and is disturbed and retarded by every imaginary discovery of error in that which may have previously obtained universal credence in the scientific world. Hence it behoves every man who thinks he has made a discovery subversive, or even derogatory of received knowledge, to announce it with diffidence, or not at all, until after mature consideration.

I cannot but think that Dr. Casper has offended against this salutary rule, in his "Essay on the Influence of Marriage on the Duration of Human Life," recently published in the *Annales d'Hygiène Publique;* an essay which you have noticed in your journal, and in such manner as to call for that degree of examination which such an astonishing announcement must undergo, before it can be admitted into the storehouse of ascertained facts.

For all the labour and all the arithmetic which has been accumulated for the purpose of establishing tables of mortality, is sadly depreciated, if, by a mere change from celibacy to marriage, which depends on the will and pleasure of individuals, the duration of human life is prolonged in any such degree as you have stated, on the authority of Dr. Casper. If well founded, every couple intending to marry, or recently married, are able, by purchase of an annuity at the market price, to profit unjustly at the expense of the unmarried part of the community; and the evil of such injustice is of the greater magnitude as being incapable of remedy, and permanently subversive of the vast benefit to civilized man derived from the knowledge (or, must I now say, the *supposed* knowledge?) of the average duration of human life throughout all the respectable classes of society, who are now encouraged, by means of life-assurance offices, to provide for their families, to live without anxiety, and to die in peace.

In England, for instance, I thought we had arrived at reasonable certainty of the increasing duration of human life throughout the last century, and almost at absolute certainty of its present value; the three tables of modern life, inserted in your journal (vol. xvi. p. 585), having been founded on various and extensive materials, and by persons far from acting in concert with each other; yet the insurable (usually insured) ages in these tables differ not from each other, beyond such fractional parts of a year, as for practical purposes are of no importance whatever. But this mighty structure is now in danger of demolition; and in defence of it, I must endeavour to convince Dr. Casper that he has advanced an exaggerated opinion of the superior duration of married life. Previously to the publication of such an opinion, he might have extended his thoughts farther than to mankind—to those domestic and civilized animals which approach nearest to man in intellect, and, in all the attributes of our physical nature, in flesh and blood, in bone and muscle, betray no difference whatever. Over the power of generation in some of these animals—the dog and the horse, for instance—man possesses, and has exercised, despotic sway in all ages of the world. The females of these animals (all such as deemed valuable) have been permitted to produce progeny, or to remain barren, at the will of their owner; and yet, after millions of instances, affording obvious comparison, it has never been suspected that a mare lives the longer from having produced colts, or a female hound, pointer, or spaniel, from breeding and suckling numerous litters of puppies: in the latter case the animal is rather supposed to be exhausted, and old age accelerated.

But if the life of a brood mare has never yet been supposed to surpass that of barren mares, how much more convincing is the case of nine males in ten of this species of animal, who are deprived of generative power soon after birth, and yet are never supposed to lose or gain duration of life as compared to the entire male of their own species. All things combine in proving to us that nature is replete with a plastic power of accommodation beyond our comprehension, and that a vast variety of treatment and of habit is compatible with the well-being of every animal; so that in our own case—in the case of man—we encounter no dissent from the proverb, "that custom is a second nature."

Speculation has always been afloat, as to the injurious or beneficial effect of marriage, child-bearing, and its consequences, on female life, and suffrages have been divided on this question; but Dr. Casper attributes superior longevity to the married man as well as to the married female; although, after dis-

serting at some length upon the moral and domestic arguments which he thinks make for or against his own opinion, as regarding both sexes *(le pour et le contre)*, he arrives at no decided preference. But he cannot seriously attribute such absolute chastity, or excessive sexual indulgence, to the generality of unmarried males, as to affect the usual duration of life in the mass; so that his opinion must be weighed and valued according to the old-fashioned evidence of facts; and of these he has produced none, except those which appear in the Essay of Deparcieux, under the title of *État des Morts* published by M. Languet, parish priest of S. Sulpice, in Paris. This meritorious pastor (whose name is preserved from oblivion in the *additional* Essay of Deparcieux) recorded during thirty years (1715 to 1744) no less than 48,540 burials, with the ages of the deceased, distinguishing the unmarried, married, and widowed: but the ages of the deceased prove absolutely nothing of the current rate of mortality, unless we also know the number and ages of the living at the same time and place, and of the several classes distinguished in the S. Sulpice statement of mortality.

I do not presume to incroach upon the patience of your readers in proving that the number of the living is requisite for ascertaining the rate of mortality, which can only be obtained by dividing the living of any given age or condition by the number of the same given age or condition who die annually, or in any longer given period; and of this nobody is more fully aware than Dr. Casper, who, having said that 31 unmarried males *(garçons)* die, while only 3 (or 2·8 more exactly) of the married die in a given period, goes on to say, "but this difference is merely in appearance, since few men become husbands under 30 years of age*;" and thus if there are 10 or 11 existing bachelors *(garçons)* to 1 married man, the actual mortality of both classes (as above stated by Dr. Casper), is the same; moreover, Deparcieux assigns reason for an unusual preponderance of unmarried young men in the parish of S. Sulpice, as containing many large *hotels* (town-houses) of the nobility, who kept numerous retinues of male servants. But no such startling disparity as 10 or 11 to 1, really appears in the mortuary register of S. Sulpice, wherein the burials of bachelors *(garçons)* of 10 to 30 years appear to have been 1110; of the married, *(hommes mariés)* 180; of widowers, *(veufs)* 3; so that the 1110 divided by 183, gives no more than six to one. In plain truth, Dr. Casper has deceived himself and his readers, by exhibiting a per centage calculation, where such calculation is inapplicable, and could not fail to produce an erroneous result. In this manner he has been led to state the burials of the unmarried as compared to the married, (30 to 45 years of age) as 27 to 18, or 3 to 2; whereas the actual difference is quite adverse to his argument, being 975 to 1232; as three to four in favour of the *unmarried*. For the sake of brevity, I subjoin a comparative view of the per centages of Dr. Casper, and the actual number of burials in each period of human life, as distinguished in the S. Sulpice register:—

Epoque de la Vie.	According to Dr. Casper.		According to the S. Sulpice Register.		
	Hommes non Mariés.	Hommes Mariés.	Garçons.	Hommes Mariés + Veufs.	H. M. et Veufs.
From 20 to 30 years	31·3	2·8	1110	180 + 3 =	183
— 30 to 45 —	27·4	18·9	975	1205 + 27 =	1232
— 45 to 50 —	18·7	30·2	664	1851 + 111 =	1962
— 60 to 70 —	11·5	20·9	410	983 + 375 =	1358
— 70 to 80 —	7·5	18·2	267	651 + 535 =	1186
— 80 to 90 —	3·0	7·8	108	192 + 315 =	507
— 90 to 100 —	0·5	0·9	12	6 + 15 =	21

* "Mais cette différence n'est qu'apparente, puisque peu d'hommes se mariaient avant trente ans."

I must here mention that as Dr. Casper (consistently with the scope of his argument) has included widowers with married men, as having had the benefit of marriage, I have inserted a total of the two corresponding columns in the S. Sulpice register accordingly; and as Dr. Casper produces no other distinct authority in support of the superior longevity of this combined class of males, I may here close the question as to the male sex, merely repeating, that a mortuary register unaccompanied by enumeration of the living, furnishes no basis on which to calculate the rate of mortality; moreover, that Dr. Casper has misinterpreted the document on which only he relies; so that I avoid asking you to incumber your pages with a female table answerable to the foregoing table of male life, and dismiss for the present the S. Sulpice register.

But regarding female life, Dr. Casper produces two other authorities, and I am sorry that to the last of these (that of M. Biches) I have no opportunity of access; but this is of the less importance, as Dr. Casper says he has abstracted from it a table *analogous* to that from the S. Sulpice register; so that the table of mortality thus extracted from the materials furnished by M. Biches is of no value, whatever may be the value of the facts recorded by the last named author.

I now recur to the table of Dr. Odier, who has calculated for himself, and unquestionably with perfect good faith; for at 60 years of age he finds the expectation of life of married and unmarried females to differ but half a year, (13·02 to 12·53) and at higher ages the advantage is alternate and inconsiderable. These facts are somewhat inconsistent with the superior longevity ascribed to the married female. Moreover, it was customary on the Continent, and long after the date of Dr. Odier's publication, (which appeared in the year 1814) to calculate the expectation of life without reference to increase or decrease of population; so that the causes suggested by Dr. Odier for the longevity of married females, and the commentary of Dr. Casper upon them, are somewhat gratuitous. But we must always speak respectfully of Dr. Odier, who has deserved well of the scientific public by his laborious and intelligent classification of the registers of Geneva and its suburbs.

Half a century earlier (in 1766) M. Muret undertook researches in the Pay de Vaud, with a special view to the comparison of female life, of the married and unmarried female; and he published the result in a Memoire, which sufficiently establishes the acuteness of its author, who yet laboured under the same defect of inattention to the movement of population; as is proved by his tables, which exhibit the annual rate of mortality to be different from the expectation of life at birth, representing the first as at 1 in 40, the last as 35 years, in ten parishes or districts of the Pay de Vaud*. Yet the Memoire of M. Muret is highly worthy of the attention bestowed on it by our own countryman, Mr. Milne, in his discussion "on the difference in the rate of mortality among the married and the single," in his Treatise on Annuities and Life Assurances, pp. 762–781, in which it appears that the radical error of M. Muret (additional to that already mentioned) arises from his supposing that all females who are married between 15 and 20 years of their age, are married equally (one-fifth) in each of these years; whereas very few girls are married in their 16th or 17th year, as compared with the number who marry in the three next years of life; and if we may suppose none to have married in their 16th or 17th year, M. Muret's mortality of the married must be increased as 5 to 3; and a much less correction than this is sufficient to invalidate all his conclusions.

What, then, shall we finally conclude as to female mortality? Does it really differ in favour of the married, or of the unmarried female? It is not a little remarkable that Deparcieux, who is so confidently quoted by Dr. Casper, as maintaining the superior longevity of married females, has furnished the best materials for negativing the question,—materials, indeed, no longer renewable, because dependent on the existence of monastic orders, male and female; but so unimpeachable and particular, that every individual is pursued through life, as in a Life Assurance, or a Government Annuity Office,—forming, indeed, the only unquestionable part of Deparcieux's justly celebrated Essai; for the lives of his tontinists being in classes,

* This difference of five years would result from such a moderately increasing population as might double itself in about 100 years.

may be diversely considered and investigated, and from the details furnished in his Essai, have been subjected to correction, according to the views of various authors who have since treated on the expectation of human life. But with regard to his monks and nuns, he had to calculate on a collection of single lives, and his results are not liable to contradiction. I subjoin a comparative table, not different from the essential part of his large Table XIII. but condensed (for the convenience of your pages) by combining, in an average column of monks, all those which are inserted by M. Deparcieux in four columns. I must also premise, that he supposes tontinists to lose 186 in 1000 of their number before they arrive at the age of 20, 814 then remaining alive; and this being also the time of life when the religious of both sexes usually took the vow of chastity, the comparison is instituted at that age, and is continued throughout life in the following table:—

	1.		2.		3.		4.		
Ages.	Tontine Annuitants. — Both Sexes.	Decrements.	Monastic Orders. — Males.	Decrements.	Monastic Orders. — Females.	Decrements.	Monastic Orders. — Both Sexes.	Decrements.	Ages.
20	814		814		814		814		20
		40		32		31		32	
25	774		782		783		782		25
		40		31		32		31	
30	734		751		751		751		30
		40		37		37		37	
35	694		714		714		714		35
		37		42		38		40	
40	657		672		676		674		40
		35		46		45		46	
45	622		626		631		628		45
		41		57		44		50	
50	581		569		587		578		50
		55		71		57		64	
55	526		498		530		514		55
		63		77		68		73	
60	463		421		462		441		60
		68		90		88		89	
65	395		331		374		352		65
		85		98		88		93	
70	310		233		286		259		70
		99		97		95		96	
75	211		13		191		163		75
		93		78		88		83	
80	118		58		103		80		80
		70		42		68		54	
85	48		16		35		26		85
		37		14		23		19	
90	11		2		12		7		90
		11		2		10		6	
95	—	—	—	—	2		1		95
						2		1	

From the extreme scarcity of M. Deparcieux's Essai, it becomes proper to describe the three first columns in his own words:—(1.) *Ordre établi sur les listes des tontines de* 1689 *et* 1696—(2.) *Ordre établi d'après plusieurs religieux, morts depuis* 1685 *jusqu'au milieu de* 1745—(3.) *Ordre établi d'après plusieurs religieuses, mortes depuis* 1685 *jusqu'au milieu de* 1745; and for the same reason, I may be permitted to add, that Deparcieux's Observation on An-

nuitants is founded on 5911 tontinists of 1689, and 3349 of 1696; in all, 9260 annuitants: his observation on monastic orders is founded on 9970 religious; the annuitants, as well as the religious of both sexes, being mostly inhabitants of Paris.

It will be manifest from the foregoing table, that until the age of 50 (when the consequences of marriage on the female may be said to cease), the survivors of the religious (both sexes) who live in celibacy, outnumber the annuitants, most of whom, no doubt, were married persons; after the age of 55, the surviving annuitants outnumber the female religious until the age of 90; at and after which, survivorship is in favour of celibacy. The effect of this alternate superiority cannot be exactly represented, otherwise than by a table of the comparative expectancy of life at all the specified ages; and I subjoin such table accordingly,—substituting, as more convenient, *decimal* parts of the year, in place of the *months* used by Deparcieux:—

Ages.	Annuitants. Both Sexes.	Religious. Both Sexes.	Religious. Males.	Religious. Females.	Ages.
20	40·25	39·16	38·16	40·17	20
25	37·17	35·64	34·61	36·67	25
30	34·08	32·08	31·00	33·17	30
35	30·92	28·54	27·42	29·67	35
40	27·50	25·10	23·95	26·25	40
45	23·92	21·73	20·55	22·92	45
50	20·42	18·43	17·36	19·50	50
55	17·25	15·35	14·46	16·25	55
60	14·25	12·46	11·67	13·25	60
65	11·25	9·96	9·09	10·83	65
70	8·67	7·65	6·88	8·42	70
75	6·50	5·66	5·07	6·25	75
80	4·67	4·10	3·62	4·58	80
85	3·17	3·35	2·79	3·92	85
90	1·75	2·79	2·33	3·25	90
95	—	1·00	—	2·00	95

It is manifest, on consideration of the two foregoing tables, that celibacy and marriage are so equally compatible with the duration of human life, that the calculations of insurance offices remain equally valid whether the individuals insured are married or unmarried; and the same salutary truth is applicable to the annuities granted by government; nor is it less satisfactory to perceive that nature is so constant and so powerful in her purposes, that the comparative expectancy of life in the male and female religious, differs in favour of the latter in the same degree as in secular life. Deparcieux, perceiving that religious life has an advantage as far as the 45th year of life, afterwards a disadvantage, endeavours to explain it by the selection of novices, who are not suffered to take the vows unless they are perfect in bodily health and organization; but after they have been fifteen or twenty years in a convent, he thinks they begin to experience the unfavourable effect of too frequent fastings, of nocturnal hours of devotion, and of voluntary discipline and austerities sometimes excessive,—reasons which will excite the smile of those who have imbibed the popular disbelief of strict regularity among the monastic orders, which yet was in fact seldom violated; but the effect of the many amusing fables of misconduct imputed to monks and nuns is irresistible. It really seems to result from the severe inquiry into this subject in the time of Henry VIII., that irregularity was not uncommon in the smaller communities, but that the abbeys and large priories of both sexes were well governed, and irreproachable.

I will not close this letter without giving a view of married life, of the comparative numbers of the married and unmarried; and this appears, on a large scale (in the Spanish enumeration of 10,541,221 persons, in 1797), to have been in the following proportions:—

	Males.	Females	
The unmarried (*solteros*)	28	28	Per cent. on the population.
The married (*casados*)	19	19	
The widowed (*viudos*)	2	4	
	49	51	

Having relied so much on M. Deparcieux's facts in refutation of Dr. Casper's Essay, I am bound to notice the opinion (not a decided opinion) of the former, in favour of married life, as quoted by Dr. Casper, and founded on the statement of mortality in the parish of S. Sulpice*. But this statement was not published until the Essay of M. Deparcieux was in the press (*sous la presse, et bien avancé*); and the latter was thus betrayed into hazarding a hasty suggestion, concerning which I shall take the liberty to quote the unanswerable remark of Mr. Milne (page 771 of his Treatise), than whom no person has mentioned the labours of M. Deparcieux with more respect:—

"The statement of mortality in the parish of S. Sulpice shews that the number of deaths of bachelors above 20 years of age was a little more than half the number of husbands and widowers who died above that age; while the number of bachelors who died above the age of 90, was but one-seventh of the number of husbands and widowers who survived the same age.

"And the number of maids who died above the age of 20, was nearly one-fourth the number of wives and widows who died above that age; while the number of maids who died above 90, was only one-eighth the number of wives and widows who survived the same age.

"Hence M. Deparcieux inferred, that of the persons of both sexes who survived 20 years of age, those who married lived longer than such as remained single. But this conclusion does not necessarily follow from the premises.

"To illustrate this, let us suppose that all who were born in one place, which we will call the country of the single, removed at their marriage into another country, which, therefore, we will call that of the married: then it will be evident that in the country of the single, a less, and in that of the married, a greater proportion of the living who had passed their 20th year, would be above the age of 90, than if the population had been stationary, and not affected by migration.

"Therefore, although the law of mortality in both these countries had been the same, of those who died above 20 years of age, a less proportion would be past 90 in the country of the single than in that of the married. So that the above statement from the register of S. Sulpice does not enable us to draw any satisfactory inference with regard to the effect of matrimony upon longevity, nor even to compare the mortality among the married to that among the single."

On perusal of what I have written in this long epistle, I am persuaded that I shall not incur the displeasure of Dr. Casper, or of any other lover of truth, by my foregoing examination of his Essay. May I venture further to hint to yourself, as a public journalist, that the announcement of such an incredible discovery should have been delayed until you had satisfied yourself of the validity of the facts and of the calculations adduced by Dr. Casper.

I remain, sir,
Your most obedient servant,
JOHN RICKMAN.

March 5, 1836.

* Dr. Casper is not satisfied with it: "Le savant (Deparcieux) dit seulement, incidemment, et sans pousser ses recherches sur une différence si frappante." But no such alarming difference existed until it was created by Dr. Casper's erroneous calculation.

STATISTICS OF THE ST. MARYLEBONE INFIRMARY, FOR THE YEAR ENDING DEC. 1, 1835;

With Observations.

By John Clendinning, A.M. M.D.

Fellow of the Royal College of Physicians; Fellow and Hon. Sec. Royal Medico-Chirurgical Society; and one of the Physicians to the Infirmary.

[Concluded from p. 935.]

But the difference between the late year's results at the Marylebone Infirmary and those of former years, however considerable, is by no means equal to what I find to obtain between the Infirmary and other medical charities in this city, of a more general nature. I have been favoured, through the kindness of various friends, with statements respecting St. George's, the Middlesex, the Westminster, and the London Hospitals, embracing a period of twelve months, ending December 1, 1835; from which it appears that the highest ratio borne by the deaths to the admissions, in any of them, for the last year, was about 10 per cent., while the lowest mortality I observe amongst them—viz. 7 per cent.—is full one-half less than that of last year at the Infirmary. The following table contains all the particulars to be depended on, that I have been able to procure, respecting the working of those great establishments for the period mentioned; and a perusal of that table will not only define the problem—viz. the relatively high mortality of the Infirmary—but also give the solution. I have appended, for facility of comparison, analogous statements from the Marylebone, Pancras, and St. George's Infirmaries. *(See next page.)*

Now I imagine the explanation of the superior mortality of the Infirmary may be gathered easily enough from the preceding table; especially from columns 2, 3, 4, 6, and 7.

From column 2, or Ratio of Deaths by Phthisis to other Deaths, it appears that the highest mortality by consumption, of those hospitals, was last year 13·33 per cent., instead of 33 per cent. as at the Infirmary. Now of this difference in the ratio of phthisical cases, there can be no cause other than differences in the relative facilities of admission into the hospitals in question. The general hospitals usually decline admitting cases of confirmed consumption, or else discharge them after a few weeks or months of unsuccessful treatment, from a feeling that the paramount duty of those establishments is to relieve the sufferings of as large a number of the sick poor as their domestic and pecuniary resources will admit of, and that the parochial Infirmaries are the proper asylums for such hopeless cases; while, on the other hand, the latter institutions are maintained for the express purpose of sheltering all destitute sick persons, having legal claims on their parishes. The practical statistical effect of this difference in the facilities of admission respectively, is, that in the records of the general hospitals, the disease which produces from 20 to 25 per cent. of all the deaths in this country, furnishes but a small item; whereas the parochial infirmaries present even more than a full proportion of phthisical mortality: the excess, however, being easily explained by the necessarily impoverishing effect, amongst the working class, of a very debilitating and very tedious disease.

From column 3, we see the greatest loss of patients above 60, in the general Hospitals, was 13 to 14 per cent., instead of 35 per cent., as at the Infirmary. No. 4, again, which at the Infirmary amounted to 9 per cent., was 2-3rds lower at the London, and 9-10ths at the Westminster. Now the greater intensity of mortality, or less tenacity of life, of very young and of aged sufferers, from sickness of whatever kind, is tolerably notorious. Mr. Finlaison's Report of 1829 is well known, at least its results; and the views of Mr. Rickman and Mr. Edmonds are familiar to the readers of the medical weekly journals: the latter gentleman, in particular, has very recently and very ably, as it appears to me, elucidated the question respecting the influence of age on the mortality of sickness, in the pages of the Lancet.

Nos. 6 and 7 show the great predominance of surgical cases in the general hospitals, and, on the contrary, the prevalence of medical cases in the infirmaries, where preference and selection are wholly excluded; and they show, at the same time, though indirectly, the more grave character of medical cases, whose average mortality appears considerably

COMPARATIVE VIEW OF THE MORTALITIES OF SEVEN GENERAL HOSPITALS AND PAROCHIAL INFIRMARIES OF LONDON,

For the Year ending December 1, 1835.

	Ratio of Deaths to Admissions.	Ratio of Deaths by Phthisis to all other Deaths.	Ratio of Deaths of Persons above 60 to total Deaths.	Ratio of Deaths under 5 years to total Deaths.	Ratio of Fatal Accidents to total Accidents.	Ratio of Medical to Surgical Cases.	Ratio of Medical to Surgical Mortality.
London Hospital	1 in 11= 9 per cent.	1 in 9= 11 per cent.	1 in 7·50= 13·33 per cent.	1 in 30·75= 3·25 per cent.	1 in 11= 9 per cent.	as 2 to 3	as 11 to 9
St. George's Hospital	1 in 10= 10 per cent.	1 in 24= 4·25 per cent.	unknown.	unknown.	1 in 10= 10 per cent.	as 5 to 9	as 11 to 7
Middlesex Hospital	1 in 14·33= 6·66 per cent.	unknown.	unknown.	unknown.	unknown.	as 2 to 3	as 7·25 to 5
Westminster Hospital	1 in 10= 10 per cent.	1 in 7·50= 13·33 per cent.	1 in 14= 7 per cent.	1 per cent.	1 in 8·33= 12 per cent.	equal	as 7 to 3·75
St. Marylebone Infirmary	1 in 6·66= 15 per cent.	1 in 3= 33 per cent.	1 in 2·90= 34·50 per cent.	1 in 11·25= 9 per cent.	unknown.	as 11 to 8	as 8·25 to 1
St. Pancras Infirmary	1 in 7·66= 13 per cent.	unknown.	1 in 2·66= 37·50 per cent.	1 in 14= 7·20 per cent.	unknown.	as 4·66 to 1	unknown.
St. George's Infirmary	1 in 4·25= 23·50 p. cent.	1 in 3·20= 33 per cent.	1 in 3·11= 32 per cent.	1 in 19= 5·25 per cent.	unknown.	as 10 to 9·16	as 11 to 1

Note.—The ratios in the table have been deduced from corresponding totals, obtained from official sources, and now in my possession, but which I have thought it unnecessary to publish, and have therefore not included in the table.—J. C.

to exceed that even of surgical accidents. It is a law not sufficiently known, I imagine, that the mortality of general hospitals is, *cæteris paribus*, directly in the ratio of the prevalence of medical cases, and inversely, up to a certain point, in the ratio of that of surgical cases, relatively to the total of cases admitted. To explain this by a few facts, I subjoin the following table, extracted from the *Resumé des Comptes Moraux*, for 1822:—

	TOTAL ADMISSIONS.		TOTAL DEAD.		RELATIVE MORTALITIES, MEDICAL & SURGICAL.
	Total Medical.	Total Surgical.	Medical Deaths.	Surgical Deaths.	
La Charité........	2933	867	515	80	Medical mortality= 19 per cent. Surgical mortality= 10 per cent.
Hôtel Dieu........	8807	2722	1471	219	Medical mortality= 16 per cent. Surgical mortality= 8 per cent.
La Pitié	4070	1142	580	53	Medical mortality= 14 per cent. Surgical mortality= 4·80 per cent.

It appears also, from the same authority, that the mortality of the Hôpital des Enfans Malades, which contains 550 beds, was as follows:—On a total including those continued from the preceding year (viz. 417) = 3509, the medical mortality was 1 in 3·41, and the surgical ditto but 1 in 7·10.

Now it is obvious, from the largeness of the numbers in the preceding tables, and the absence of any predominant professional influence in the administration of the Parisian hospitals, and, indeed, from obvious *à priori* considerations, that the results of that table are not, *by any accident*, different from those of any similarly constructed tables of London hospital results.

1. The *à priori* consideration is the obvious one, that medical diseases are principally lesions of vital parts; of the functions of the great viscera of the head, chest, and abdomen, and of the crasis of the vital fluids: whereas surgical diseases proper, are, for the most part, injuries or defects of parts external, and not absolutely and immediately necessary to animal existence.

2. The total beds of those three hospitals is 1900 (a number equal to four-fifths of the beds of the eight principal hospitals of this city), and the total of in-patients considerably exceeds 20,000; an amount little inferior to that of the in-door cases of the same eight London charities.

3. Then the admissions into the Parisian establishments named in the table, includes all cases, in whatever stages, and without distinction; with the exception of young children, lunatics, incurables, (in the broadest sense of that term), venereals, and lying-in women; for each of which classes the French government has provided a peculiar asylum, under the same administration with the hospitals in question; as appears from late Nos. of the Journal Hebdomadaire and Gazette Médicale.

In like manner, for this island, I may cite, in proof of the superior gravity of medical diseases, the Report of the Royal Infirmary of Glasgow, for 1827, the only one at present at hand; which gives the medical mortality about 11 per cent., and the surgical 7 per cent. only; and a statement for the last year (ending December 1st, last), in my possession, from which it appears that the medical mortality was 10·66, while the surgical mortality was only 4·80 per cent., at the Middlesex Hospital.

If I had succeeded in procuring sufficient data to enable me to include St. Bartholomew's, St. Thomas's, and Guy's Hospitals, in the table of London hospitals, I should further have to insist on the large proportion of venereals they contain, as a cause of their relatively small mortalities. St. Bartholomew's contains, I understand, 130 beds for venereals, and St. Thomas's about 100; so that probably, in each, not less than from 15 to 20 per cent. of their annual admissions consist of venereal cases. Now, from the "Recherches Statistiques," published by order of Mons. De Chabrol, in 1821, it appears that for ten years, ending with 1813, the average mortalities of several of the principal hospitals of Paris was as follows:—Hôtel Dieu, 1 in 4·93; La Pitié, 1 in 5; La Charité, 1 in 7·80; while, for the same period, the mortality of the Hôpital des Vénériens was, for adults, about 1 in 60, and for infants only considerable—viz. 1 in 2·50: and, from a statement with which I have been favoured from the Grosvenor Place or Lock Hospital, it appears that, for the year ending 1st December last, the deaths recorded on a total of 445 cases were but 3. The four general hospitals from which I have been able to procure adequate statements, admit few or no venereal cases, and the parochial infirmaries have comparatively few applications on account of venereal disease. The number of venereals in the Marylebone Infirmary is under 100 annually.

In addition to the preceding causes of inferior mortality, and consequently of apparently more successful treatment in the general hospitals, as compared with parochial infirmaries, which my data have enabled me to identify, there are others of whose existence I have no doubt, but which I am unable to estimate in numbers with any approach to accuracy. I allude more especially to the more advantageous condition of the subjects admitted by general hospitals in point of age. The salaried, and therefore practically responsible, officers of those charities are not required by their employers to keep such official records as will serve the purposes of the statistical pioneer of medical science; there is usually no sufficiently accurate account of the trades, duration of sickness, diseases, &c. of the patients, kept in the official journals; and even the ages are often neglected. This last is an especially important item in the account, and should never be omitted. Owing, however, to my inability to procure sufficient data, I have not been able to contrast the table of ages of cases admitted into the Infirmary, with similar tables from the general charities, and have been prevented from in that way accounting numerically for a considerable share of the excess of the Marylebone mortality above those of the general hospitals. I shall, therefore, merely extract a few particulars from the records of military medical practice, for the purpose of shewing what is apparently obvious, yet is, I imagine, less known, or more frequently overlooked, than might be expected, viz. that the results of medical practice depend much on the ages of the sick treated; and that, consequently, those medical institutions that receive the largest proportion of inmates between puberty and old age must, if tolerably administered, necessarily give the largest proportion of discharges, and least of deaths; while the results of treatment in such as, giving shelter to all sorts of cases, receive a larger proportion than do our general hospitals, of very young and of aged persons, must, however administered, present a relatively high mortality. Every one knows more or less of the privations and hardships of military life on actual service, that they far exceed any to which peaceable citizens are exposed; also, that the improvidence of the soldier is quite puerile; yet, on a total of nearly 340,000 cases admitted into the general and regimental hospitals in the peninsula, in the course of thirty months, ending June 20, 1814, the mortality was, Sir James Macgrigor informs us, but 18,513, or about 1 in 18·30; that is, more nearly two-thirds than one-half less than that of the Marylebone Infirmary for the past year, or 5·50 per cent. instead of 15 per cent.

In like manner as to the navy, from which, likewise, very young and aged persons are excluded, it appears from returns published by Sir G. Blane, that on a grand total of admissions, amounting to 106,631, the mortality was but 1 in 14·60, or about 7 per cent. Now there is no way of accounting for the bulk of the difference between the civil and military mortalities above stated,

but by referring to the known superior tenacity of life, and elasticity of health, of young or middle-aged adults, as compared with immature or decayed constitutions. I know no reason for supposing military medical officers more skilful than civil medical officers; and though they were more skilful, I know of no reason for believing that such superior skill would make much difference in the relative aggregate mortalities of civil and military practice. The waste of human life is notoriously owing principally to causes over which medical practitioners have professionally little if any control—to indolence or excessive labour, to gluttony or deficient nutriment, to atmospherical vicissitudes, to noxious effluvia, to mental depression, to peculiar predispositions to disease, natural or acquired, and to organic deterioration by lapse of years. And the utmost effect that could be reasonably expected from even an universal medicine, long established in general use, to imagine such a chimera for illustration's sake for a moment, would be, not the prolongation or restoration of youth at will, but the elevation of the average duration merely of life, to some threescore and ten, or perhaps fourscore, revolutions of the seasons. J. C.

Wimpole-Street,
March 1836.

FALLOPIAN TUBE PREGNANCY;

WITH THE POST-MORTEM APPEARANCES.

To the Editor of the Medical Gazette.

Sir,

The following case of Fallopian tube pregnancy is one affording many curious and mysterious points for consideration, and, I think, worthy of a place in your valuable periodical. It will not appear surprising the patient's sufferings were not ascertained during her life. Many circumstances arise in practice which makes it highly desirable for a professional man to give a correct diagnosis and prognosis of the nature of the malady under which his patient may be labouring. I was compelled, in the present instance, however unwillingly, to acknowledge my ignorance of the nature of the disease: I never before witnessed a similar complaint. This case clearly shows how necessary it is for medical men to be guarded in their opinions. I have purposely omitted the treatment, as it could not possibly be interesting, and confined myself to the history of the case. As there were several rumours afloat as to the cause of this young woman's death, after some little delay I obtained permission to examine her body. It was my opinion, from the commencement of her illness, that there was something unusual in it. In consequence of which opinion, I requested the presence of Dr. Turner during the post-mortem examination; and he witnessed every stage of the dissection.—I am, sir,

Your obedient servant,
William Robbs.

Grantham, Feb. 26, 1836.

Ann Wright, aged 21 years, of spare habit of body, had been labouring under cold for some days. On the 5th of February, 1836, she was much worse. The bowels were costive; she had considerable fever, and pain of the head. In the morning she had taken some antibilious pills, which did not operate. I ordered her a saline aperient, to be repeated frequently till the bowels were freely relieved. On the following day she was much better, but appeared low-spirited, and presented a dejected countenance. My attendance was hastily requested on the afternoon of the 9th. On my arrival, she was seated on a chair in her bedroom, crying out most violently with pain in her belly. As she was seated in a dark part of the room, I requested her to rise and walk to the window; this she declared herself unable to do, or even to stand upright. At this time her pulse was very small, frequent, and irregular. The surface of her skin was cold. She appeared writhing under the most agonizing pain. The bowels were costive, and she was harassed with frequent vomitings. The catamenia had been regular, and there existed no reason to suspect she was pregnant. I carefully examined the belly, supposing at first she was about to miscarry; the state of the abdomen from touch appeared natural, but rather flatulent. The pain was not increased on pressure. There existed great anxiety of countenance.

The next day she was greatly relieved from the pain of the belly. There was tenderness over the region of the sto-

mach, and she had been sick several times during the night. The bowels had not been evacuated; the pulse had become more full and regular; the tongue was covered with a cream-coloured fur. On examining the abdomen, it appeared rather larger than natural, but she experienced no inconvenience from pressure, except about the scrobiculis cordis. On the morning of the third day of the attack the bowels had been relieved, and she appeared much better. My attendance was requested at five o'clock in the morning of the 13th of February, the fifth day of the attack. She was lying on her back in bed, with the knees slightly drawn upwards, and crying out of violent pain of the stomach and lower part of the belly. The breathing was very rapid, and performed with great labour. On my asking her questions, she replied with difficulty. The pulse was feeble and irregular; the surface of the skin was cold, and bedewed with cold perspiration; the difficulty of breathing kept increasing, and she expired about nine o'clock.

I examined the body forty-eight hours after death. Nothing unusual was to be observed externally, except the abdomen appeared rather larger than natural. On inspection, the hymen was found wanting, and the os uteri appeared near the orifice of the vagina. I proceeded to open the cavity of the abdomen, it having been the seat of all her sufferings. On a division of the integuments, the stomach, liver, and omentum, were respectively brought into view: the former was distended with gas, and the latter appeared of a very dark colour. As the incision was continued, a large quantity of fluid and coagulated blood was made manifest. At this stage of the dissection, I removed two wash-hand basinfuls of blood, and then carefully sought from what source the hæmorrhage had taken place. I commenced my examination from the œsophageal extremity of the stomach, and continued it downwards to the pelvis. This latter cavity was filled with coagulated blood. On removing this, a tumor was observed, about the size of a moderate apple, situated in the neighbourhood of the right ovarium, and partially covered with coagula (some having been previously removed). By carefully stripping off the remaining coagula, I found the seat of the hæmorrhage. There was a rupture in the tumor previously observed, and in its centre was a perfect fœtus. Having proceeded thus far with the examination, I thought it advisable to remove the whole contents of the pelvis, for a more minute dissection. This was easily accomplished. The bladder and its ureters were in a natural condition. The body of the uterus was larger than is natural in the virgin state.

The ovarium on the right side was healthy, and presented several "vesiculæ Graafianæ," in a very mature state. The Fallopian tube on this side was pervious and natural. The ovarium on the left side was rather more than half the size of the right. It contained a vesicle, filled with about half a drachm of pellucid fluid. The other part was in a healthy state, and presented nothing unusual. The Fallopian tube on the left side was shorter than the opposite one; it appeared as if the fimbriated extremity had grasped the ovum, and the latter to have partly passed down the tube for about half an inch. The tumor, or impregnated ovum, had attained the size of a common apple. I distinctly exposed the fibres of the Fallopian tube, expanded over its surface. The parietes of the tumor in the neighbourhood of the rupture were very tender, and easily broke down under pressure. The contents of the ovum were a fœtus of about three or four months' gestation, umbilical cord, placenta, and its membranes. The blood-vessels in the parietes and adjacent parts were of large size, and very numerous. The tumor was attached to the ovarium by the medium of the peritoneum. I endeavoured to inflate the Fallopian tube, from the part expanded over the ovum to the womb; at first I thought I had succeeded, but, on a more careful examination, it was found to be the peritoneum. I now opened the cavity of the uterus, and this appeared more capacious than it would have been in a natural state. It contained a membranous substance of a mucous character, which, however, did not line the whole internal surface. The mouth of the womb was open, and the internal surface was highly vascular. I now endeavoured to inflate the Fallopian tube from its uterine extremity; but this I was unable to accomplish, owing to a complete obliteration of the passage of the tube for more than an inch. A most careful dissection of this part took place, and

there did not exist the slightest passage (for an inch) from the uterus to the fimbriated extremity. The tube at this part appeared very solid, and of a distinctly fibrous character; and in fact there seemed to be no remains of a passage having ever existed at this particular part.

Observations.—The foregoing case is one highly interesting in a physiological point of view. From the appearance of the affected Fallopian tube, I should be induced to believe the obstruction was of no recent date, there being no signs of any inflammation or other disease having affected it, or the adjacent parts. But did this obstruction exist before pregnancy had taken place? I am inclined to think it did. And if the Fallopian tube on the impregnated side were impervious, how was fecundation effected? I believe this woman had advanced twelve or fourteen weeks in her pregnancy; and consequently there had been sufficient time for adhesion of the lining membrane of the tube to have taken place subsequent to impregnation. It would be highly interesting, therefore, to know how long a time elapses after a fruitful coition, and the reception of the impregnated ovum into the body of the uterus. It appears improbable that inflammation of the lining membrane, or thickening of the parietes of the tube, should have been so immediate and severe as to have prevented the descent of the ovum. Dr. Blundell has made many interesting experiments, which go to prove that impregnation cannot take place, without the semen come in contact with the rudiments, and he therefore considers, when the ovum lodges either in the Fallopian tube, the peritoneal cavity, or in the ovarium itself, and is there impregnated, that the semen must be conveyed to these parts. If Dr. Blundell's theory is correct, it would appear impossible for fecundation to have been effected subsequent to the obliteration of the tube; and therefore that it was consequent on the changes which take place during coition. This appears to be the more probable supposition, and that my opinion of the disease of the tube having pre-existed, was incorrect; but I came to the conclusion from the appearance of the part on a minute dissection.

I am fully aware there are a variety of opinions as to the manner in which fecundation is effected. Dr. Davis, in his excellent work on Obstetric Medicine, gives us the following theory, and which appears very plausible. "I hold it most probable that, in the first place, a definite time is required for the various fluids of each sex, which seem to be emitted into the cavity of the uterus after a fruitful intercourse, to be more intimately admingled, and brought into a state of mutual agency. When the period of this preparation is complete, the mature and intimately blended fluids become animated by the nisus formativus, by which the hitherto unformed materials of generation are partly moulded into the elegant coverings of the ovum, and partly into the figure of the contained embryo." From this it appears Davis considers that a certain time is required for the admixture of different fluids in the body of the uterus. Is it not probable in the present instance, that a certain time elapsed for this preparation to take place, not in the body of the uterus, but in the ovarium itself; and during which period a change might have taken place so as to prevent the natural descent of the impregnated ovum? If such were the case, we can easily imagine, from the ovum increasing in size, that the parietes of the tube would become thickened, and the passage liable to be obliterated.

TWO CASES

OF

HYDROCEPHALUS.

To the Editor of the Medical Gazette.

Sir,

If you think the two following cases worthy of publication in your Gazette, I shall be obliged by your inserting them at your convenience. I give them to you as copied from my case-book.

I am, sir,
Your obedient servant,
T. Marsh,
Surgeon, &c.

Coleford, Forest of Dean,
Gloucester, March 7, 1836.

The infant (aged about ten months) of a poor woman named Beach, residing in the Forest, was placed under my

care, in the Dispensary, some time ago, suffering from hydrocephalus. The mother told me she had had a very severe labour; her child's head was unusually large when born, and had been increasing in size ever since. Upon examination I found the head considerably enlarged, the fontanelles widely extended, pupils dilated, oppressed pulse, great drowsiness, but sleep disturbed, coldness of the feet, and evident dimness of sight; added to these symptoms the pulsation at the temporal arteries was strong, though slow; and if I might judge from that, and the pain which I produced by raising or moving the head to either side, the meninges of the encephalon, as well as the brain itself, was in a state of high inflammation. Though it appeared a hopeless case, I could not abandon hope whilst life remained, and recollecting the maxim of Celsus,

"Satius est anceps auxilium quam nullum,"

I proposed to the parents, as a last resort, and the only one likely to be beneficial, to suffer me to perform the operation of paracentesis. After some hesitation they consented.

I performed the operation in the common way, cutting through the integuments with a knife, and then evacuating the fluid (which had evidently accumulated upon the surface of the brain, although very probably there was a small quantity in the ventricles) with a trocar. The fluid was straw-coloured, and measured about twenty-four ounces. Immediately after the operation I bandaged the head as firmly as circumstances admitted, and, as quickly as I could, applied the ung. hydriod. potassæ; kept the bowels regular with calomel and mild aperients, and though the little sufferer was some time in a precarious state, he did recover, and is now a very fine boy, never having had the slightest return of the complaint.

Case II.—The latter end of the year 1834, I was called to the wife of Humphry Lugg, (one of the men belonging to Messrs. Pearce and Allamay's tin-works, at Lydbrook-in-the-Forest). I visited her immediately, and found she was in labour of her sixth child; the pains natural, and common presentation, the head first engaging with the pelvis. For 12 or 15 hours the pains were very violent, and produced little effect: anticipating a case of great difficulty, and my patient at best an invalid, I sent her husband to Coleford, about 5 miles distant, for the instruments; before he returned, however, nature *(the safest of accoucheurs)* had accomplished the work. The infant was apparently dead, but as there was evident pulsation in the funis, I used the warm bath, &c., and the child recovered. Its head, which at the birth was unnaturally large, rapidly increased in size; and as the parents strenuously opposed any operation, all remedies were tried, and all failed; the poor child lingered in a most distressing state *ten months* and four days. For the last four months of its life, the measurement varied but little from the following, which I took in the presence of a grandson of the late justly distinguished Mr. Hey, of Leeds.

From radix nasi to protuberantia occipitalis, 26 inches; fronto-occipital circumference, 32 inches; from ear to ear, across the vertex, 24 inches; round the chin, and across the vertex, 30 inches; length of corpse, 19 inches; circumference of neck, $4\frac{1}{2}$ inches; fluid, 154 ounces.

The friends would not permit the slightest post-mortem examination, only allowing the evacuation of the fluid by the trocar. The bones of the skull seemed to be obliterated; and I have little doubt, if the child had lived another week, the integuments would have given way.

Having given you these cases, may I ask the following question:—Why are the optic nerves commonly relaxed in hydrocephalus? does it depend upon an increased quantity of fluid in the ventricles, or arise from some interruption to the natural functions of the brain?

ATMOSPHERIC INFLUENCE IN THE PRODUCTION AND MODIFICATION OF DISEASE.

To the Editor of the Medical Gazette.

Sir,

In this variable climate of ours, every one is constantly talking of the weather, and of its influence over the healthful functions of the human body; and so

far as the changes of temperature are concerned, every body is agreed in ascribing a due portion of complaint to atmospheric influence. It is also generally admitted, that the changes of season, also the extremes of heat and cold, of moisture and of dryness, are productive of diseased action in the human system; and also that marshy situations alter the condition of the atmosphere, producing marsh miasm or malaria, and generating diseases, varying according to the country or climate in which they occur. These instances of atmospheric influence are obvious, and such as can be recognized by the senses, or proved to a demonstration; but I apprehend there are other changes taking place in the atmosphere, productive of not less important results than those already enumerated, but of so subtile a nature as to be wholly impalpable to our senses, and which are only to be recognized from certain effects being produced, which can only be accounted for by their agency—such. for example, as the simultaneous appearance of some disease over a large district or a whole kingdom, as in the instance of influenza extending over great part of Europe at one and the same time. Another example I may mention, on a more limited scale, was the appearance of puerperal fever, in the year 1812, in all parts of the counties of Northumberland and Durham; and every medical man must have witnessed the sudden out-breaking of continued fever, or of small-pox, measles, or scarlet fever, where none existed before; and if further evidence is wanting, I would just remind my medical brethren how often it happens, when we meet with one case of any disease not of a contagious nature, how generally it is followed by several similar ones. I have repeatedly known several cases of apoplexy or of abortion occur in quick succession, and after a little time the tendency to these has passed away for an indefinite period.

Atmospheric influence does not appear to be confined alone to the production of disease, but it seems also to modify the type of a disease already established. Of this I will state an instance. In the winter of 1818—19, typhus fever prevailed in this district to a great extent; the treatment found to be the most successful was bleeding and mercury, together with purgatives and diaphoretics. It diminished during the summer, but increased again in the succeeding winter: but its character was changed; it would no longer bear depletion: bark, wine, and ammonia, were the appropriate remedies. Every practitioner of a few years' standing must have witnessed similar instances, where diseases have occurred, in which the ordinary remedies were either altogether inapplicable, or where it became necessary to employ them with great circumspection. This was remarkably the case after the invasion of cholera. Even in cases of acute inflammation it became necessary to bleed with great caution; and the same held good with regard to purgative medicines. Nor can it have escaped observation, that periods occur in which there is a greater tendency for complaints in general to terminate fatally than at other times, many deaths occurring within a short space of time.

If I have succeeded in proving the probability of atmospheric influence being the proximate cause in the production and modification of disease, and that certain states and conditions of the atmosphere favour the development of certain diseases, it would then appear that the germs of these diseases are always present, but in a latent state, until such atmospheric changes occur as call them into activity. If this is admitted, and I see no reason to doubt it, it then becomes a matter of grave importance to inquire what is the nature of these conditions of the atmosphere which give activity to individual diseases. It is a subject of somewhat difficult investigation, but I think much useful information might be gained were every medical man to observe and record the weather and temperature, the moistness or dryness of the atmosphere, in connexion with prevailing diseases, and the mode of treatment he found the most successful. In this way such a mass of medical statistics would gradually be acquired, as might ultimately produce very important results in the knowledge and treatment of disease.

I am, sir,
Your obedient servant,
EDWARD GREENHOW, M.D.

North Shields, March 12, 1836.

MEDICAL INSTITUTIONS OF GERMANY.

BAVARIA — MUNICH.

To the Editor of the Medical Gazette.

SIR,

I BEG to forward you some further particulars respecting the Bavarian medical institutions, which may interest many of your readers.

The hospital at Munich is one of the best I have seen on the Continent. Its interior organization might serve as a model. It was erected in 1813, and occupies a quadrangle, which encloses two large court-yards. The wards (fifty-four in number) are of the same size, each containing ten or twelve beds; they are extremely clean and well ventilated, and are paved with square pieces of marble; as are also the spacious corridors into which they open. Between every two wards is a small apartment, containing every thing required in ordinary use, and a plentiful supply of warm and cold water. They are all kept at the same degree of temperature, by an apparatus for heating air at the top of the building. The operating theatre is commodious, well lighted, and supplied with bandages and dressings of all kinds, ready for immediate use. The baths are convenient and in good order, though baths are much less frequently prescribed in Germany than in France. The office of nurses is performed, both in the male and female wards, by fifty *sœurs de la charité;* as is also the case in the Austrian dominions. There are two physicians and a surgeon, who visit the patients every morning before nine o'clock. There is also a physician who has the exclusive charge of the syphilitic patients; seven or eight assistants accompany by turns the principals in these rounds: three of these reside in the house for two years, and receive 300 florins annually. The four principal medical attendants receive about 600 florins each per annum. Besides the public wards, to which all are admitted on application, there are—a ward for students of the University and of the Academy of Arts; a male and female ward for persons of the Jewish religion; and about twenty rooms, neatly furnished, for the accommodation of as many patients, each of whom pays a florin a day, and has the exclusive attendance of a nurse.

Syphilitic patients, those with psora, and other cutaneous diseases, and those with diseases of the eyes, have separate wards appropriated to them. The ophthalmic wards are four in number, each containing six beds: attached to this department is a small theatre, where all operations on the eyes are performed.

The hospital is supported, like most others in Germany, by contributions from the town, and a trifling tax on servants and others of the inferior classes. The number of patients in the house generally averages about three hundred. There are no out-patients. Those who die are examined in the new anatomical building erected a short distance from the hospital. The amphitheatre, for anatomical lectures and demonstration, is commodious, and capable of containing 300 spectators. The dissecting-rooms are also convenient and clean. This building contains the pathological museum, which is small, but possesses some valuable preparations. Pupils dissect here with much more care and neatness than I have observed elsewhere on the Continent.

Munich also contains military hospitals; a madhouse (which is in very bad order, and can only be considered as a place of detention); a foundling hospital, and *maison d'accouchemens.* This latter has about sixty beds; of which one-half are for pregnant women, who are obliged to work at spinning, knitting, &c. five or six hours a day. Women are allowed to remain nine days after their delivery, and are obliged to take their infant away with them. The wards of this building are low and indifferently ventilated; too many beds being placed in one apartment. In the clinical ward, lectures are delivered by the professor of obstetricy of the University, and accouchements take place. Midwives attend labours in all ordinary cases. About fifty women are annually instructed in midwifery, and are obliged to undergo an examination before obtaining a license to practise. Between four and five hundred women are annually delivered in the hospital. Inflammation, and other unpleasant consequences, are said to occur but rarely, and the mortality is exceedingly small.

The University has not been established many years, but already ranks high as a school of medicine, several of

the professors being persons of the highest reputation and attainments. The number of students formerly amounted to 2000; but owing principally to political circumstances, and the restrictions imposed upon the admission of foreigners, there are at present not more than 1200, of which about one-third follow the medical classes. There is no fixed order of study, and the examinations are much less rigid than at most other universities, although diplomas are never granted to foreigners without examination, for which a candidate may present himself after three years' study. At the expiration of this period, the student undergoes the examination termed *rigorosum*, which consists in written and verbal questions and answers, and the performance of a surgical operation before the examiners, who are professors at the University. The subjects of this examination are anatomy, pharmacology, general and special therapeutics, surgery, and midwifery. The candidate is also required to write and defend a thesis, and to publish a Latin dissertation; after which, if approved, he receives the diploma of Doctor of Medicine or Surgery; which, however, does not authorize him to practise until he have passed two other examinations. The first of these takes place two years after the *rigorosum*; during which time the candidate may travel to other countries, or practise under the direction of an established physician. This examination, termed the *probe-relation*, is held before a committee of professors, and also consists of verbal questions and written propositions, with the performance of any operation selected by the examiners. The last examination takes place in October of every year, at which period from forty to sixty candidates present themselves; five questions are proposed, to which written answers are made, and, according to the manner in which the candidate acquits himself, is he placed in the first, second, or third class of practitioners. The first class practise in Munich, or in some other principal town of Bavaria. The second class are mostly restricted to country practice; and the third to thinly-populated districts, or, if in towns, to the management of the slighter cases of disease. The expense of attendance on lectures, and examination fees, amounts to about 500 florins, including the preparatory studies on physic, chemistry, natural history, botany, and mineralogy. The professors most highly estimated, are the well-known Walther, who fills the chair of clinical surgery and ophthalmology; Döllinger, professor of anatomy and physiology; Ringseis, medical clinique; Buchner, pharmacy and materia medica; Weissbrod, midwifery; Vogel, chemistry.

Should you think these communications worth inserting in the Gazette, I will, in my next, transmit a sketch of the state of medical and surgical practice.

EDWIN LEE.

Munich, March 2, 1836.

GALEN, ON FEIGNED DISEASES.

"HOW TO DETECT THOSE WHO FEIGN DISEASE."

[THE following translation of Galen's tract—Πῶς δεῖ ἐξελέγχειν τοὺς προσποιουμένους νοσεῖν—is, we believe, the first that has ever been made in English. Nor are we aware of any other modern version of it, except one in German, by Metzger, published in Pyl's Repertorium. It is the oldest treatise extant, expressly devoted to a medico-legal subject; and, taking into account the period at which it was written, will probably to many appear, however imperfect, sufficiently curious and interesting. The author, it will be recollected, flourished in the second century of our era *.]

Persons feign illness for many reasons. Now, in all such cases, the discovery of the truth seems to be of the physician's province, although ordinary individuals often fancy themselves competent to the task. To the medical practitioner, certainly, it belongs to distinguish and discriminate that sort of phlegmon, erysipelas, and swelling, which are

* The present is not the only subject connected with legal medicine that Galen has treated, or at least touched upon. In his celebrated work, *De Usu Partium*, he has pointed out the peculiarities of the heart and lungs of the fœtus, which serve to distinguish them from those of the child that has breathed and lived for a time. But, as compared with other parts of medical science, he has said so little on medico-legal matters, that his very silence is significant. That so great a master of the learning of his age, and who wrote so much, should have never once alluded to the importance of medicine as applicable to the proper administration of justice, is fatal to the supposition of those who labour to prove the antiquity of legal medicine.

purposely produced by external applications, from those which have their origin in the complaints of the body itself; also, to mark the difference between blood spat from the interior of the mouth, and that which originates in the œsophagus, stomach, or organs of respiration: for persons have been detected who had the power, whenever they pleased, towards the end of a cough, to spit blood—some small vein about the gums being conveniently ruptured for the purpose, and kept in readiness, so that at pleasure, by sucking it with the tongue, blood was discharged upon coughing, as if it came from the chest or abdomen.

Again: individuals have pretended madness and folly, while they have been only making fools of other people. It is in cases of this kind that medical men are generally allowed to be the fittest persons to ascertain the truth.

It often happens that where a malady is feigned, intolerable suffering is pretended. I once knew a man who, on being summoned to a public assembly which he did not like to attend, simulated a painful colic. I suspected at the time that he was feigning, in order to escape the meeting; and he afterwards confessed to me that such was the fact. I ordered him fomentations; but he was not very anxious for assistance, though at other times the least illness used to alarm him. He was aware, besides, that only a few days before, I had speedily relieved a patient from colic by a dose of *philonium**: a medicine which he certainly would have asked for, had he really been in agony. But the event showed the nature of the case: as soon as the public meeting was over, the complaint ceased—there was no longer any pain. The mode in which this person lived previously was also calculated to throw light on the circumstances: his diet was not of a kind to produce colic; for disorders of this description usually arise from indigestion and chills (ψύξεων), neither of which had happened in the present instance. It was therefore a matter for medical experience to decide (taking into account the manner of living which had been antecedently practised) that there was here no real disease. The suspicion that the business of the meeting might lead to some trick, I by no means claim as owing to *medical* sagacity; it was the mere exercise of that *reason* which is said to be common to all, though few have it in so practised and perfect a state as to be ready on every emergency for what is best to be said and done. Let it accordingly be remembered, that if, along with our medical experience, we possess this intellectual power, we have the means of detecting many similar and gross deceptions.

A case of the latter kind occurred to myself. The pretending sufferer was a slave, of that class who have to perform journeys on foot, running by their masters' side. He complained of excruciating pain in both his knees. I immediately suspected a trick, as his master was to set out on a journey that day, and I was aware that the young man was not very honest, or scrupulous about the truth, in matters of this description. Besides, I inquired of a fellow-slave of his, who knew him intimately, whether the patient was not amorously attached to some female, on whose account he was desirous not to leave home, when his master was about to travel a considerable distance? And this proved to be the fact: so that here was detection by extraneous means (ἔξωθεν). But one of the knees was immoderately swollen, so as easily to alarm a non-professional person: the eye of experience, however, could see that all the ailment present was caused by the application of *thapsia**. And

* This was a famous electuary of the opiate kind. It was called Philo's antidote, after Philo, of Tarsus, its inventor, who lived, it is supposed, in Augustus's time. The composition of the Philonium, described in Greek elegiac verses, is preserved and explained by Galen, *De med. comp. sec. loc.* lib. ix. 4. The terms of the recipe are enigmatical, and may amuse some readers; we give the substance:—"Take of the yellow and fragrant hair of the divine Crocus, whose blood glitters in the fields of Mercury, as many drachms as a man has senses; of the Eubœan Nauplian, a drachm; of the slayer of Menœtiades, as preserved in the bowels of sheep, the like quantity; add twenty drachms of white flame, and twenty of the bean of the wild animal of Arcadia; a drachm of the root (falsely so called) which grows in the land famous for the Pisæan Jove; take twice five drachms of πιὸν, written with the masculine article prefixed; and mingle all with the production of the daughters of the bulls of Athens." Galen interprets this curious medico-poetical farrago, which without his aid would certainly be not a little obscure, as implying the admixture of saffron, pyrethrum, euphorbium, white pepper, hyoscyamus, spikenard, opium, and Athenian honey. It is, moreover, stated in the verses, that the pains for which this μέγα εὕρεμα was most serviceable were those of colic, of the liver, dysuria, and stone.

* Pliny, copying Dioscorides, mentions *thapsia* as a species of *ferula*: see his account of the virtues of the plant, which he conceives to be virulently poisonous, and to be meddled with

here was detection peculiar to *medical* sagacity; to which it also belonged to decide that nothing had previously happened capable of raising such a tumor so suddenly; for the person had not run of late to any excess, nor had he been beaten by any one; nor had he been dancing; nor in jumping over any ditch had he hurt himself; neither did he exhibit any appearance of too full a habit, for he had not lived either luxuriously or intemperately previously. Further, when I came to question him concerning the sort of pain he felt, he neither answered promptly nor consistently. His master being now gone, I administered an application which had no efficacy whatever in the allaying of pain, but simply the virtue of cooling the quality of *thapsia*. In the course of an hour he confessed that the pain was gone. Yet, had his suffering been caused by real phlegmon, so far from being relieved by the cooling remedy I gave him, he would have been only rendered worse.

Thus it is evident, that if medical skill be combined with ordinary sagacity, the reality or groundlessness of alleged pain may be precisely determined.

But from the very manner itself in which the malady is endured, a judgment may sometimes be formed that the pain cannot be very severe; for those who really suffer under a painful affection are almost invariably agitated in divers ways, according to the severity of the ailment,—a thing which those who practise deception do not take sufficient care to observe. Those also who labour under excruciating pain are ready to adopt any means of relief; they even exhort their medical attendants to do whatever may seem best to them for the cure of their disorder. But persons who only suffer a little, or none at all, carefully eschew all such aid, not being particularly fond of low diet or active treatment; and we should do well probably to tell them, that for such complaints as they say they labour under, there can be nothing better than scarifications, the cautery, and abstinence from drink and food of the very sort to which they are known to be most attached.

There are other medical observations, connected with actual pain of a severe nature, which may serve to detect impostors,—such as exhaustion, coldness of the extremities, paleness, sometimes cold perspirations, and the irregularity of the pulse (which, indeed, happens even to those who suffer very little pain), the pulse being often more frequent in moderate than in severe suffering, and under obscure than strongly-marked symptoms.

Further, when a feigner is asked about the kind of pain he feels, he always describes it as settled in the part which he says is affected; whereas, it is to be borne in mind, that this is not invariably the case with respect to really severe pains, some of them being felt extensively over the adjoining parts, while others are fixed and confined to a particular locality: some, for example, throb as if struck with blows; others seem to tear the parts asunder; others are of a benumbing quality; others bear down; while others, in fine, are attended with vomiting, disturbance of the belly, and obscurity of vision*.

only with great caution, Hist. Nat. lib. xiii. c. 43. Matthiolus gives a good figure of the thapsia of Dioscorides; it is one of the *umbelliferæ*, and its English synonym (acc. Loudon) *deadly carrot*. Hippocrates employed thapsia, in the shape of a very diluted extract, as an emetic; in the treatment of diarrhœa, he gave the leaves with honey; and externally, mixed with cerate, the root of thapsia was used as a discutient—for the removal of ecchymoses.—*Dierbach: Arzneim. des Hippoc.* The irritating or inflammatory quality of thapsia is well known to the people of the southern shores of the Mediterranean at the present day. Poiret, in his *Voyage en Barbarie* (t. ii. p. 138), mentions that he saw an Arab rub his cheek with *thapsia* (*T. villosa*) for the cure of ringworm: the part became swollen in a few hours, and was greatly inflamed. Dierbach thinks that the thapsia of the ancients was *T. garganica*, a native of Calabria, the north of Africa, and Greece: Sprengel, however, calls it *T. asclepium*, L.

* It deserves to be added, that the five general rules laid down by Zacchias for the detection of feigned diseases, and which, as Dr. Beck says, "are so discriminating as to have received the sanction of most succeeding writers," are all deduced from the principles contained in the above tract. Dr. Beck, in giving an abstract of the rules, commends them, on the score of "the universality of their application, and the ingenuity of their author." Galen, however, and not Zacchias, seems to have the best right to be accounted their author; for Zacchias himself says, in introducing them, "*Ego illius auctoris* (meaning the author of the tract) *doctrina munitus, et rationabili discursu adjutus, dicendum censeo, quod quinque potissimum ingeniis deprehendi possint ii, qui morbum aliquem se pati simulant,*" &c.: and the reader will at once perceive, on considering the rules in question, that they are all illustrated in Galen's treatise, as given above. Their heads are briefly these:—1. To look to the possible motives of the suspected party; 2. To the likelihood of the alleged disorder—taking into account the patient's previous habits and mode of life; 3. The disposition to be relieved; 4. The consistency of the attendant symptoms; and, 5. That of the consecutive or secondary symptoms.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

On the Analysis of the Blood and Urine, in Health and Disease; with Directions for the Analysis of Urinary Calculi. By G. O REES.

THERE is probably no source whence pathological discovery is more likely to flow than animal chemistry. The field it presents was long comparatively neglected, but now begins to attract a larger share of attention. In this country we have hitherto been mainly indebted to Dr. Prout, who has united the characters, too seldom found together, of chemist and practitioner; but within a very recent period several others have appeared in the same department, and with every prospect of advantage to themselves and to the profession. It was but in our last number that we inserted the concluding portion of an elaborate and very valuable history of urinary deposits, by Mr. Brett, with whose attainments as a chemist the readers of this journal have been for some time familiar. Another claimant to attention is before us in Mr. Rees, and we can safely recommend his present volume, as calculated, by its perspicuity, simplicity, and accuracy, greatly to facilitate an acquaintance with the more important practical parts of animal chemistry. The methods of performing both quantitative and qualitative analyses of the blood and urine are described in a manner, which even the uninitiated cannot fail to understand, while the processes are economical, and require patience rather than skill in conducting them.

The detection of foreign principles in the urine has received much more attention than as regards the blood, yet no greater difficulty attends the examination, and we can scarcely hesitate to admit that their detection, while still poisoning the frame by their presence in the circulating mass, must be of very great importance to practical medicine. On this point Mr. Rees makes some judicious observations, and points out, that the medical profession has, till very lately, devoted so little attention to the analysis of the blood, that we have but few observations of value regarding its diseased condition. This may account for the fact, that as yet our practical improvements have not kept pace with our increasing knowledge of pathological chemistry. It is to be hoped, however, that as more physicians than heretofore become practical chemists, this will cease to be the case.

The most interesting change with respect to the blood is where it contains principles which belong to the secretions, but which, from some morbid cause, cease to be eliminated: the most remarkable of these are urea, the colouring matter of the bile, and cholesterine. We extract Mr. Rees' account of the methods of detecting these; by which it will be seen how true is the remark which he makes with respect to the fact of care and patience being the chief requisites in conducting the necessary manipulations:—

"*Examination of Blood supposed to contain Urea.*

1. Let a portion of serum destined for analysis be accurately weighed, and then evaporated to dryness over an open steam-bath.

2. A quantity of distilled water (amounting to about one ounce for each 200 grains of serum used for experiment) is to be heated to 200° Fahrenheit, and then poured on the dry extract, which must be previously broken up with a sharp spatula.

A digestion over the steam-bath for about half an hour is now to be performed; the loss of water by evaporation being supplied occasionally by the experimenter.

3. The digested fluid is to be filtered, and the residue on the filter washed with distilled water (the washings being added to the original liquor). The whole of the filtered liquor is now to be evaporated to dryness over an open steam-bath, and the residue of the evaporation digested, with a considerable proportion* of absolute alcohol, at a gentle heat, for half an hour; care being taken that the loss by evaporation do not materially diminish the bulk of the fluid.

4. A second filtration is now to be performed, and the filtered fluid must again be evaporated to dryness, and then dissolved in a small portion of lukewarm distilled water. We thus

* About eight times the bulk of the solid extract for digestion.

procure an aqueous solution of urea, combined with animal extractive; to this solution (previously evaporated to the consistence of a thin syrup) we now add a few drops of nitric acid, which causes an effervescence. This mixture must be set aside to crystallise.

5. Should crystals appear, of the peculiarly characteristic appearance of nitrate of urea, we may conclude that urea is present: indeed, if crystals exist at all after the foregoing process, they must be nitrate of urea; since no principle of the blood that can possibly exist in the last-tested fluid possesses the property of becoming less soluble by the addition of nitric acid.

6. Crystals being formed in the liquor*, we may now proceed to ascertain the proportion of urea. For this purpose we must first allow some time to elapse, in order that the deposition of crystals may be perfectly accomplished.† When such is the case, the supernatant liquor must be poured off; which, with the assistance of a small glass rod (to retain any very minute crystals) may be perfectly effected. The acid crystals remaining are now to be carefully dried over the open steam-bath, and then weighed; from their weight we may infer the proportion of urea present, since we know the composition of nitrate of urea to be—

Urea	52·63
Nitric acid	47·37
	100·00

If it be required to ascertain the relative proportion of each other ingredient of blood containing urea, it will be right to make a separate analysis for that purpose; since the use of the open steam-bath and distilled water at 200° Fahrenheit will materially interfere with the determination of the quantity of albumen. I feel convinced that the wish to ascertain all by a single analysis has frequently been the cause of failing in the detection of urea when it existed in the blood.

Examination of Blood containing Colouring Matter of Bile.

The best account which has yet been given of this diseased condition of serum is by Lecanu, who satisfactorily proves that in jaundice the vital current is strongly impregnated with those matters, that in the healthy state are peculiar to the secretion of the liver. This able chemist has established, that in cases of jaundice, the blood contains the following principles, foreign to its healthy constitution:—

1. A combination of albumen and soda, scarcely at all soluble in water.

2. An orange-yellow colouring principle.

3. A blue colouring principle.

These colouring principles have been demonstrated to exist in the bile by M. Chevreul.

The examination of the serum of icteric blood is performed as follows:—

The serum is diluted with a considerable excess of alcohol, which renders it turbid, and precipitates a quantity of flocculi. These are collected on a filter, and washed repeatedly with cold alcohol. This filtered alcoholic solution is of a yellow colour, and possesses an alkaline reaction; it yields a dark yellow-coloured residue on evaporation, which has a saltish disagreeable taste. It is deliquescent, and almost entirely soluble in ether. The portion insoluble in the last-named menstruum is granular, and of a salt taste, without bitterness. It contains, besides salts, an extractive matter, soluble in alcohol, and another organic matter, such as is met with in healthy blood.

The ethereal solution is now left to spontaneous evaporation, when a considerable orange-yellow residue is obtained, which contains crystals of the crystalline fatty matter of the blood. These crystals may be separated by warm alcohol, which also extracts an oily matter, of a beautiful deep yellow colour.*

The albumen which has been precipitated is now to be treated with boiling alcohol, which assumes a dark green tint, and by cooling deposits crystals of fatty matter. The liquor, when cold, is to be filtered, and then evaporated over a water-bath. During the evaporation, it retains its original tint for some time; but when the alcohol is nearly dissipated, the green colour disappears, and a yellow tint is observable, while at the

* It will always be observed, in specimens where urea exists, that, long before any material diminution occurs in the bulk of the fluid (which has been mixed with nitric acid), a crop of crystals appears.

† This must be determined by the discretion of the operator, as it is impossible to lay down rules on such a subject.

* This has been lately shown by Braconnot to consist of the yellow matter of bile in combination with an oily matter.

same time a portion of brownish matter deposits on the sides of the vessel. This brown deposit may be washed with cold alcohol, to free it from the other matters; when it will be found very soluble in boiling alcohol, and capable of producing a fine blue colour when dissolved in that liquid. It is remarkable that exposure to the rays of the sun destroys this colour.

The yellow serum of jaundice may be very easily tested for bile by the addition of dilute sulphuric acid, which is capable of changing the colour to a delicate green after the lapse of a few minutes. This is a very simple method, and no less easy than it is satisfactory, and void of fallacy.

The jaundiced serum is thus described by Lecanu:—It possesses a sickly taste; it is of a saffron colour, which passes to a canary yellow on being diluted with water; it froths by agitation, and turns syrup of violets to a fine green colour.

Examination of Blood containing Cholesterine.

This principle is very easily discovered when it exists in blood; and many instances are on record, in which it has been detected in icteric serum. The late researches of P. S. Denis have convinced him that cholesterine is not a constituent of healthy blood; and it is on the faith of his observations that I class it as a substance foreign to the normal constitution of serum. I am at present engaged in a series of observations on the fatty matter of the blood, which will bear strongly on this subject, and I hope to be able to communicate the results in the Appendix. When serum is suspected to contain cholesterine, it should first be evaporated to dryness over a water-bath, and the dry residue must be digested with ether for several hours. The ethereal solution may now be decanted, and allowed to evaporate spontaneously. The residue consists of the fatty matters of blood combined with cholesterine.

These are to be well washed with cold alcohol, which extracts the oily matter of the blood, leaving the crystalline fatty matter and cholesterine. This latter may now be removed with the point of a penknife, or any fine instrument, as its crystals are very obvious and easily distinguishable."

In making these extracts regarding the blood, and in directing attention more particularly to this portion of Mr. Rees' volume, we by no means intend to imply that his account of the urine is not equally good; but having recently devoted so much space to the chemical history of the latter fluid, we are desirous of giving variety to our pages; and, indeed, by adding the methods of detecting the chief morbid ingredients presented by the blood to the processes applicable to the urine, contained in some former numbers, we give our readers, within the compass of the present volume, a little manual of animal chemistry, which we think they will find of considerable value.

Elements of Medical Jurisprudence. By THEODRIC R. BECK, M.D. Professor of the Institutes of Medicine, &c.; and JOHN B. BECK, M.D. Professor of Materia Medica, &c. Fifth Edition. pp. 1010.

THE merits of this work are so well known, that we need only mention what has been done for it by the authors in preparing the new edition. Chapters have been introduced on Policies of Life Assurance, and on Medical Evidence; the excellent tract on Infanticide, by Dr. J. Beck, has been still further improved and extended; Mental Alienation has been materially revised; and the whole subject of Poisoning has been remodelled. Christison's classification of the poisons has been adopted, and thus has one of the greatest objections to the book, as it previously stood, been removed.

The pains taken in bringing the work into its present shape—*au niveau*, as it is, with the best medico-legal information of the day—must have been prodigious: a large portion of it has been re-written, and the additions throughout, from almost every available source, are at once interesting and highly valuable.

We commend the zeal and conscienciousness of the authors, in scrupulously referring to the works (and the number of those works is amazingly great) from which they have drawn contributions: in this respect, they strongly contrast with the "compiling" pirate, whose catchpenny we noticed the week before last. In short, Beck's Medical Jurisprudence, in its present enlarged form, has been rendered quite an Encyclopædia on the subject: it is an admirable book of reference, and ought to be in the hands of every member of the profession.

MEDICAL GAZETTE.
Saturday, March 19, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."
CICERO.

THE METROPOLITAN UNIVERSITY ARRANGEMENTS.

THE rumours which are abroad respecting the appointments, said to be in contemplation, for the new University, are any thing but calculated to satisfy the professional public. What the Government really mean to do, we do not profess to know: the rumours to which we allude *may* be true; and on that ground it is that we venture to make a few observations.

The appointments which are matter of concern to all well-wishers of the projected establishment, are not those of chancellor, vice-chancellor, or any merely honorary functionaries: but those of the acting men—the personages who are to constitute the Board of Examiners, and on whose merits and respectability will depend the reputation —the success—of the whole scheme. Let us suppose, for an instant, a possible case: that the Government may be misled by the counsels of selfish and interested advisers, and in consequence are induced to nominate parties but ill qualified for the duties devolved upon them, and whose names are scarcely known to the profession at home—wholly *un*known to the medical world abroad: what may be the effect? Every one must be convinced that the whole undertaking — the long-desired consummation of founding a Metropolitan University—must, under such circumstances, prove a complete failure. Instead of possessing a great institution in London, in connexion with which would be found the names of men most illustrious for their professional merits, we should have a lumbering establishment with amateur physicians—persons never distinguished as practitioners, lecturers, or men of science — for its board of management. The result must be disastrous—the fair fame and utility of such a university will, by such a course, be smothered in the birth. Who ever cares for the honours (so called) of a university that has no great names attached to it — no truly eminent men among its conductors?

But it may be said that the choice is difficult, and that it will be hard to make the appointments at the outset, so as to please every body. Now, this we take to be the very reason why nothing should be done precipitately: at the same time that we deny the difficulty to be of a formidable nature. To be sure, if the line be permitted to be drawn by the parties already alluded to, and that they, in their short-sighted selfishness, so draw it as to exclude all others, beside themselves and a congenial few, from the executive of the establishment, it will, indeed, be difficult to rectify this unlucky stumble on the threshold. The doom of the new University will be sealed; and none of those who *might* retrieve its fortunes, fallen almost ere risen, will be willing to venture on such a forlorn hope.

We have heard with some feelings of surprise, of a deputation (consisting of three or four individuals), which lately waited on the Chancellor of the Exchequer, and the object of which was to persuade that gentleman, that in the new arrangements no person should be appointed an examiner who had any connexion with any school or hospital. Was ever known such egregious folly—if, indeed, it may not be characterised by a less reputable epithet! We wish the sapient members of this deputation would favour us with half a dozen—nay, half that half again —of names of persons coming within

their category, and whom they deem suited to the duties in question. If the hospital physician be excluded, and he who has become distinguished as a teacher of medical science, where, we should be glad to know, will a competent examiner be found? It will not, we presume, be said that the business of an examination is a mere farce: the day is gone by, in this country, when the mockery of a pretended ordeal would be tolerated; and with the efficiency of its examinations, we are persuaded, the new establishment will either stand or fall. Perhaps it is the object of the parties referred to, merely to pursue the ancient beaten track of testing candidates by the extent of their book knowledge, and to bestow honours on them in proportion to their degree of polish acquired by *grinding*. If so, let the pure and inexperienced examiners, recommended by the deputation, receive their appointments; but then let us hear no more of a University founded in the metropolis, and worthy of the present state of medical knowledge in Great Britain.

In sober sadness, it is a great misfortune to have an excellent project, like the present, marred by meddling fools or designing knaves; and never, we believe, was a grand undertaking more near being spoilt than is that of the Metropolitan University that is to be. Hospital physicians, and actual lecturers, — no matter how eminent their attainments and qualifications, — are objected to. May we, in that case, beg to be informed, how is an efficient examination to be given? By an efficient examination we mean one suitable to the present advanced state of medical science and practice: one that should be *practical* in the proper sense of the word, and not merely conducted by rote; one that should be held not only in the examination hall, but by the bed-side of the patient. Nor let it be said that this is innovation: call it by whatever name we may, the thing, however new here, is universally adopted in other countries, and most fitting to be introduced amongst us; it cannot be much longer excluded. Is it to be contemplated for a moment, that a new University, in the nineteenth century, and in the metropolis of England, is to be established, and that medical honours are to be distributed in it, without any system whereby proper discrimination may be observed? To pause for a reply were only contemptuous. The examination *must* be practical; and is it proper that, being so, it should be entrusted to other hands than the most competent?

Not only, as it appears to us, is there no alternative, but it is the most desirable proceeding, to select the medical examiners for the new institution from among the hospital physicians, and the most distinguished lecturers of the metropolis. Valid objection, we repeat, we can see none. That a man's own pupils may sometimes come before him, is a counterplea of but little force, — force, too, more apparent than real. How easily could it be so arranged that an examiner should not interfere, under such circumstances,—as has long been the custom in examining at the College of Surgeons;—and would not the very occasion call forth a feeling of impartiality, well adapted for the furtherance of justice?

So little, in short, can be urged against the propriety of having actual practitioners and eminent teachers to examine candidates, and so simple are the means of removing any possible objection, that we hesitate not strongly to recommend it as the wisest course which can be adopted.

The plan, on the other hand, of confiding so important a task to persons little conversant with practice, and wholly unconnected with the business

of teaching, is fraught with much absurdity and certain mischief. How, or where, we once more ask, can we find half a dozen men of this description fitted for the due discharge of the duty of examiners? The persons proposed, and we may say the persons talked of as likely to be appointed, can only be considered as mere amateurs in science — men deriving their professional information from books and the scanty resources of a limited private practice—men too, some of them, founding their claims to appointment, as we are given to understand, on still more questionable pretensions. With such nominations as these, should they unfortunately be made, it is utterly impossible that the professional public can be satisfied, or be otherwise than highly offended. For our own parts we would fain hope that the government will exercise a more sane discretion in the arrangements they intend to adopt. Should, however, our *fears* prove true, we can only enter our strong protest against the whole proceeding.

EFFECTS OF MATRIMONY ON LONGEVITY.

We beg to call the attention of our readers to the very interesting paper of Mr. Rickman on this subject, in our present number. The views of Dr. Casper, of Berlin, which, a short time since, made some impression upon us, and to which we gave some currency in this country by the circulation of our journal, are not Mr. Rickman's views. Our correspondent thoroughly exposes the insufficiency of the data on which the German statistician founds his inferences respecting the (supposed) longevity of married life; and matrimony is now proved to have no marked effect in prolonging the life of either man or woman. Mr. Rickman gives us a concluding hint, that *we* ought to have been more guarded in announcing the novel doctrines of Herr Casper, before we had satisfied ourselves of the validity of his facts and calculations. It will be recollected, however, that we gave them solely on the responsibility of their author: something, too, might be pleaded for us on the score of the research required for such investigations; but we can scarcely regret our having noticed the paradoxes of Dr. Casper, when, by so doing, we have elicited so able a refutation of them from the pen of Mr Rickman.

MR. TWEEDIE AND THE "FREE HOSPITAL."

In a subsequent page will be found the statement and appeal of Mr. Tweedie relative to the late proceedings in Greville-street. We must say that his case appears to us one of considerable hardship: he has been infamously treated by Franks—in which respect, however, he does not stand alone: other parties also seem to have poured out their little phials of petty malice upon him; but we doubt greatly whether quite too much is not made of the whole affair. After such testimony in his favour, and what we may call so complete an acquittal, surely Mr. Tweedie may very fairly shake the dust off his feet, and trouble himself no more about the business.

CLINICAL LECTURE

ON

CRUSHING THE STONE IN THE BLADDER,

Delivered at the Middlesex Hospital School, March 14, 1836,

By Sir Charles Bell.

I propose to-day giving you a clinical lecture on crushing the stone in the bladder. We are bound to address you on this subject by every motive that can actuate the humane mind. There is no torture which a man suffers greater than that from stone in the bladder; and there is no duty which you will have in after life to perform so oppressive, as that of the operation of lithotomy; for although in favourable circumstances it is safely done, yet, whilst any obscurity hangs over the condition of the patient, or his constitutional peculiarities, or as to the size of the stone, or the state of the bladder, it is not without danger. But independently of these reasons, I say we are bound to draw your attention especially to this subject, since the very house over our heads has been

built at the expense of those who have taken our promise to attend to the methods of removing the stone without cutting.

First, then, how stands the opinion regarding the operation of lithotomy. You may have heard patients declare that they would rather suffer the operation for the stone twice over, than bear the torture from its presence for one night. You may have heard them say that the operation of sounding for the stone is more painful than the operation of cutting.

The incisions for lithotomy, performed by a man properly educated as an anatomist and surgeon, are simply and quickly made; but in regard to the extraction of the stone from the bladder, it should be done very *very* slowly, and consequently the rapidity with which an operation is performed is not the mode of judging of the merits of the operator. There is not an authority in our profession who has not declared against judging of an operation by the time. The taking out of a stop-watch is an indication of improper education. In a late operation performed in our theatre by our assistant, I have understood that the watch was looked to. That indicates both bad teaching and bad example, and would almost incline one to believe, that these individuals had come to see the operation ill performed, instead of witnessing it carefully and well performed, in the mode they ought to imitate.

Still, gentlemen, these deep incisions, made on oneself, who can contemplate without shrinking? and therefore it is of incalculable benefit to have an operation in which these incisions are not necessary. The advantage is this, that when a man entertains a suspicion of stone in the bladder, he comes at once to his surgeon; whereas, heretofore, he would not allow himself to believe that he was so unfortunate. He lets time pass; he is unwilling even to be sounded, lest his worst suspicions should be confirmed: time passes, the stone gets to be of a large size, and then, indeed, there is danger from the operation of lithotomy. You have no notion how men shrink from the certainty that they have the stone, and linger on suffering from irritation until it is unbearable; and then, when the stone has acquired a great size, they are forced to submit, in unfavourable circumstances. Therefore I say it is of incalculable benefit to society, and to us especially, that there is an operation, simple and safe comparatively, which is offered to the contemplation of these sufferers, and which brings them earlier under cure. Let us, then, give our whole attention to this subject.

An unfortunate idea prevails, got up upon this occasion, I know not on what authority, that we have been all along under a misconception, and that the urinary bladder can bear a great deal more injury than we have imagined.

On this head I beg your particular attention. I have at various times pressed upon you the difference of sensibility in internal and external parts. I have shewn you that internal parts—the viscera—have their peculiar sensibility, and that it is totally different from that of the surfaces. But there is more than this: suppose that you are examining a patient with diseased liver, or under the suspicion that he has diseased liver. You lay him down, relax the abdomen, and press along the margins of the ribs, and feel the hardened edge of the liver. You ask him if he has pain on this pressure; he says "no." Or if, not feeling the liver, you press down the cartilages of the ribs to ascertain if there be morbid sensibility, indicative of inflammation, still he says that he suffers no pain. But by and by, when he sits down, and proceeds with the narrative of the disease, he becomes pale, speaks with difficulty, and tells you that he is now in great pain. There is a peculiarity in the sensibility of the liver different from that of the external parts; but it rises slowly after pressure. In the same way, to bring us nearer to the subject, if you are examining *per anum* the state of the prostate gland with the finger, and press all around to find if there be sensibility, or any mark of disease there, and you ask the patient—"have you pain here—have you pain there?" he says "no." But when you have withdrawn the finger, and after the examination has been finished some time, the patient begins to complain of the dull sensation which arises, and becomes at last very painful. So, again, with regard to the bladder, but more especially its neck, there is a certain pain, no doubt, felt by the patient during the introduction of instruments; but all the effect of the interference with the internal part is not shewn during your operation, but after it; so that if you are not careful, light of hand, and delicate with the patient, you are called back in the afternoon of the day, and there you find him in a paroxysm of suffering, cold and shivering, the bed shaking under him, and he is in a state most alarming to himself and his friends. Hours have passed, but the paroxysm is the indication of the injury you have in the morning committed upon the neck of the man's bladder. Do not, therefore, be deceived by this sort of new aphorism got up, that you may do a great deal more injury to the bladder without inconvenience,

than has been hitherto supposed; it is an entire mistake.

For the history of the operation of crushing the stone, you must go elsewhere. It is a long history, which I cannot undertake to follow up so as to appropriate the merit of the discovery to the right individuals. But when the operation was first performed in this country, they brought me an instrument similar to that which I now show you, but larger and stronger, and of which this might almost be said to be the model, for this is intended for a child. The instrument you perceive is straight; and here is another grand discovery, that you can pass a straight instrument into the urethra. This you certainly can do, but I fancy that, as anatomists, nobody will convince you but that there is a curve in the urethra, and although you can pass a straight instrument, you do certain violence to the curved urethra, more especially to the neck of the bladder. When I saw this instrument first, I said, "It appears to me a most dangerous instrument"; you see that it is a tube from which certain blades possessed of elasticity can be thrust out, and which, by their expansion, are prepared to seize the stone. I remarked, "suppose you get hold of the stone, and cannot break it, how can you withdraw the instrument?" The answer was very ingenious: "there is a wire to pass through this tube, which projects against the stone, and displaces it from the grasp of the forceps." That is good, it is ingenious; but suppose that we have got hold of the stone, and that by attempting to crush the stone, instead of effecting our object, the ends of the blades of the instrument bend, and are permanently expanded, what is then to be done? There was no answer to that question, except that it was not likely to happen. But it did happen:—it has happened not once, but several times. How often it has happened, and how often patients have died under this operation, and from what immediate causes, I am unable to state; and yet these misfortunes should be recorded. Several, I know, have died; but I shall speak of one case, of which I was a witness.

A much-respected gentleman had a stone in his bladder: he was unwilling, as most patients are, to suffer the operation of lithotomy, and called to his assistance the Parisian operator. He proceeded to his operation; he seized the stone, but in pressing down the instrument, the blades did not break off—that would have been of less consequence—but they became permanently expanded, and in this state the instrument was withdrawn through the prostate. (Think of the effects of withdrawing such an instrument through the prostate!) It was brought into the membranous part of the urethra, but farther it could not be withdrawn! What was to be done? There was a call for a pair of blacksmith's forceps—strong pincers; then the patient was placed as for lithotomy, the perineum was cut into, the bulb of the urethra opened, and the forceps applied to the blades to squeeze them together, in order that the instrument might be withdrawn along the urethra. Dr. Hume and Sir B. C. Brodie being in attendance upon the operation, said, "here is the incision as for lithotomy; why not proceed, and finish the operation?" They knew well, and you ought to know, and remember it, that an ineffectual operation for lithotomy, the stone remaining in the bladder, is generally fatal, because, in addition to the violence of the operation, there is permanent irritation from the presence of the stone. They determined that the stone should not remain in the patient's bladder; and so Sir B. C. Brodie, with characteristic decision and ability, immediately sent for his instruments, and performed the operation for lithotomy. It was after this period of the case that I was called in, and I saw that gentleman suffering gradual decay, in consequence of the severity of the operation—first, from the introduction of the instrument; secondly, from the operation of seizing, and the attempts to crush the stone; thirdly, from the violent withdrawal of the dilated instrument through the prostate; fourthly, from the incision into the bulb of the urethra and the formidable hæmorrhage; and fifthly, from the performance of the common operation of lithotomy. Can you wonder, then, that after lingering some weeks, the patient finally died?

So far, then, I maintain I was right in my conjecture regarding this instrument. It is a most villanous and dangerous instrument; and if you have been tempted to buy it, keep it till you grow rich, and give it to your butler to draw corks from a bottle.

With regard to the patient in the hospital, you see an old blind man suffering severely from the stone. On sounding the man, and dodging the instrument over the stone, I calculated that there was a stone about the size of a chesnut; and remarked that, if we are ever to perform this operation of bruising the stone, here is the instance; and I was prompted to think that I could do it (pardon me for saying so) with as light a hand, and with as much regard to the patient's feelings, as I had seen the operation performed. I performed the operation at three different times, and he is now discharged, well.

The operation is this:—In the first place the bladder must be injected, and for this reason, that the stone is apt to lurk

between the fleshy columns of the bladder. When the bladder is irritated by the presence of a calculus, fleshy columns arise, formed out of the fibres of the detrusor urinæ; by dilating the bladder you separate these, as it were, and force the stone out of the recesses between the columns; and then again, in catching the stone, you are in no danger of including the folds of the bladder when it is distended. That is the reason of filling the bladder with tepid water. In the next place, you lay the patient so that the stone falls a little to one side.

I have said, in speaking of lithotomy, that the surgeon who sounds dexterously will perform the operation well, because, by sounding well, he calculates the depth of the bladder, and the actual position of the stone, and he can get the stone to that recess where he is sure to strike it. If he acquire a proper notion of the position and size of the stone, the operation of lithotomy is comparatively simple. So also he that can discover the stone with little pain to the patient, is likely to succeed in this operation (not forcing in the instrument, and striking with violence against the stone, but rather, by turning the patient, bringing the stone by gravitation into contact with the instrument). I say that the surgeon ascertaining thus the actual position of the stone, will perform the operation of catching it and crushing it easily.

As to the instrument which you are to introduce to crush the stone, this which I now present to you is it. You see at once that it is a totally different instrument from that which I before showed you. There is an advantage in the curved form; it may be very easily introduced into the neck of the bladder without inflicting pain. But the greatest advantage of this instrument is its strength. This instrument is the invention or the improvement of a person who certainly will not hide his candle under a bushel; and he is quite prepared to stand his ground with any inventor in Paris or elsewhere. Notwithstanding there is a sort of conceit about him, yet I value the instrument on account of its form and strength; for I repeat, I care not so much about breaking the instrument, as the fragment can be withdrawn with the stone by lithotomy; but bending the instrument is the most formidable thing, inasmuch as we can neither let it remain nor withdraw it without a rude and painful operation. Indeed, all these instruments should be proved like a piece of ordnance before they are used. Now, observe how it is to be used. If, for example, the stone be of the size of a walnut, we must then prepare that the blades shall separate to that extent. Having introduced the instrument into the bladder, and turned it to the side on which the patient lies, and felt the stone, you slowly open the blades by withdrawing the upper one thus; and perhaps you feel the patient flinch a little. You saw that as soon as I had chucked upon the stone, it was immediately seized. There is not the slightest difficulty in this part of the operation, neither is there pain if you do not open the instrument suddenly, and to a great extent. Take time, do it delicately and nicely, and there is neither difficulty to you nor pain to your patient. During the operation, I was desirous to know what pain was inflicted by the operation. I requested the patient to tell me whether he was suffering pain. "Oh," said he, "I cannot expect to get rid of a stone in the bladder without pain." "Nay; but tell me," I replied, "how do you feel—are you suffering much pain?" "Oh, you know there must be pain." But he never winced,—never moved a muscle, never interrupted his chat,—and therefore I must presume that the man was not suffering; for if he endured pain, he must have had extraordinary fortitude neither to wince, nor cry, nor even to change his voice, but readily to converse with me during the operation.

When the stone is seized, you can move the instrument to any part of the bladder you choose. You cannot, by pushing down the button attached to the sliding blade with your finger, crush the stone; but there is a screw, having great mechanical power, and you must use it, not by turning it uniformly, but by bringing it back again, and so working it as to grind the stone gradually, and by distinct blows. By this means you break the stone down as if it were bruised with a hammer; and it is not so apt to break and fly by each successive impulse as by screwing down the sliding blade at once by the mechanical power of the screw. The stone being thus crushed, and a great portion of the fragments being brought out in the grasp of the instrument, it is so far satisfactory. It will sometimes happen that the stone will remain, choaking up the instrument, and you can with difficulty get the blades together. The meaning of this slit, which is otherwise to be regretted, as weakening the instrument, is to let the *debris* be forced through. You calculate, from the position of the sliding ring along this scale, how far the blades remain apart; and they must be brought nearer, almost together, before you attempt to withdraw the instrument. There is a part of the operation here which I like, as being ingenious; a vice having attached to it a heavy mass of metal, is screwed upon the shaft, which belongs to the further and fixed blade, and then the other shaft is struck down with smart blows of the steel hammer,

until the upper blade is so far pressed upon the lower, that the instrument can be withdrawn. What is the meaning of this appended mass of metal? There is a *vis inertiæ* in it; and accordingly, in striking the instrument, instead of endangering the bladder, the impulse is resisted by the appended mass, and you can therefore give a smart blow without any shock.

Is it not an admirable thing to bruise a stone into these fragments [handing round a bottle containing them]?—and does it not appear to you a very simple thing?

But you have not yet considered the source of pain and danger. The operation, I say, is not a painful one, but the consequences are sometimes very formidable. In the first place, there is a great uncertainty of getting the stone altogether away; there is a possibility of some fragment remaining, and if it should, there is great pain, and another stone will form upon it as a nucleus. I was consulted by a gentleman not far removed from our hospital, on whom this operation was performed. He seemed in just such a condition as the patient whom you have seen, and the operation was perhaps as easily done. There had been two or three successive operations; and the patient said, "I think I will do handsomely—I will give the operator 100 guineas." "No," said the operator; "my fee is 400 guineas!" He gave his 400 guineas; and when I sounded him some considerable time afterwards, I found there was still a stone in the bladder, and that the pain had returned, and the glairy deposit in the urine. I was quite satisfied with my one-pound-one; but still I could not help drawing a little contrast between my honorarium and that thrown into the "egregious maw" of this cormorant. I felt that it would then have been easy to crush the stone at a common visit. It is possible that in this instance a new stone had descended from the kidneys. But nevertheless, it is a fact, that with the most experienced performers a portion of the stone, like a shell, is apt to remain, and to form the nucleus of another stone. I do not consider this as a great objection to the operation, but it points out to you the necessity of not being too sanguine that you have removed every part; and it shows the necessity of again and again examining the bladder, and washing it out carefully.

There is not merely danger of a portion remaining in the bladder, but worse than this—a fragment, such as you have in that bottle, may stick in the orifice of the bladder, or in the lacunæ of the prostate, and then the suffering of the patient is beyond expression. The contraction of the neck of the bladder upon the sharp stone, the inability to discharge the urine, the spasm that takes place in the bladder, and the inflammatory condition of the prostate, are all very serious consequences, and attended with great suffering.

You can the better comprehend all this because you have seen it in my patient. You saw that after the second operation he suffered excessively, from a sort of paroxysm, which implied no more than might have taken place if a very rude hand had forced a large bougie through the prostate. But afterwards he suffered in a different way—viz. from a portion of the stone lodging in the neck of the bladder, so that he had frequent calls to make urine, and purulent and mucous discharge from the neck of the bladder.

In a private patient, whom I attended during the progress of the operation, it was necessary to introduce the catheter and inject tepid water, so as to push and wash back the fragment into the bladder, to give temporary relief.

It was on account of this patient's suffering that I got an instrument made for injecting the bladder. I did not know that this very excellent instrument [presenting it] had been provided and was in the case. I commend this to you for your adoption: it combines the means of holding a portion of the stone, with the uses of a catheter. You can introduce this and inject the bladder, and then allow the bladder to empty itself through this tube; trusting, that if there be a portion of stone remaining, it will come, by the force of the stream, into the groove, and then you can take hold of it and withdraw it. It is a most ingenious and excellent addition to the other instruments.

At one time our patient had frequent calls to make water, and discharged great quantities of glairy mucus. Those were the symptoms of the portions sticking near the orifice of the bladder. This mucus you saw tough, hanging from the pot; and in some cases it is discharged bolt out in a mass, so as to give the patient the sensation of a large body being discharged.

Such is the result of a broken portion of the stone (which is often like the broken shell of an almond), sticking and clinging to the prostate, or entering into the dilated part of the urethra which is within the prostate, obstructing the urine mechanically, and at the same time causing painful spasm.

One operation of crushing will not be sufficient; nor two, nor three; it must be repeated again and again, and it is only a small stone which you ought to attempt to crush: you will be baffled by a large stone, and you must acknowledge that when there is a large one it is a case in which it will be necessary to perform lithotomy. When there is a small stone, this operation

is safely performed, and with the best success; but, as I said, not by one operation, but by several. The suffering, I repeat, is not during the operation, but afterwards; nor must it be concealed that many have died in consequence of this operation. If I have a feeling against the operators, it is from finding there have been deaths undivulged, and not fairly put in balance when the operation has been contrasted with lithotomy.

I believe I have fairly stated to you all that has come to my knowledge—that which I have seen with my eyes and felt with my hands; keeping out of sight all that has been said in controversy: in fact, I know very little about the controversies which have been entertained on this subject.

In conclusion, then, consider the operation as belonging to your profession, and as a thing you can do with propriety and efficiency, if you know the form and position of the bladder—if you calculate the gravitation and lodgment of the stone, and if you can sound well and safely. All that I have to recommend to you is, that, having crushed the stone, you should see that the fragments are removed, and take care to wash the neck of the bladder during the paroxysm. I believe there is an instrument to slit and open the passage, and allow the stone to escape: it is highly improper to use such an instrument, and the invention of it is only declaring demonstratively the severity of the suffering from the sticking of the fragments in the passage. On the whole, this is a very important operation—a real accession to our means of giving relief: it brings patients to your hands, when you can do them essential service; it prevents them lingering and hanging off till the stone is too large for the operation of crushing, or even of lithotomy.

I look upon the ingenious inventions exhibited in these instruments [there were many on the table], as promising us means of breaking the stone in the operation of lithotomy. If the operation of lithotomy is to be limited to the cases where the stone is large, then will the character of the operation quickly decline: for there is no accident in lithotomy which, by care and dexterity, may not be obviated, but those which attend the extraction of a large stone, and hardly any contrivance or exercise of ingenuity will mitigate the evil. To do the operation without violence or tearing, is to make it certainly successful; and for this purpose the stone, if large, must be divided, not brought through entire. It is not an operation for display, but to ensure you a better reward than the approbation of a whole theatre of gentlemen, with watches in their hands.

SCHOOL OF PHYSIC IN IRELAND.

DR. MACARTNEY AND DR. LENDRICK—CORRESPONDENCE, &c.

To the Editor of the Medical Gazette.

SIR,

IN a letter addressed to you by Dr. Lendrick, he complains that the communications between the Board of Trinity College and me, during the years 1833 and 1834, have not been published. I did not think it necessary to print more of the correspondence than was sufficient to explain the facts, and account for my conduct, not calculating that any person acquainted with the history of medical education, would, in the present day, become the champion of imperfect instruction. I now send you the intermediate correspondence,—which Dr. Lendrick will find does not strengthen his case, but, on the contrary, contains answers to almost all the arguments employed in his letter.

In addition, I wish to state the grounds on which any of the hours he mentions would be unfit for my lectures on surgery. 1st. The morning hours are given up to the practice of the hospitals, which most of the pupils attend. 2d. The hour of twelve would be impracticable to the same person teaching anatomy at one. 3d. Evening lectures have been tried in Dublin by others, and myself, and relinquished on account of the irregular conduct of the pupils; and under the circumstances in which I am placed, they would be peculiarly objectionable, as the entrance to the anatomy house is through a lane, where there is a low tavern, in which several of the students have been led into scandalous excesses even during the day; and further, a night hour is unsuitable for a course of surgery, in which operations are shewn. In London the operations were performed in the anatomical course, which would be inconsistent with the Edinburgh regulations. At all events, it must be manifest that I would not have returned money, and surrendered my class into the hands of other teachers, if I could have with propriety employed any of the hours which Dr. Lendrick recommends.

If the order of the Board, of 31st October, 1834, communicated to Dr. Barker by the Provost, be enforced, the instruction on both anatomy and surgery will be perfectly useless, and will be sought for in other places than the School of Physic in Ireland. I do not impute to Dr. Lendrick any wish to injure the school with which he is connected, but I attribute to him, as

well as others concerned in this transaction, very short-sighted views of their own interest.—I am, sir,

Your very obedient servant,
JAMES MACARTNEY.

Dublin, March 12, 1836.

From Dr. Wray to Dr. Lendrick.

Dear Lendrick,—I am directed by the Board, as Registrar, to say that the only lecture given from three to four o'clock, which will be required by the Board as a qualification for a *liciat ad examinandum*, is that given by the Professor of the Practice of Medicine.

Believe me, yours truly,
HEN. WRAY.

Board Room,
November 9, 1823.

From Dr. Macartney to Dr. Barker.

Dear Dr.,—As it is probable I shall not be able to attend the meeting of the professors, for the purpose of framing the advertisement of the lectures, I have to request that arrangements may be made for allowing me the necessary number of day hours for the two courses of anatomy and surgery. If I were to abbreviate both those courses, so as to give them at *one* hour, certificates of *neither* would be received in Edinburgh; the effect of which would be not merely to injure, but destroy the school. The character of the Dublin school at present is lowered by having the course of surgery discredited on account of its brevity.

It has been hitherto thought desirable to provide as much as possible the opportunities of giving at home the education required for the medical degree. This was the very ground on which Dr. Montgomery urged his claims for having his lectures recognized. At present pupils are compelled to take in Edinburgh a course of surgery of sufficient length, because I have not been allowed an hour for the purpose here, although it is as much my duty, according to the constitution of the School of Physic in Ireland, to teach surgery in a competent manner as anatomy. I have, however, now resolved to give the courses on the two subjects of which I am the Professor, of a length consistent with the advanced state of our knowledge on both subjects; and if distinct hours be not allotted for this purpose, I must only take those which will be most convenient to the class. I send you my printed advertisement.

Yours truly,
JAMES MACARTNEY.

Lucan, Aug. 25, 1834.

From the Provost to Dr. Barker.

(Communicated to Dr. Lendrick, Nov. 25, 1834.)

My dear Dr.,—I have laid before the Board this day the *representation* of the Professors of the School of Physic, and the Board have accordingly directed the Registrar to inform the Professor of Anatomy and Surgery, that they will consider the lectures on these subjects as constituting but one course, for attendance on which the students will be entitled to receive but one certificate from the *Registrar*: also, that they will not recognize any medical lectures delivered at three o'clock, but those on the practice of medicine.

I remain, sir, &c.
B. LLOYD.

October 31, 1834.

From Dr. Phipps to Dr. Macartney.

Sir,—I am directed by the Provost and Senior Fellows to direct you to refrain from lecturing at one of those hours which have been already appropriated to the lectures of another Professor of the School of Physic.

By order of the Board,
ROBT. PHIPPS,
Registrar.

Trinity College,
Dec. 13, 1834.

From Dr. Macartney to Dr. Phipps.

Sir,—I have to acknowledge the receipt of your letter, conveying to me the direction of the Provost and Senior Fellows, that I should refrain from lecturing at three o'clock.

I am sorry it was thought necessary to take this step from information derived from *ex parte* statements; and I am convinced the Board have issued this order without knowing what consequences would result from a compliance with it. I cannot suppose that it is desired to render the certificates of the Professor of Anatomy and Surgery useless to students, by making his courses so imperfect, that they would be deservedly discredited in every institution in this country. Would it tend to the reputation or the interest of Trinity College, or of that which in the act of Parliament is named the "*Complete School of Physic*," to abolish or restrict any part of the course of medical study, at a time when in all other Universities, including Oxford and Cambridge, it is greatly improved? Would it not have the appearance of partiality (for favouritism I am bound to presume does not exist), to compel students to seek a full course on surgery in Edinburgh, when it can be had at home; while at the same time it is made imperative to attend midwifery *here*, although not given by a legally constituted Professor in the School of Physic?

Although the Provost and Fellows may

hold the anatomical and surgical courses to be so much inferior to that on the practice of physic, that they think the former should give place to the latter, or rather to the fancied interests of the Professor of Physic, yet I cannot believe that it is intended to forbid students who have no wish or intention to attend Dr. Lendrick, occupying themselves with another subject at three o'clock.

As the Board seem to be quite unacquainted with the bearings of the case, I shall state some facts for their information.

For many years I have not been able to give two courses at *one* hour each day in the week, nor can it be done without mutilating one or both subjects.

I have been in the habit of lecturing two, three, or sometimes four times in the week, at three o'clock, on surgery,—of giving no holidays at Christmas,—of prolonging the lectures sometimes into the middle of May,—and towards the end of the course lecturing three times a-day, thereby producing very great (and what perhaps might be deemed unjustifiable) inconvenience to the pupils; notwithstanding, I have been obliged to condense and abridge much of the surgical course,—as the lectures on fractures, luxations, hernia, the venereal disease; and omit altogether two very important branches—the diseases of the skin and eye—subjects which have been so much extended by modern improvements.

I lectured at three o'clock before Dr. Lendrick was elected, and was complained of by the College of Physicians, at the instance of Dr. Osborne, who was acting as *locum tenens*. When the subject was discussed at that time, the Provost advised me to persevere with my lectures, and disregard complaints from *all* quarters; and this advice I followed, so that Dr. Lendrick found me using three o'clock when he was elected. I now appeal from the Provost of 1831 to the Provost of 1832, being satisfied that I have not deserved any unfavourable change in his sentiments.

Since that period the Board have made regulations forbidding the attendance on more than *three* Professors in the same year, which gives so much latitude, that two Professors lecturing at the same hour can produce no inconvenience, except from the merest accident, which would not occur again. A student *might* attend the practice of physic during *three* seasons, but I never heard of any attending more than once.

When there is a full course of medical education, interference of hours is unavoidable; several of the courses interfere in Edinburgh and on the continent. It is not desirable that the subjects I teach should be studied the same year with the practice of physic. Anatomy and surgery ought to precede; and wherever there is a rule for education, they do precede, the practice of physic.

It is quite manifest that I gain no advantage from lecturing at three o'clock on surgery, if another hour were at all convenient to the pupils, or practicable with respect to the subject,—and it so happens that the hour of twelve, offered to me by the other Professors (apparently because it would not suit me), would answer extremely well for Dr. Lendrick, as all the lectures at Sir Patrick Dun's (except Dr. Graves') would then be consecutive, which would save to the pupils that time at lecture they now spend passing backwards and forwards.

In the Edinburgh University and the College of Surgeons, five lectures in the week are required for anatomy, and as many on surgery. The College of Surgeons of London demand as many for anatomy, and *at least* three in the week on surgery. The Apothecaries' Company require the same number for anatomy. The College of Physicians in Ireland, even from *graduates* of the University, insist upon a distinct course of surgery, such as I could not give, using only one hour in the day. The consequence of depriving me of a *second* hour, would evidently be that my certificates would cease to be qualifications; and once destroy the anatomical and surgical courses of instruction, the school would speedily fall back to the state I formerly knew it, *i. e.* empty benches in place of pupils, and three, four, or five graduations in medicine in the year.

In your letter you say that the hour of 3 was previously appropriated to another Professor. I beg to observe that the law gives no authority to appropriate or determine what hour a Professor is to use. This matter has always been left to private arrangement, it not being supposed possible that it would require the interference of higher powers, nor could there have been any difficulty in the present case, if the other Professors had been as much disposed to accommodate me as their own interests would dictate, if they could perceive them.

The observations I have made apply to the past and future seasons, but with regard to this year, no change can now be made which could benefit any party concerned. The period is gone by in which pupils can with propriety be received by *any* Professor, with the intention of inserting their names in his return to the senior lecturer, or of granting them certificates.

Yours very truly,

James Macartney.

Dec. 16th, 1831.

From Dr. Phipps to Dr. Macartney.

SIR,

I have read your letter to the Board, and am directed to inform you, that they will not require you to refrain from lecturing at the hour of three o'clock, for the remainder of the present session; but that they will not extend this indulgence beyond that period.—I am,

Your very obedient servant,
ROBERT PHIPPS, Registrar.

Trinity College, Dec. 26, 1834.

From Dr. Macartney to Dr. Phipps.

SIR,

I am sorry to find by your letter of the 26th instant, that the Board have not bestowed sufficient consideration on the subject of my surgical lectures, to enable them to arrive at the real state of the case.

The question is not whether I shall lecture at three o'clock, but whether the course on surgery is to be given at a practicable hour, so as to make it of any value to the pupils, or to be abolished, as a part of the education in the University. The language used in your letter proves that the Board have mistaken my objects altogether. You say that the *indulgence* will not be extended to me after this season, that is, the indulgence of giving *two* full courses for the price of *one*, in order to discharge the duties of a double Professorship, maintain my own reputation, and the character of the school. I asked no favour nor indulgence, and it is well I had no occasion, as it seems I would have been refused: I merely claim the right of being unmolested in the conscientious performance of my duty. It is, perhaps, looking too far forward to pronounce what is to take place next year, or how medical education may be regulated.

The Board have already come to *four* different decisions on this subject, and until the matter shall be more closely investigated than it has been yet, I do not consider it as finally settled.—I am, sir,

Your obedient servant,
JAMES MACARTNEY.

Dec. 27, 1834.

DISMISSAL OF MEDICAL OFFICERS, FOR ALLEGED QUACKERY.

MR. A. TWEEDIE'S APPEAL.

To the Members of the Medical Profession.

GENTLEMEN,

You will have observed by the newspapers, that the Governors of a public institution, entitled the "Free Hospital," Greville Street, have removed their resident apothecary, Mr. Hentsch, and one of their unpaid surgeons, Mr. Tweedie (myself), from their respective offices, on the alleged charge of having administered and recommended a nostrum called "Franks' Specific Solution."

Such a proceeding is without precedent in the medical history of this country; and I deem it not only respectful to you, but due to my own reputation, to lay before you a brief statement of the facts relating to it.

About the beginning of February, 1835, Mr. Hentsch, the resident apothecary of the "Free Hospital," mentioned to me that Mr. Franks had succeeded, by distillation and otherwise, in freeing the "balsam of copaiba" from much of its impurity and irritating properties, whereby a quantity of greenish-olive ropy sediment was removed; and the remedy so purified had been found by him to be more grateful to the stomach, and more efficacious as a medicine, than was the balsam itself before it had been so prepared. He requested me to give it a trial.

Now Mr. Franks was not at that time advertising like a quack; he was a regular medical practitioner, residing officially at the South London Dispensary, and also a Governor of this "Free Hospital;" and observing, besides, that Mr. Green and Mr. South, at St. Thomas's Hospital; Mr. Bransby Cooper, at Guy's; Sir Astley Cooper, Sir Benjamin Brodie, and other eminent surgeons, were making trial of the preparation, I had no hesitation in prescribing it publicly to my patients at the "Free Hospital."

The result justified me in speaking favourably of it; so that when, two months after this, Mr. Franks called and asked me for my written opinion, I had no suspicion of the use that would be made of it; and in imitation of the example of Mr. Green, Mr. B. Cooper, and others, I wrote and gave him, without inquiry, a professional statement of the result of my experience of his "preparation of *copaiba*."

Since these things, Mr. Franks, judging, no doubt, that more emolument would result to himself from selling what *he* chose to make a quack medicine, than from the orthodox pursuit of his profession, has taken out a patent for his "solution of *copaiba*," and has published our letters, mutilated and garbled to suit his purpose, in hand-bills and advertisements, as though we sanctioned a secret remedy, which he now designated "Franks' Specific Solution," leaving out all mention of "*copaiba*."

The extreme indecency of such a proceeding struck me so forcibly, that within two hours after one of these hand-bills had been pointed out to me, I had written Mr.

Franks a note, expressive of my disapprobation, and requested, as a personal favour, that he would withdraw my letter altogether from his future publications.

To this note I have never been favoured with a reply.

Shortly after this, learning that Mr. Green was about to apply to the Court of Chancery for an injunction to restrain Mr. Franks from the misuse of his testimonial, I obtained, through Mr. B. Cooper's kindness, an interview with Mr. Green, who very liberally permitted me to add my affidavit of facts to those of himself and the several other aggrieved parties.

The Vice-Chancellor's answer to this was, that as no stipulation had been made at the time of writing the letters, he had not the power of preventing Mr. Franks from publishing them, his authority only empowering him to order, that to whatever purpose they might be applied, they should not be garbled or altered in any way from the original.

Such, gentlemen, is the entire story of my connexion with this nostrum,—this the extent of my offending. Without note or comment, I leave it to you to determine, after the perusal of the following documents, whether you will agree with the Council of the College of Surgeons in acquitting me, or with the newly-made governors of the "Free Hospital" in pronouncing my condemnation.

I subjoin the reply of the Council of the College of Surgeons to my statement, and also the resolution of the governors of the "Free Hospital," begging of you to contrast the one with the other, and to compare the facts of the case with the allegations of the resolution itself.

The Answer of the Council of the College of Surgeons.

"Royal College of Surgeons, in London, Jan. 27, 1836.

"Sir,—Having laid before the Council "of this College your letter of the 25th "instant, and its accompanying statement, "I am directed to acquaint you that the "Council are precluded from giving an "official or collective opinion on the sub-"ject; but they cannot believe that the "governors of any public institution will "be inclined to remove a surgeon from "their establishment, merely for trying a "new mode of administering an old and "well-known medicine."—"I am, sir,

"Your most obedient servant,

(Signed) "Edw. Belfour, *Secretary.*

"*To Alex. Tweedie, Esq. &c. &c.*"

The Resolution of the Governors of the "Free Hospital," on the 1st of March.

"That it appears to this meeting, that "Mr. Alexander Tweedie, one of the sur-"geons of this hospital, has, without con-"sulting with his medical brethren, ad-"ministered to the patients under his "care, a nostrum called 'Franks' Specific "Solution,' the properties of which could "not be known to him; and has, in direct "violation of the principle upon which "this hospital was founded, given a writ-"ten testimonial, recommending such "nostrum, which recommendation being "most extensively advertised, and appear-"ing on the face of it to emanate from a "surgeon of this establishment, cannot "fail to injure the charity most seriously "in the estimation of the public, whereby "its utility will be lessened, its prosperity "retarded, and the reputation of the other "medical officers materially affected.

"Under these considerations, it becomes "the painful duty of the meeting to re-"solve, that Mr. Alexander Tweedie be no "longer a surgeon of this hospital."

Having thus detailed the abstract facts of the case, permit me briefly to call your attention to the method and means by which our removal has been brought about.

About six months before the date of our removal, a motion of censure for this very business had been proposed against us by my then colleague, Mr. William Marsden, at a quarterly general meeting of the governors: the subject received ample discussion, and the explanation I gave was so satisfactory, that, by a large majority, the subscribers referred the matter to the officers of the establishment, and negatived the vote of censure. The gentlemen accordingly met together, and came unanimously to a resolution that was calculated to set the matter finally at rest. This resolution was received and confirmed without comment at a subsequent meeting of the committee of management.

It was with no small surprise, therefore, that myself in particular, and many old governors in general, received the announcement of a new attempt being made to re-open a question that had been, to all appearance, finally settled six months ago.

I believe, firmly, that the majority of the old and legitimate governors of the charity were, in truth, no parties to the scene that has recently been enacted.

Prior to the meeting at which our removal was effected, reports had been insidiously circulated, calculated to prejudice the minds of the subscribers, by reason of their partial and one-sided character. It was rumoured that I was connected, and on friendly terms, with Mr. Franks—which is not the fact:—it was insinuated that I had *been paid* for my letter—which is on the face of it a falsehood:—and it was studiously NOT stated, that I had made use of every endeavour in my power to put a stop to the nuisance. In short, it was given

out that I was physicking the poor with quack medicine, and that I was nothing but a quack myself.

But the gravest act of misrepresentation of which I have to complain, is one that emanated from my late colleague, Mr. Marsden. Whether done in ignorance of the fact or not, makes little difference; for Mr. W. M., unmindful of the delicacy due to a colleague in my situation, thought proper to issue a circular to the Governors, *on the eve of their meeting*, identifying himself with the business, gravely charging us with having recommended the "indiscriminate use" of a nostrum, and with having been guilty of "a direct breach of professional etiquette;" alleging, moreover, that this was "a circumstance (identified as I am with the hospital) which is seriously injuring both my reputation and practice."

In each of Mr. William Marsden's circulars came enclosed a caricature hand-bill, purporting to be "Copy from bills issued by Mr. Franks." From this caricature it was made to appear that I had given a "First testimonial," and a "Second testimonial;" whereas, in truth, I had never writen but one letter—that headed, "First testimonial from Alexander Tweedie, Esq." &c., being, in fact, the mutilated and garbled one published by Mr. Franks, before the Vice Chancellor's decision; and that headed "Second testimonial from Alexander Tweedie, Esq." &c. being the real letter that he has published since that decision.

I am willing to believe that such things might possibly have been circulated in ignorance of the truth; but the effect certainly was to mislead Governors, and to make them imagine that I had verily written *two testimonials.*

But the 23d February last, the evening of meeting, arrived, and disclosed a scene that had, perhaps, not been anticipated by the very leaders in the transaction. Nearly 400 persons (many of whom have been recognized not to be subscribers at all, and who had no right to be present) occupied the great room of the Gray's-inn Coffee-house; absolutely a larger number than appears in the printed list of Governors, including ladies, peers, corporation, parishes, and all.

In this assembly it was impracticable for any gentleman favourable to Mr. Hentsch or myself to obtain a hearing; clamour supplied the place of reason; they came to vote, not to inquire; and they had determined to have it all their own way. The following will serve to illustrate the spirit of the thing. A gentleman rose to address the meeting, "as a professional man." At these words, the uproar and confusion that resulted was beyond description; but the speaker was an adroit tactician, for he lifted up his voice, exclaiming, "I am going to speak in favour of Mr. Marsden's motion." Immediately the meeting became tranquil, and he was allowed to proceed.

After Mr. Hentsch was removed, there were those present who wanted me to resign; but I think you will agree with me, that such an act, with the fear of expulsion before my eyes, would have been suicidal of my reputation, and not worthy of that noble profession of which, I trust, I still continue a respectable and not dishonoured member.

Accordingly, when my turn for expulsion arrived, I first protested against the illegality of being tried by a meeting constituted as that meeting was, and then having made my statement for the satisfaction of the respectable governors, and having heard from my colleagues, Dr. Uwins, Dr. Ryan, and Mr. Lucas, that so far from considering their "reputations likely to be materially affected" by my conduct, "they were prepared to resign with me if the meeting persisted in so gross an act of injustice as to remove me;" and from another colleague, Mr. Jones, that although he would not resign, he did not consider his reputation to be of so sensitive a nature as to sustain any injury from such a cause: having done these things, and the meeting becoming so uproarious that it was evident to the real governors of the institution that justice would not be done, I, in company with a large number of respectable friends, retired, and left the meeting to sustain the whole responsibility of carrying, in our absence, "by a majority of five to one," the resolution, of which a copy has been given.

It was not my original intention to have intruded my grievance thus before the professional public, but circumstances and the advice of friends have induced me to alter that determination. Allow me, gentlemen, to indulge the hope, that in your estimation I shall be exonerated from blame, as I have been in the declaration of the Council of the College of Surgeons.

I have nothing further to say—than to acknowledge with pride and gratitude, thus publicly, the assistance and unsolicited support afforded to me on the occasion, not only by many of the eldest and most respectable lay-subscribers of the charity, but also by a number of medical friends, amongst whom Mr. Bransby Cooper was the foremost, who, of their own accord, kindly came forward to bear testimony in my favour.—I am, gentlemen,

Yours faithfully,

ALEXANDER TWEEDIE, M.R.C.S.

43, Cannon-street, London,
16th March, 1836.

ROYAL INSTITUTION.

Friday, March 11, 1836.

On Warming and Ventilating Houses.

It was announced that Dr. Arnott was to give some new views on this subject this evening, and the theatre was filled to hear him. But we regret that not happening to procure a seat on any of the benches near the lecturer, we were not within ear-shot of a tenth part of what he said. Dr. Arnott ought to contrive some ingenious method of diffusing his *voice* through a large apartment, as he does heat and air: and if we might suggest a simple plan to him for the purpose, it would be to *speak up*, and fancy himself talking to somebody at a distance. What we did hear, principally related to the importance of warming houses economically, and the propriety of adopting Dr. Arnott's new invention of a sheet-iron box, for generating hot air, and radiating heat from its surface. Dr. A. described the various methods which he previously tried—such as setting up in his library a box full of hot water kept at a given temperature, by being in connexion with a boiler on the kitchen fire, then boiling the water within the box, and ultimately putting into the iron inclosure a small stove with lighted fuel, admitting cold air below, causing it to circulate within, and regulating all by a throttle valve, and compound metal bar. His plans of ventilation were wholly unintelligible, as he referred perpetually to drawings in faint outline, and placed at such a distance as to defy the most piercing sight: they were, as he himself facetiously observed, "drawings on the imagination:" but we thought it rather *too much* for the Dr. to tax the imagination, in addition to the senses of sight and hearing.

LITERARY INTELLIGENCE.

Preparing for publication, Observations on the Efficacy of Iodine in Secondary Syphilis. By Forbes Winslow, M.R.C.S.

WEEKLY ACCOUNT OF BURIALS,

From Bills of Mortality, *Mar.* 15, 1836.

Abscess . .	2	Hernia . . .	1
Age and Debility .	33	Hooping Cough .	1
Apoplexy . .	10	Inflammation .	17
Asthma . .	15	Bowels & Stomach	4
Cancer . .	3	Brain . .	2
Childbirth . .	2	Lungs and Pleura	4
Consumption .	57	Insanity . .	7
Convulsions .	15	Liver, diseased .	3
Dentition or Teething	5	Measles . .	1
Dropsy . .	16	Mortification .	4
Dropsy on the Brain	9	Paralysis . .	2
Dropsy on the Chest	2	Small-pox . .	8
Erysipelas . .	1	Spasms . .	1
Fever . . .	2	Tumor . .	3
Fever, Scarlet .	1	Unknown Causes	19
Gout . . .	1		
Heart, diseased .	1	Casualties . .	2

Decrease of Burials, as compared with the preceding week . . } 139

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

March 17, 1836.

James Hogg, Brighton.
Richard Charles Lamb, Middleham, Yorkshire.
Charles Jey, Queen Camel, Somersetshire.
George Style, Dorchester.
George Hill Smith, Rodstone, Northamptonshire.
Thomas Wilkinson Atkinson, Broughton House, Cartmel, Lancashire.
Frederick Francis Whitfield, Ashford, Kent.
Edward Bennett, Chapel-en-le-Frith.
Alfred Baker Cutfield, Deal, Kent.
Charles Spencer, Chippenham, Wilts.
William Benedict Hart.
Richard Holmes, Birstal, Yorkshire.
Edward George, Rochester, Kent.
Joseph Dunn, Manchester.

METEOROLOGICAL JOURNAL.

Kept at Edmonton, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

March, 1836.	Thermometer.		Barometer.	
Thursday . 10	from 33	to 46	29·33	to 29·25
Friday . . 11	36	47	29·09	29·[illegible]4
Saturday . 12	33	46	29·10	29·30
Sunday . . 13	34	50	29·47	29·54
Monday . . 14	40	49	29·17	29·31
Tuesday . . 15	34	47	29·01	29·36
Wednesday 16	29	46	29·64	29·94

Prevailing winds, S.W. & W.

Generally cloudy, with frequent and heavy rain. Hail storms on the 11th, and following day.

Rain fallen, 1 inch, and ·175 of an inch.

Charles Henry Adams.

NOTICES.

We treated Dr. Kerrison uncourteously last week, by abridging him of his legitimate title. His paper on Tic Douloureux was announced on the wrapper as by R. M. Kerrison, *Esq.*: this was mere accident, which we hope Dr. K. will overlook.

It would be desirable to have the remainder of the MS. on the "Animal Fluids," ere we put the first part in the printer's hands. We should be glad to commence the series in the first number of our new volume.

Errata.—In Mr Brett's paper on Urinary Deposits, p. 799, 2d col. line 38 from the top, *for* "in this colouring matter," *read* "in this form of deposit." Page 849, 2d col. line 8 from the top, *for* "It is, however, maintained," *read* "It is *not*, however, maintained." Page 897, 1st col. line 11 from the bottom, instead of placing the period after the word "colour," place it after the word "*urine*;" and instead of reading, "from any of the urinary salts which may become precipitated," &c. *read* "For any of the urinary salts," &c.; and instead of "olizarine," read "alizarine." Page 924, 2d col. line 28 from top, *for* "three reagents," *read* "those reagents."

Mr. Belinaye requests us to supply an omitted phrase in his paper of last week. At p. 946, 2d col. line 48, after "83 in 100 died in the hospital," *read* "or in private dwellings."

Wilson & Son, Printers, 57, Skinner-St. London.

THE

LONDON MEDICAL GAZETTE,

BEING A

WEEKLY JOURNAL

OF

Medicine and the Collateral Sciences.

SATURDAY, MARCH 26, 1836.

LECTURES

ON

MATERIA MEDICA, OR PHARMACOLOGY, AND GENERAL THERAPEUTICS,

Delivered at the Aldersgate School of Medicine,

BY JON. PEREIRA, ESQ., F.L.S.

LECTURE XXVI.

CARBONIC ACID.

THE last of the mineral acids which I shall notice is the carbonic.

History and synonymes.—Although the ancients were acquainted with its poisonous properties, the nature of carbonic acid gas was first explained by Dr. Black, in 1757. The *spiritus lethalis* of the ancients is evidently this acid, as is also the *spiritus sylvestris*, or *gas*, of Paracelsus and Van Helmont. *Fixed air*, *acid vapour*, and *aërial acid*, are other names by which it has been known.

Native state.—You are aware it is a constituent of the atmosphere. In some parts of the world it is evolved from the earth in large quantities, particularly in old volcanic countries. Thus in the vicinity of the Lake of Laach, Bischof estimates the exhalation as equal to 600,000 lbs. daily, or 219,000,000 lbs. annually! Some of the acid evolved in the Brohlthal, on the Rhine, he employs in the manufacture of chemical preparations on the large scale. You are, I presume, familiar, by report, with the *Grotto del Cane*. It is a cavity in a rock, through the fissures of which carbonic acid is evolved. It has received its name from the practice of putting dogs into it, who fall down suffocated. I cannot resist alluding to the *Valley of Poison*, in Java, which has been described by Mr. Loudon. From this gentleman's statement, it appears to be a cavity of an oval form, about three-quarters of a mile in circumference, and from thirty to thirty-five feet deep; filled to the height of about eighteen feet with carbonic acid gas. The bottom of it is covered with the skeletons of men and various other animals, who have fallen victims to its destructive operation. If a traveller should be so unfortunate as to enter it, he cannot be sensible of his danger until too late to return. Mr. Loudon thrust a dog in; he fell in fourteen seconds. A fowl thrown in appeared to be dead before it reached the ground!! In mines and wells we sometimes meet with this gas, and in these situations it is usually termed *choke damp*, the word damp being evidently derived from the German *dampf*, vapour. Few mineral waters are without this acid, and in some it exists in such large quantities as to render them sparkling and effervescing. In combination with lime, it is one of the most common constituents of the crust of our globe. Lastly, it is evolved both by vegetables and animals.

Preparation.— Pour hydrochloric acid, diluted with four or five times its volume of water, on some pieces of marble contained in a glass retort. Carbonic acid gas is copiously evolved, and may be collected over water. By the mutual reaction of the hydrochloric acid and carbonate of lime (marble), water, chloride of calcium, and carbonic acid, result.

Re-agents.	Results.
$\dot{\mathrm{Ca}}\,\ddot{\mathrm{C}}$	$\ddot{\mathrm{C}}$
$\mathrm{H\,Chl}$	$\dot{\mathrm{H}}$
	$\mathrm{Ca\,Chl}$

Properties. — At common temperatures and pressures it is gaseous, invisible, and odourless, (or nearly so). Its specific gravity is 1·5277. It is neither combustible

nor a supporter of combustion, except in the case of potassium, which, when heated in it, readily takes fire, and burns. It extinguishes most burning bodies when introduced into it in the ignited condition. Some years since, Dr. Faraday succeeded in condensing it into a liquid form; and lately it has been announced that a French chemist has solidified it.

Characteristic tests.—It is distinguished from other gases by being incombustible, and a non-supporter of combustion (except in the case just mentioned), and by its precipitating the solutions of lime or barytes white, the precipitate being soluble in acetic acid. The carbonates effervesce on the addition of hydrochloric acid.

Composition.—By burning charcoal in 1 volume, or 16 parts by weight, of oxygen gas, we obtain 1 volume, or 22 parts by weight, of carbonic acid.

Before combustion.		Result.
oxygen = 16	& carbon = 6	carbonic acid = 22

Hence this acid is composed of—

1 atom of carbon	=	6
2 atoms of oxygen	=	16
1 atom of carbonic acid ..		22

Solution of carbonic acid.—At the ordinary temperature and pressure of the air, one volume of water absorbs one volume of gas; and by doubling the pressure the quantity of gas absorbed is doubled, and so on for other degrees of force. The bottled soda water of the shops is usually only a solution of this gas in water; but some manufacturers add soda to the water before they condense the gas in it.

Physiological effects.—These vary according to the part of the body to which the acid is applied.

(*a.*) *Inhaled*, it acts as a positively narcotic poison. In the pure state it excites spasm of the glottis; but when mixed with atmospheric air, it causes a sensation of tightness at the chest, uneasiness, giddiness, loss of muscular power, insensibility, and stertorous breathing, sometimes accompanied with convulsions or delirium. That it is a positive and not a negative poison, as Nysten and others have asserted, seems proved from two facts: 1st, that an atmosphere composed of 79 parts of carbonic acid, and 21 of oxygen, acts as a poison, although there is as much oxgen present as there is in common atmospheric air; 2dly, that one bronchial tube of the land tortoise may be tied, without any serious injury to the animal; but if, instead of tying it, the animal be made to inhale carbonic acid gas by it, death takes place in a few hours.

(*b.*) Applied *externally*, it acts as an irritant. Thus it excites pain, redness, and a flow of tears, when brought in contact with the *conjunctiva*. Applied to the *skin*, care being taken that it be not respired, it excites a sensation of warmth, and pricking, or tingling, sometimes accompanied by pain, increased frequency of the pulse, sweating, and excitement of the nervous system.

(*c.*) Taken into the *stomach*, dissolved in water, or in the form of effervescing draughts, it allays thirst, and diminishes preternatural heat, thus acting like the other dilute acids. If it be evolved in the stomach, it distends this viscus, excites eructations, and checks both nausea and vomiting. It appears to promote the secretions of the alimentary tube, to assist the digestive process, to allay irritation, and to act as a refreshing and exhilarating substance. It is said to be diuretic and diaphoretic. Wöhler and Stehberger expressly state that the use of carbonic acid did not increase the quantity of this substance in the urine. When drunk too quickly, and in large quantity, water impregnated with this gas has been known to excite giddiness and intoxication; and it is probable that champagne is indebted to this substance for part of its intoxicating powers.

(*d.*) *On dead animal matter* it acts as an antiseptic.

Uses.—(*a.*) *When inhaled.*—In some diseases of the lungs, particularly phthisis, it has been proposed to mix carbonic acid gas with the atmospheric air breathed by the patient, with the view of lessening the stimulant influence of the oxygen; and it is said to have had the effect of diminishing the quantity of matter expectorated, and of improving its quality, while at the same time it relieved the hectic symptoms. It is not at all unlikely that temporary relief might be gained by its employment; but difficulty is experienced in the mode of applying it. Of course, to be beneficial, it ought to be constantly inspired. Temporarily it is readily inhaled, by generating it in a large bottle or jar, and breathing it by means of a tube; but for its continued use, the only method that can be employed is to evolve the acid in the patient's chamber. In some parts of the continent a popular remedy for consumption is a residence in rooms or houses inhabited by cows; and the asserted beneficial effects are supposed to be in part attributable to the carbonic acid gas with which the chamber is impregnated; but, says Vogt, in his *Lehr-*

buch der Pharmakodynamik, "we must not overlook the humid, mild, balsamic vapour, with which the air of cow-houses is commonly filled*."

(*b.*) *Taken into the stomach*, carbonic acid is a most valuable remedy for checking vomiting, and diminishing irritable conditions of this viscus. The best mode of exhibiting it is, I believe, in the form of an effervescing draught, composed of citric acid and bi-carbonate of potash. In fever, you will find it an excellent refrigerant; especially serviceable in those cases which are accompanied by a disordered condition of the digestive organs. In that form of lithiasis attended with a white or phosphatic deposit in the urine, water, impregnated with carbonic acid, may be taken with advantage; but in this case you cannot substitute the common effervescing draught, (made of a vegetable acid, and a carbonated alkali) since this communicates an alkaline property to the urine, as I shall have occasion hereafter to explain, when speaking of the alkalies and their salts.

(*c.*) *Clysters of carbonic acid gas* have been employed in certain affections of the rectum and colon,—for example, ulceration of the rectum, especially when of that kind commonly denominated cancerous.

(*d.*) *A stream of carbonic acid gas* has been applied to the *uterus* with great benefit, in a painful condition of that viscus. [See p. 134 of the present volume.]

(*e.*) *Applied to the skin,* care being taken that the gas is not inhaled, it is employed either in its gaseous form, or dissolved in water. It is, of course, adapted to those cases where it is desirable to excite the vascular system, especially of the skin, and to cause perspiration; while, on the other hand, it is objectionable in inflammatory cases. In chlorosis, amenorrhœa, dyspepsia, hysteria, scrofula, &c., it has been found useful.

(*f.*) It has been applied to the surface of cancerous and other ulcers, to allay pain, improve the quality of the secretions, or to check sloughing. It is readily administered by means of a tube connected with a bottle generating the gas. In this case you had better procure it by the action of dilute sulphuric acid on marble, for if you employ hydrochloric acid, the gas requires washing, to remove any of this acid which may pass over with it. Or it may be used in the form of solution, in which case the bottled soda-water may be employed.

Poisoning.—As carbonic acid gas, when taken into the lungs, is a powerful poison, and as it is a substance which may be frequently met with, I think it right to point out under what circumstances you may expect to find it, and the best method of treating patients labouring under its effects.

In the first place, as to the *sources* of it: I have already alluded to some instances of its natural evolution from the earth; its development in wells and mines. It is produced by organized beings, plants as well as animals, and, therefore, is met with in greenhouses, especially during the night, when it is given out by vegetables. In crowded rooms, with imperfect ventilation, accidents have sometimes happened, from the accumulated carbonic acid. It is developed by fermentation, and hence the danger of descending into brewers' vats; it is evolved by the burning of limestone (carbonate of lime) at lime kilns, and by the combustion of charcoal and carbonaceous gases, as coal gas, and the fire damp of coal mines.

The best *treatment* is the following:—remove the patient immediately into the open air, and place him on his back with the head somewhat elevated. Produce artificial respiration in the way I have on a previous occasion described, namely, by pressing down the ribs, and forcing up the diaphragm, and then suddenly removing the pressure. Dash cold water over the body, and abstract a small quantity of blood either by venæsection or cupping. Stimulants of various kinds may be em-

* I am indebted to Mr. Steinhaeuser (a very intelligent pupil of mine), a native of Saxony, for the following note on residence in cow-houses, as a remedial agent in diseases of the lungs:—"In Germany, the balsamic air of cow-sheds is commonly recommended as a preventive in suspected pulmonary disease, or as a means of prolonging life in confirmed phthisis. Although this latter disease is comparatively of rare occurrence in Saxony, yet several cases have fallen under my own observation, in which this plan of treatment was adopted. The mode of effecting it has varied according to circumstances: in some cases the patient has merely retired from a crowded town to a farm-house; in others, the sitting and bed rooms have actually been converted into residences for cows. Of the former, I have known several instances where patients have been greatly benefitted by sleeping in apartments built over cow-stalls; and this, I should say, is the most usual plan adopted. Of the latter I can only record one case, which is somewhat remarkable. It is that of the late Prince Putiati, a Russian exile, resident in the vicinity of Dresden, and well known there on account of his eccentricities. His young, beautiful, and only daughter, the Countess ———, being affected with this destructive malady (phthisis), to which she eventually fell a victim, the warm air of cow-sheds was recommended by her physicians, as a mode of prolonging her life. The Prince ordered the lower part of a wing of his magnificent but curiously constructed mansion (Schachwitz, to be converted into a cow-stable; and the elegantly furnished sitting and sleeping apartments of his daughter were so arranged, that she was actually in the same room with the cattle, from which she was merely separated by a low partition." Of the eccentricities of the Prince I may just add two illustrations: his boots were made of tin, to guard against the possibility of his legs being bitten by mad dogs, while his umbrella was attached to his shoulder by a hook, and was glazed, that is, had what we may call a sky-light in it.—J. P.

ployed either internally by the stomach, or in the form of frictions, or inhalations (as of ammonia, weak chlorine, &c.).

A word or two as to the means of guarding against the inhalation of carbonic acid gas. It sometimes happens that you may be required to enter a room or other place containing this gas, but which may have sufficient oxygen present to support life, if the noxious gas can be got rid of. Now it is important to know how to strain the air, so as to stop the acid while the oxygen is allowed to enter the lungs. The principle is simple enough; namely, to breathe through an alkaline or even aqueous solution. Thus, dip a handkerchief or piece of flannel in a weak solution of potash or of lime, or even in common water, and apply this over the mouth and nose. If at hand, *Mr. Roberts's fire-hood* may be regarded as the best kind of apparatus. It consists of a leathern hood fitting over the head, with glass eyes, and a breathing tube or pipe, at the end of which is a piece of sponge dipped in cream of lime; so that all the air inhaled has to pass through the lime, which stops the carbonic acid.

Yeast Poultice.

As the efficacy of the *yeast poultice* is usually supposed to depend on the carbonic acid gas which is extracted from it, this, I conceive, will be the most convenient place for noticing it.

Preparation.—In the London Pharmacopœia it is ordered to be prepared by mixing a pound of *flour* with half a pint of *yeast*, and applying a gentle heat until the mixture begins to rise. But this is not the method invariably followed. Thus, you may substitute the *grounds of stale beer* for the yeast, and a mixture of oatmeal (or of potatoes) and linseed meal for the flour.

Yeast, or the *fermentum cerevisiæ*, somewhat approaches gluten in its nature; it is a viscid substance, composed, according to F. Marcet, of—

Carbon	30·5
Oxygen	57·4
Nitrogen	7·6
Hydrogen	4·5
	100·0

I suspect, however, there is some error in this analysis, the quantity of oxygen being too large. I quote the analysis from Gmelin's *Handbuch der Chemie*. The most characteristic property of yeast is that of exciting fermentation. Hence its use in this preparation. Now one of the products of the process of fermentation is carbonic acid gas, which is supposed to be the active ingredient of this poultice.

Effects and Uses.—It is applied to fœtid and sloughing sores, as an antiseptic and stimulant: it destroys the fœtor, often checks the sloughing, and assists the separation of the dead part. I have frequently heard patients complain of the great pain it causes.

WATER.

History.—By the ancients, water was regarded as an elementary substance, and as a constituent of most other bodies. This opinion, apparently supported by numerous facts, was held until the middle of the last century, when the Hon. Mr. Cavendish proved that this liquid was a compound of two other bodies—oxygen and hydrogen. It is, however, only doing justice to Mr. Watt, to say that he had previously inferred that this was the composition of water, but was deterred from publishing his opinion in consequence of some of Dr. Priestley's experiments being apparently opposed to it.

Native State.—(*a.*) *In the inorganized kingdom.*—In the *atmosphere*, water exists in two states—either as a vapour, which is supposed to communicate the blue colour to the sky, or in a vesicular form, constituting the clouds. Three-fourths of the whole *surface of the globe* are aqueous; and the average depth of the ocean is calculated at between two or three miles. Now as the average height of dry land above the surface of the sea is less than two miles, it is evident that if the present dry land were distributed over the bottom of the ocean, the surface of the globe would present a mass of waters a mile in depth. Water is also found in some minerals in a solid state, constituting what are called *hydrates*.

(*b.*) *In the organized kingdom.*—Water is an essential constituent of vegetables and animals. The only exception that can be urged against this statement, is, that some *musci*, and also some *polygastrica*, may be dried without losing their vitality, although, while in this state, they give no sign of life, but may be restored by moistening them.

Preparation of pure water.—Absolutely pure water may be procured by combining its elements; but for all practical purposes, it is obtained pure by the distillation of common water. The purest natural water is snow and rain water; then follow river, spring, and well waters.

Properties of water.—Pure water has the following properties:—At ordinary temperatures it is a transparent liquid, usually described as being both odourless and colourless; but it is well known that the camel can scent water at a considerable distance, so that to this animal it is odorous; and as regards its colour, we know that all large masses of water have a

bluish-green colour, though this is usually said to arise from foreign matters. When submitted to a compressing force equal to 30,000lbs. on the square inch, 14 volumes of this liquid are condensed into 13 volumes; so that it is elastic. At the temperature of 60° Fahr. it is about 814 times heavier than atmospheric air: but being the standard to which the gravities of solids and liquids are referred, its specific weight is usually said to be 1·. When cooled below 32°, it crystallizes, and forms ice; the fundamental form of whose crystal is the rhombohedron. It evaporates at all temperatures, but at 212° boils, and is converted into steam. It unites with both acids and bases, but without destroying their acid or basic properties. Thus the crystallized vegetable acids, tartaric, citric, and oxalic, are atomic combinations of water with acids. Potassa fusa and slacked lime may be instanced as compounds of water and basic substances: these are called *hydrates*. It is a chemical constituent of some crystallised salts; for example, alum, sulphate of soda, and sulphate of magnesia. It rapidly absorbs some gases—as fluoride of boron, ammonia, &c. It is neither combustible nor a supporter of combustion.

Characteristics.—In the liquid state, it is recognized by being tasteless, and neither combustible nor a supporter of combustion: it is miscible with alcohol, but not with the fixed oils; if potassium be thrown on it in the open air, the metal takes fire. Lastly, water may be decomposed into oxygen and hydrogen by the galvanic agency. The most delicate test of aqueous vapour in any gas, is fluoride of boron (commonly called fluoboric acid gas), which produces white fumes with it.

Composition.—The composition of water is determined both by analysis and synthesis. If this liquid be submitted to the influence of a galvanic battery, it is decomposed into two gases—namely, one volume of oxygen, and two volumes of hydrogen gases. These gases, in the proportions just mentioned, may be made to recombine by heat, electricity, or spongy platinum; and water is the result.

Atomic composition.			Per centage composition.
1 atom of oxygen..	= 8	..	88·8
1 atom of hydrogen	= 1	..	11·1
1 atom of water....	9		99·9

It is composed of 1 volume of hydrogen, and half a volume of oxygen, which unite and condense into 1 volume of aqueous vapour.

Constituents.		Results.
Hydr. = 1	Oxyg. = 8	Aqueous vapour = 9

Tests of its purity.—Pure distilled water ought to remain unchanged on the addition of any of the following substances:—solutions of the alkalies, lime water, oxalic acid, the barytic salts, nitrate of silver, and solution of soap. If any turbidness, milkiness, or precipitate, be occasioned by any one of the above, we may infer the existence of some impurity in the water.

Physiological effects and uses.—In a *dietetical* point of view, water serves at least three important purposes in the animal economy; namely, it repairs the loss of the aqueous part of the blood, caused by the action of the secreting and exhaling organs; secondly, it is a solvent of various alimentary substances, and, therefore, assists the stomach in the act of digestion, though, if taken in very large quantities, it may have an opposite effect, by diluting the gastric juice; thirdly, it is probably a nutritive agent, that is, it assists in the formation of the solid parts of the body.

In a *therapeutical* point of view, we employ it for several purposes—thus, sometimes as a medium for communicating or abstracting heat from the body; sometimes as a diluent, as in poisoning by acrid substances; sometimes to promote the activity of the exhaling or secreting organs; lastly, at other times it seems useful by its impulse, or pressure. In some of the modes of employing it remedially we attain more than one of these objects; thus in cold affusion the water acts both by its temperature and impulse.

1. *Effects of ice, snow, and ice cold water.*—The temperature of these agents is not higher than 32° F. When brought in contact with a living part, they produce two series of effects—the first of which may be denominated *direct*, *primary*, or *immediate*; while the second may be termed *indirect*, *secondary*, or *mediate*, since they are developed by the vital actions merely, after the cold agent has ceased to abstract heat. (*a.*) *Of the primary effect*.—When ice is applied to the body, it abstracts heat, causes pain, reduces the volume of the part, and diminishes vital action; and if applied for a sufficient period occasions mortification,—an effect which is hastened by the previously weakened condition of the part. If applied to a large surface of the body, and for a sufficient length of time, the

processes of secretion, circulation, and respiration, are checked, and stupefaction, followed by death, ensues.

The primary effect of ice or snow applied *externally* is much greater than when either of these agents is swallowed The reason of this will be obvious presently.

(*b*.) *Secondary effects.*—If, however, the application be of a temporary nature, more especially if the subject be young and robust, reaction follows the removal of the cold agent. The vascular action of the part is increased, the pulse becomes full and more frequent, and the animal heat is restored to its proper degree, or is even increased beyond its natural standard. These effects, more or less modified, are observed both from the internal and external employment of ice.

When taken *internally*, the sensation of cold which it produces is not so obvious as that occasioned by its external application (in consequence, probably, of the stomach being accustomed to the contact of cold substances); and the effect is more temporary, from the greater heat of the internal parts, by which the ice is sooner melted, and the resulting liquid quickly raised to the temperature of the body. When, however, it is taken in sufficient quantity, the effects are of the same general kind as those already described, namely, a sensation of cold in the epigastrium, sometimes attended with shivering; diminished frequency of pulse; temporary contraction of the alimentary canal; diminution of irritability, and of secretion. Employed in small quantities, these effects are not at all perceived, or are only momentary; and the second stage, or that of reaction, almost immediately follows. A feeling of warmth at the epigastrium soon succeeds that of cold, and this extends shortly over the whole body: the secretions of the alimentary canal, of the kidneys, and of the skin, are increased; and the circulation is accelerated. Sometimes these secondary effects are attended with those of a morbid character; thus inflammation of the stomach is occasionally brought on by the employment of ice.

Uses.—Sometimes we administer ice *internally*, for the purpose of obtaining its primary effects; thus, in *hæmorrhages* from the stomach, we use it for the purpose of causing contraction of the vessels of the gastric surface, and thereby of checking or stopping the sanguineous exhalation. So also in violent hæmorrhage from the lungs or nose, ice-cold water, taken into the stomach, has been beneficial. In most cases, however, we use it on account of its secondary effects. Thus, in relaxed and atonic conditions of the stomach—in dyspepsia and cardialgia—to check vomiting—and to allay spasmodic pain. In those forms of fever denominated putrid, the internal use of small quantities of ice is sometimes beneficial.

Ice is sometimes employed, *externally*, to check hæmorrhage, more especially when the bleeding vessel cannot be easily got at and tied. Thus after operations about the rectum (more especially for piles and fistula) hæmorrhage sometimes occurs to a most alarming extent; and in such cases our principal reliance must be on cold. In two instances that have fallen under my own observation, I believe the lives of the patients were preserved by the introduction of ice within the rectum. In many other cases of hæmorrhage, the external application of cold (either in the form of ice or ice-cold water) is exceedingly useful. Thus, applied to the chest in dangerous pulmonary hæmorrhage, to the abdomen in violent floodings, it is oftentimes most beneficial. In some of these cases, especially in uterine hæmorrhage, more benefit is obtained by pouring cold water from a height, than by the mere use of ice. I shall allude to this hereafter.

Pounded ice, tied up in a bladder, has been applied to *hernial tumors*, to diminish their size and facilitate their reduction; but notwithstanding that the practice has the sanction and recommendation of Sir Astley Cooper, it is, I believe, rarely followed, not having been found successful; and if too long continued, may cause gangrene. In this, as well as in other cases where ice or snow cannot be procured, you may substitute a freezing mixture. For this purpose, five ounces of muriate of ammonia, five ounces of nitre, and a pint of water, are to be placed in a bladder, and applied to the part. Ice has also been applied in *prolapsus* of the rectum or vagina, when inflammation has come on which threatens mortification.

In *inflammation of the brain*, ice, pounded and placed in a bladder, may be applied to the head with a very beneficial effect. In *fever* also, where there is great cerebral excitement, with a hot dry skin, I have seen it advantageously employed. In *apoplexy* also it might be useful. In the *retention of urine* to which old persons are liable, ice-cold water applied to the hypogastrium is sometimes very effective, causing the evacuation of this secretion.

In the above-mentioned local uses of ice, we either apply it directly to the part, or enclose it in a bladder: the latter is to be preferred, since the patient is not wetted with the melted water, while the effect is less violent.

In the last place, I must notice the employment of ice or snow in the form of

friction. Whenever used in this way, the ultimate object in view is the production of the secondary effects, or those which constitute the stage of reaction. Thus this practice has been resorted to in diminished sensibility of the skin, in the rheumatism or gout of old and enfeebled persons, in order to produce excitement of the skin; but its most common use is as an application to parts injured by cold. The affection thus induced is called *pernio*, or the *chilblain*; and the parts which are thus affected are said to be *frost-bitten*. The feet, hands, tip of the nose, and pinna of the ear, are the organs most frequently attacked. Now, with the view of preventing the mortification and other ill consequences arising from the application of cold, you must most carefully avoid sudden changes of temperature. The frost-bitten part, or the chilblain, should be rubbed with snow or pounded ice, or bathed in ice-cold water, very gradually raising the temperature of the applications until the part acquires its natural heat.

The further consideration of the effects and uses of water must be left until our next lecture.

OBSERVATIONS ON THE HEART'S ACTION AND SOUNDS.

By Charles Cowan, M.D.

Notwithstanding all that has been done and written for the purpose of elucidating the mode of the heart's action, and the true causes of its sounds, no one can deny that the question continues to be involved in considerable difficulty, and that various contradictory hypotheses still find their respective supporters. Much research and anatomical ingenuity have been expended in unravelling the complicated structure of this important organ, leaving to our successors rather the proper application of the knowledge we possess, than space for much additional discovery. Its movements, so far at least as they can be studied by ocular inspection on the living, though mutilated animal, have been watched with scrupulous accuracy, and its sounds both in health and disease, so vigilantly inspected by but a lately appreciated sense, that we may almost despair of any very important novelty being detected by those who are continuing the investigation. There is, however, one department of the subject, which we think has not sufficiently arrested the attention of observers, viz. an accurate appreciation of the physical conformation of the cardiac cavities, as well as of their valvular appendages, and a strict association of these with the hydrostatic conditions of the circulation. Hypotheses have been set forward, with apparent facts for their basis, which, when impartially scrutinised, may be referred back for their origin to the very theory they are supposed to originate: conjectures have been founded upon experiments either wholly artificial and of more than questionable accuracy, or made on the living animal when the physiological integrity of the organ was seriously impaired: and when we reflect on the numerous elements of the problem attempted to be solved, and the difficulty of submitting them all to the test of rigorous demonstration, it must cease to be a matter of surprise that differences of opinion continue to prevail.

In the following observations we shall endeavour to direct attention to considerations which we believe are not without importance when analysing the possible causes of the heart's sounds, and we do so the more willingly, since we are satisfied that any suggestion we may offer will be impartially canvassed by those who are now engaged in the arduous task of untying the gordian knot of this anatomical and physiological puzzle.

The ingenious researches of M. Bouillaud, in his recent work on diseases of the heart, have first clearly pointed out the fact that the ventricular cavities consist of an auricular and vascular portion; the former immediately continuous with the auriculo-ventricular opening on either side, and the latter with the aorta and pulmonary arteries. We have repeatedly verified the reality of this description, both in the heart of man, the ox, and sheep; and our only surprise has been, that so palpable an arrangement could have been hitherto almost entirely overlooked*.

Let us briefly avail ourselves of M. Bouillaud's description. The cavity of

* To estimate the accuracy of the succeeding remarks, the student should have an opened healthy heart before him. The ventricular cavities should be exposed, by cutting through the aorta and pulmonary artery.

each ventricle, he says, is composed of two very distinct regions: in the right side, the pulmonary is united to the auricular by a kind of angle, the sinus of which is directed upwards. In the left, the aortic and auricular regions are parallel to one another, or nearly so, and take a vertical direction. They are separated by the right or anterior lamina of the bicuspid valve, and by two large fleshy columnæ, which, projecting in the interior of the left ventricle, rise by numerous fasciculi from the posterior paries; the one outwards, towards the junction of the anterior surface of the ventricle, with its posterior forming the left border of the heart; the other inwards, at the point of union of the posterior surface with the interventricular partition. It is below, backwards, and to the outside of this natural division, or bridge, that the auricular portion is placed, whereas the aortic region, larger than the other, is situated above, forwards, and inwards. These regions communicate with one another at the interval which separates the two voluminous columnæ mentioned.

From this arrangement it will be seen, in the first place, that the ventricular cavity, taken as a whole, is not placed (as the majority of descriptions would lead us to suppose) *immediately beneath* the auriculo-ventricular opening, but that the latter corresponds to only one, and that the smaller portion of the ventricles, which we have seen, in the left side, communicates with the aortic portion, between the two fleshy pillars, to which all the chordæ tendineæ are attached. The blood, therefore, in its descent from the auricle, cannot drop, as it were, into a vacuum previously formed for its reception by the active ventricular dyastole (Hope—Pigeaux), but must *gradually* pass downwards through the auricular portion, and from thence into the aortic portion, through the communicating space to which we have already alluded.

The theory, therefore, of one of the sounds depending on the shock of the blood against the apex, in its descent from the auricle, is, we think, refuted by an examination of these physical conditions. Besides, the blood must enter the ventricle from the first moment a vacuum begins to be formed; and it is evident that the portion of the ventricle which is most muscular, and consequently most powerfully contracted, will first expand, whether the cause of its expansion be either active or passive. Now the muscular structure of the ventricle is more developed in the auricular than in the aortic portion; the former, therefore, will first open, admitting the blood from the auricle, which from thence will successively occupy the remaining portions of the cavity. The idea of the ventricle being *empty and dilated*, for however short a moment of time, cannot, we think, be regarded as even possible; since the slightest augmentation of the cavity after the completion of the diastole, must necessarily involve a proportionate descent of the blood from the auricle; the valves presenting no resistance whatever in a downwards direction. The conditions of a shut cavity are, in fact, such that we do not hesitate to say that the ventricular diastole *could* not take place, if the ingress of the blood were impossible; the latter follows the augmentation of the cavity in the direction and in the manner we have described, and the ventricle is successively filled, without that shock and agitation of the fluid which is generally believed to take place at the moment to which we are referring. We need not, therefore, be surprised if no *sound* is generated by the descent of the blood into the ventricle, this fluid maintaining a constant and intimate relation with the gradually expanding walls of the cavity. All contrary hypotheses involve a want of this correspondence between the descending fluid and the dimensions of the cavity it is filling—a want, we believe, physically impossible, and which we could not conceive to exist without materially interfering with the regularity and efficiency of the heart's action.

An attentive examination of the ventricles, particularly of the left, will prove that the act of contraction almost obliterates the auricular portion of the cavity, and that the region corresponding to the sigmoid valves is conical in its form, smooth in its surface, and continuous with the calibre of the aorta; into which, therefore, the blood is propelled in a direct and equable manner, without those conflicting and counteracting currents, those violent molecular collisions, on which Dr. Hope and others have so much insisted.

Another important consideration, when analysing the heart's action, is the fact that at no one moment is there an empty

space, either in the cavities of this organ or in the vessels communicating with them; the line of blood being invariably unbroken, with the exception of the temporary intervention of the valves. It is true that all conceivable variations in the relative amount of fluid in the different cavities alternately exist, but this increase or decrease is invariably accompanied with corresponding variations in the containing parietes. Both theory and observation prove that the heart, by contracting, never expels the whole of its contents, the exit of the latter being uniformly opposed by the resistance of the fluid which is without; and the idea of the blood, either from the auricles or ventricles, passing into successively emptied spaces, would, as we before remarked, not only violate physical conditions, and involve a perpetual waste of power, but be irreconcileable with the undoubtedly constant, though regularly accelerated, progress of the circulating fluid.

The blood from the pulmonary veins, is, in a state of health, *perpetually* entering the auricular cavity; for during the moment of systole its progress cannot be materially interrupted, both on account of the superior dimensions of the ventricular opening, contrasted with the pulmonary orifices, from the more favourable direction of the former for the effects of gravitation, and still more in consequence of the active dilatation of the ventricle itself, which forces, as it were, a particular direction upon the current, and acts more or less on the contents of the veins, as well as on those of the auricle. It is also evident, that the fresh quantity of blood propelled by each ventricular systole into the aorta, is not simply projected forwards, as would necessarily be the case were that vessel in the condition of an unyielding and empty tube, but it also re-expands the collapsing arterial tunics, by forming a sudden increase to their previous contents; thus exciting their elasticity, by which the force originally impressed by the ventricular contraction is perpetuated, and a continuous current is sustained by the reaction of the vascular parietes on the gradually diminishing fluid, until a fresh impulse and supply renew the original conditions, and the same operation is repeated. These reflections are equally applicable to the right side of the heart and its corresponding vessels; we may, therefore, safely conclude that the vascular and cardiac parietes are at all times in direct contact with their contents, on the same principles that the pulmonary and costal pleura never cease to be approximated, notwithstanding the changes which are continually taking place in the volume of the lungs.

Let us now apply these and other considerations to the action of the valves attached to the auriculo-ventricular openings. They are generally described as membranous laminæ, hanging loosely in the cavity of the ventricles, which, during the systole of the latter, are mechanically thrown upwards by the pressure of the blood; their retroversion into the auricles being impeded by the tendinous filaments attached to their edges. There are, however, several circumstances connected with the physical conformation of the heart, in a state of health, rendering, we think, this generally adopted mode of describing the action of the valves both vague and unsatisfactory. In the first place, we have shown that the auriculo-ventricular opening corresponds to the smaller or auricular portion of the ventricular cavity, and that during the systole of the latter, this region is almost obliterated; the opening itself is in consequence diminished, becomes nearly excluded from the aortic portion of the cavity, from which it is, in fact, separated by the anterior lamina of the bicuspid valve. In the second place, the two powerful muscular columnæ, to which all the chordæ tendineæ are attached, contract in common with the walls of the ventricle itself, and, from the radiated nature of their connexion with the valves, the latter are necessarily pulled downwards and inwards; their edges approximating to a common centre, and the whole forming the figure of an inverted cone, whose apex corresponds to the auricular portion of the ventricle. The valves are therefore *muscularly tightened* from the first moment of ventricular contraction; they are every where raised from the muscular parietes on which they passively repose during the systole, and their functions are completed by the fluid pressure in the ventricle bringing a greater or less portion of their upper surfaces into immediate and accurate apposition. This is very different from the usually received opinion of their being mechanically flapped upwards by the blood, and their progress suddenly

arrested by the limits of the chordæ tendineæ; an operation purely mechanical, sudden, and abrupt in its effects, and which would inevitably strongly react on the blood in the auricle, where we should expect it to produce venous regurgitation; as is actually the case when extensive dilatation occasions unnatural distension of the valvular surface*.

On the other hand, the mode in which we have supposed the valves to execute their functions is regular and uniform, produces no agitation or shock on the blood, and cannot, we think, be productive of sound in any way distinct from what results from the contraction of the muscular fibres to which the chordæ tendineæ are attached,—a sound which, if existing, can only form a part of, and must necessarily be confounded with, what results from the contraction of the ventricle as a whole. The strain upon the tendons attached to the valves, it must be recollected, is gradual, proportionate to the force and duration of the muscular contraction, and would take place in an empty as well as a full heart, minus the force exerted by the compressed fluid, which we have shown, by reference to the relations of the auriculo-ventricular opening to the aortic portion of the cavity, can only be actively exerted on a very small portion of the valves.

The hypothesis, therefore, that the first sound of the heart (Bouillaud, Rouannet, &c.) depends on the reaction of the blood on the mitral and biscuspid valves, is, we think, but very partially, if at all, correct, these valves being principally tightened by the muscular contraction, which, with the exception of instances of extreme dilatation, is never instantaneous. In the latter case, where the action of the heart is sudden, though weak, it is possible that the equally sudden tension of the tendinous attachments of the valves, considered in connexion with the increased surface exposed to the action of the fluid, may be the source of some trifling addition to the resulting sound.

It should also be remembered that the valves, when functionally active, are in contact with the blood both above and below; and that when driven upwards or downwards, the retiring surface never corresponds to an empty space, but is forcing before it a greater or less quantity of fluid, and they are therefore not favourably situated for the production of sonorous vibrations.

M. Bouillaud considers that M. Rouannet has shown himself somewhat too exclusive when ascribing the first sound *solely* to the play of the mitral and bicuspid valves, and thinks that we should take into the account the *sudden projection* of the sigmoid valves against the aorta and pulmonary artery. Now this imaginary cause of sound could only be efficient on the supposition that the space immediately above the valves was unoccupied, whereas we think that the preceding reflections have tended to prove that the interval between their upper surfaces and the arterial parietes is filled by the blood in common with every other portion of the cavity, and the valves, when thrown upwards by the passage of the ventricular contents, have first to expel the portion of fluid separating them from the artery, thus rendering their contact with the latter necessarily gradual, and in proportion unfavourable to the production of sound. The same reasoning will also apply to the supposition of this sagacious observer, of sound being produced by the sudden striking of the mitral and bicuspid valves against each other; for, as in the preceding example, the interval between these surfaces is occupied with blood until the moment of contact, while the fluid displaced by their approach retires into a full though distensible space, and offers throughout a certain resistance, which is opposed to the idea of sudden and abrupt collision. Indeed, under any circumstances, the sudden contact of membranous laminæ like the valves, immersed in a fluid, appears to us utterly inadequate for the production of appreciable sonorous vibrations.

The analogy, which some have strongly insisted upon, between the mitral and bicuspid valves and those of pneumatic or hydraulic pumps, is, we think, at best, very incomplete, if not wholly illusory. The solid structure of the latter would alone, where a question of sound is at issue, form a glaring and

* The accommodating power of the valves to the varying dimensions of the auriculo-ventricular opening, may be easily demonstrated by an examination of their physical structure, and is quite in harmony with the explanation we have given of their action; since the portion of the inverted cone which is compressed, may diminish with the increasing space, until all contact ceases, and the function of the valve becomes impossible.

essential distinction. Their play is also arrested by materials more solid and more capable of sonorous vibrations than the valves themselves; they are closed by the reaction of the fluid, are wholly unconnected with the passive parietes of the cavity they temporarily limit,—conditions which forbid any thing like a just and accurate comparison with the membranous and muscularly attached valves of the heart.

On the other hand, the analogy is in some of its leading features capable of being maintained with regard to the sigmoid valves; and it is still further supported by the results, viz. the production of a sound recognized to be valvular in its character by all.

We have thus successively directed attention to the form of the ventricular cavities, to their division into an auricular and vascular portion, to the respective relations of these to the auriculo-ventricular opening, to the hydrostatic conditions of the heart and large vessels, to the structure and mode of acting of the mitral and biscuspid valves, and, we hope, satisfied the mind that a correct estimate of the physical conditions, regulating both the heart and its contents, is of primary importance in any attempt we may engage in to elucidate either its action or its sounds; and, we think, proved that in some of the hypotheses hitherto advanced, these conditions have not been sufficiently attended to.

We have also successively excluded the descent of the blood from the auricle into the ventricle, the agency of conflicting currents and molecular collisions, and the shock of the blood either against the apex or larger vessels, as causes of sonorous vibrations; and by the method of exclusion have reduced ourselves to the admission, that the first sound is principally, if not wholly, produced by the contraction of the muscular parietes of the ventricles.

The fact of sound being developed by muscular contraction, was proved by the ingenious researches of Dr. Wollaston and others; and attempts to make this cause active in the production of the cardiac sounds may be traced in the writings of Harvey, Senac, Haller, Bichat, Corvisart, Laennec, &c.; but to Dr. Williams is justly due the credit of having made a scientific application of the fact, and of founding his investigations upon a careful and sagacious examination of the heart's form, functions, and contents. We cannot help thinking that this view of a long-debated question will ultimately prevail, when names shall be disassociated from facts; but while expressing our individual opinion, we are not forgetful of our liability to err; and the intention of the preceding observations has not been to prejudge a confessedly difficult question, but to fix the attention of observers upon certain data of the problem which cannot be safely omitted in our efforts to attain its satisfactory solution.

An experiment illustrative of some of the preceding statements is the following:—Immerse an Indian rubber bottle, about the size of the left ventricle, and with a proportionate orifice, under water, and supply the contracting power of the heart by external pressure with the stethoscope, applied at the same time to the ear. It will be found that the most sudden and forcible expulsion of the contents will fail to produce any appreciable sound depending on the compressed and rapidly ejected fluid, but whatever is heard may be distinctly traced either to the agitation on the surface of the water in the reservoir, or to the Indian rubber itself. Indeed, this last source of sound is equally active when the bottle is compressed in air, and has evidently no other origin than the parietes themselves. The elastic property of the bottle may very well represent the active dilatation of the ventricle, on whatever cause it may really depend; and if compressed under water, and allowed suddenly to expand, by quickly returning the stethoscope (contact being always preserved) *no sound* indicates the entrance of the external fluid; there is no rushing, or shock, or agitation, appreciable by the ear, and the diastole, in this instance, is certainly quite as sudden as that of the ventricles. The absence of sound, as in the case of the heart, depends upon there never being, at any moment, a space unoccupied by fluid, the entrance of the latter exactly accompanying the dilating walls of the cavity, the rapidity of whose diastole is wholly regulated by the greater or less size of the communicating orifice. When the opening is artificially closed, the elasticity of the bottle, though considerable, is incapable of producing the diastole, which can only take place in proportion as

the consequent empty space becomes filled by air or fluid. The entrance of the latter may be said rather to accompany than follow the expanding parietes. If a small quantity of air be retained in the bottle, both the entrance and exit of the fluid generate sounds. We are immediately sensible of agitation, bubbling, and conflicting currents, which persist while any air remains, but entirely cease when the latter is expelled, and fluid contact restored. We have already insisted on the application of these facts to the heart's action and sounds, and need not again revert to them.

Bath, March 5th 1836.

CASE OF

PARTIAL SPONTANEOUS HUMAN COMBUSTION,

WITHOUT ANY FATAL RESULT.

By James Overton, M.D.
Of Tenessee.

The subject of the following case is a gentleman about 35 years of age, middle size, light hair, hazel eyes, sanguineo-lymphatic temperament, of habits entirely temperate in the use of stimulating drinks of any kind, fermented or alcoholic, with a constitution considerably enfeebled from long and zealous devotion to the sedentary and exhausting labour of scientific investigation. In early life he was very subject to derangements in the functions of the stomach and bowels; and at the present time suffers frequently from different modifications of these maladies, as costiveness, occasional diarrhœa, acidity of the stomach, heart-burn, &c. &c., with their usual train of sympathetic affections, involving parts of the organism at a distance from the primary seats of diseases into a participation of their suffering.

At the time of the occurrence of the accident, he was afflicted with acidity of the stomach, and by an unusual and irritating quantity of the matter of urea in the secretion of the kidneys; for the relief of which he was in the habitual use of aperients, antacids, &c.

Mr. H., Professor of Mathematics in the University of Nashville, was engaged as usual in his recitation room, in attendance upon the morning exercises of his class, till eleven o'clock in the forenoon. He then buttoned his surtout coat close around him, and walked briskly thus clothed to his residence, a distance of about three-fourths of a mile, taking exercise enough to produce a glow of warmth on the surface of the body, without inducing fatigue, but feeling at the same time his usual acidity of the stomach, for which he resolved to take some soda as a remedy within a short time. Having arrived at his lodging, he pulled off his over-coat and kindled a fire, by placing a few pieces of dry wood on three burning coals which he found in the fire-place, of the magnitude of two-inch cubes each; and immediately left the fire, and retired to a remote part of the room, and made his observations on the weight and temperature of the atmosphere, as indicated by the barometer and thermometer, which were suspended in that situation. He then took the dew-point by the thermometer. These operations, together with the registration of their results, occupied about thirty minutes. This having been accomplished, he went immediately into the open air, made observations on the hygrometer, and was beginning his observations upon the velocity and direction of the winds. He had been engaged in this latter process about ten minutes, his body all the while sheltered from the direct impression of the wind, when he felt a pain as if produced by the pulling of a hair, on the left leg, and which amounted in degree to a strong sensation. Upon applying his hand to the spot pained, the sensation suddenly increased, till it amounted in intensity to a feeling resembling the continued sting of a wasp or hornet. He then began to slap the part by repeated strokes with the open hand, during which time the pain continued to increase in intensity, so that he was forced to cry out from the severity of his suffering. Directing his eyes at this moment to the suffering part, he distinctly saw a light flame, of the extent at its base of a ten cent piece of coin, with a surface approaching to convexity, somewhat flattened at the top, and having a complexion which nearest resembles that of pure quicksilver. Of the accuracy in this latter feature in the appearance of the flame, Mr. H. is very confident, notwithstanding the unfavourable circumstances amidst which the observation must have been made. As soon as he perceived

the flame, he applied over it both his hands open, united at their edges, and closely impacted upon and around the burning surface. These means were employed by Mr. H. for the purpose of extinguishing the flame by the exclusion of the contact of the atmosphere, which he knew was necessary to the continuance of every combustion. The result was in conformity with the design; for the flame immediately went out.

As soon as the flame was extinguished, the pain began to abate in intensity, but still continued, and gave the sensation usually the effect of a slight application of heat or fire to the body, which induced him to seize his pantaloons with one of his hands, and to pinch them up in a conical form over the injured part of the leg, thereby to remove them from any contact with the skin below. This operation was continued for a minute or two, with the design of extinguishing any combustion which might be present in the substance of his apparel, but which was not visible at the time.

At the beginning of the accident the sensation of injury was confined to a spot of small diameter, and in its progress the pain was still restricted to this spot, increasing in intensity and depth to a considerable extent, but without much, if any, enlargement of the surface which it occupied at the beginning. A warmth was felt to a considerable distance around the spot primarily affected, but the sensation did not by any means amount in degree to the feeling of *pain*. This latter sensation was almost, if not entirely, confined to the narrow limits which bounded the seat of the first attack; and this sensation was no otherwise modified during the progress of the accident, than by its increasing intensity and deeper penetration into the muscles of the limb, which at its greatest degree seemed to sink an inch or more into the substance of the leg.

Believing the combustion to have been extinguished by the means just noticed, and the pain having greatly subsided, leaving only the feeling usually the effect of a slight burn, he untied and pulled up his pantaloons and drawers, for the purpose of ascertaining the condition of the part which had been the seat of his suffering. He found a surface on the outer and upper part of the left leg, reaching from the femoral end of the fibula in an oblique direction, towards the upper portion of the gastrocnemii muscles, about three-fourths of an inch in width, and three inches in length, denuded of the scarf-skin, and this membrane gathered into a roll at the lower edge of the abraded surface. The injury resembled very exactly in appearance an abrasion of the skin of like extent and depth, often the effect of slight mechanical violence, except that the surface of it was extremely *dry*, and had a complexion more livid than that of wounds of a similar extent produced by the action of mechanical causes.

The condition of the pantaloons and drawers was next carefully inspected. The left leg of the drawers, at a point exactly corresponding with the part of the leg which had suffered injury, and at a point accurately correspondent to the abraded surface, were burnt entirely through their substance. They were not in the slightest degree scorched beyond this limit, the combustion appearing to have stopt abruptly, without the least injury to any portion of the drawers, which had not been totally consumed by its action. The pantaloons were not burnt at all; but their inner surface opposite to, and in contact with, the burnt portion of the drawers, was slightly tinged by a thin frost-work of a dark yellow hue. The material of this colour, however, did not penetrate the texture of the pantaloons, which were made of broad cloth, but seemed to rest exclusively upon the extremities of the fibres of wool which were the materials of its fabric. The colouring matter was entirely scraped off with the edge of a penknife, without cutting the woolly fibres, after which there remained upon the garment no perceptible trace of the combustion with which they had been in contact. The pantaloons may be said with entire propriety to have suffered no injury of any kind from the accident. The drawers, which were composed of a mixture of silk and wool, were made tight and close at the ankle, and tied with tape over a pair of thick woollen socks, in such a manner as to prevent even the admission of air to the leg through their inferior opening. Considering the injury not to be of a serious character, Mr. H. bestowed upon its treatment no particular care or attention, but pursued his usual avoca-

tions within doors and in the open air, which was very cold, until the evening of the succeeding day. At this time the wound became inflamed and painful, and was dressed with a salve, into the composition of which the rosin of turpentine entered in considerable proportion. This treatment was continued for four or five days, during which time the wound presented the usual aspect of a burn from ordinary causes, except in its greater depth and more tardy progress towards cicatrization, which did not take place till after thirty-two days from the date of the infliction of the injury. The part of the ulcer which healed last was the point of the inception and intensity of the pain at the time of attack, and which point was evidently the seat of deeper injury than any other portion of the wounded surface.

About the fifth day after the accident, a physician was requested to take charge of the treatment, and the remedies employed were such chiefly as are usual in the treatment of burns from other causes, except that twice a week the surface of the ulcer was sprinkled over with calomel, and a dressing of simple cerate applied above it. In the space between the wound and the groin there was a considerable soreness of the integuments to the touch, which continued during the greatest violence of the effects of the accident, and then gradually subsided, The cicatrix is at this time, March 24th, entire; but its surface is unusually scabrous, and has a much more livid aspect than that of similar scars left after the infliction of burns from common causes. The dermis seems to have been less perfectly regenerated than is usual from burns produced by ordinary means, and the circulation through the part is manifestly impeded, apparently in consequence of atony of its vessels, to an extent far beyond any thing of a similar nature to be observed after common burns.

Since the wound has healed the health of the patient has been as perfect as usual; and while the wound continued open, his ordinary occupations were interrupted by a week's confinement only to his chamber. The accident occurred on the 5th of January of the present year, the day intensely cold, and the thermometer standing at only eight degrees above zero, sky clear and calm, and the barometrical admeasurement of the atmosphere being 29·248 inches.

Such is the history of the case of partial spontaneous combustion, which has recently occurred in this city. The facts have been stated as nearly as practicable in the words of the sufferer himself, and are consequently entitled to all the credit attributable to any statement of a similar character, which is or can be supplied by the annals of the profession*.

ACCIDENTAL POISONING OF THREE NEGROES WITH CANTHARIDES.

RECOVERY.

By James Maxwell, Esq.
Surgeon to the Annotto Bay Marine Hospital.

Three healthy robust negro men, on Fort Stewart Estate, had a bottle of rum brought to them by a young man who stole it from the overseer's house, in which six drachms of finely powdered cantharides had been macerated eight hours previously, to apply to the ringbone of a horse. The person who stole it tasted it, but not relishing the draught, carried it to his friends, and informed them that he had found a bottle of bitters, which the white people occasionally used, and as he was not fond of bitters, they might share it amongst them. This happened about 8 P. M. in fine moonlight, and it was divided as equally as possible, by each person putting the bottle to his head, and taking what he considered to be his share. R. Pollock had eaten a hearty supper before he partook of it; the other two drank it upon empty stomachs, and ate their supper immediately afterwards. Shortly after they complained of sharp lancinating pain of stomach, burning sensation of the throat, with great nausea, which increased, and in a couple of hours they were seized with violent retching; the contents of their stomachs were mixed with blood, mucus, and froth.

The kernels of six antidotes (*Feuiella cordifolia*) were boiled in a pint of water, and given to each by the black dispenser, which acted as an emetic.

17th.—I saw them early next morn-

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* Amer. Journ. Med. Sciences, Nov. 1835.

ing. They are all complaining of excruciating burning pain in the region of the stomach; surface covered with cold clammy sweat, sense of constant burning of the throat, most intense at the top of the œsophagus, and descending along that passage to the stomach. There is great difficulty in swallowing, and to use the significant expression of one of them, his throat is on fire. There is great depression of the vital powers—they are incessantly moaning, and consider death to be inevitable; pulse accelerated about twenty beats in a minute, and rather weak. The breathing is not difficult, nor do they complain of any irritation of the urinary organs; no pain or tension of the abdomen: the bowels have not acted since they took the poison. The vomiting recurs at intervals, and they eject frothy mucus in great quantity, tinged with a vermilion colour. They are regularly salivated, spitting incessantly a mixture of mucus and saliva of a pink colour. The throat of each was examined, and found to be swollen; had an erysipelatous blush of inflammation, with turgid veins running across the fauces. To be perfectly satisfied that every particle of the powder was removed, I administered twenty grains of the sulp. of zinc to each, which acted instantaneously; they drank copiously of warm water, and after their stomachs were settled, three ounces of fine olive oil was given to each, which they retained. Enemata of the infusion of ochro, (*Hibiscus esculentus*,) with castor oil, ordered to be exhibited frequently, and the infusion to be drunk ad libitum.

The contents of the bottle were kept for my inspection; I had the dregs strained through blotting paper, dried in the sun, and ascertained that they had swallowed three drachms of the powder, and the whole of the rum, in which the cantharides had been macerated.

1 P.M.—They have had no recurrence of vomiting. One of them has had a copious alvine dejection, unmixed with blood. The symptoms continue the same as in the morning, with the exception of the stomach, which is less irritable. Two ounces of fine castor oil prescribed for each.

Evening visit.—The oil has operated on all; no blood has been passed by stool; they spit mucus and saliva tinged with blood incessantly. They complain of unremitting acute burning pain of stomach, with great difficulty of swallowing; thirst not urgent. Pulse of Pollock and Buchanan, 100. Mackintosh 80; no strangury. Blooded to eighteen ounces. Applic. emplast. lyttæ. regioni gastricæ. The enemata were directed to be given occasionally, and the ochro tea to be used as common drink.

18th.—R. Pollock's blood exhibited no buffy coat; oil operated freely, and since the emetic has had no irritation of stomach. There is neither blood in his stools nor urine, but he now complains of severe strangury, and the urine comes away in small quantities. The gastric symptoms are not so urgent; throat still painfully affected, inflamed and covered with coagulable lymph. He is constantly spitting a frothy matter mixed with blood; great thirst; no heat of skin; pulse 104, regular and full; tongue highly furred, with red edges. There is no fœtid smell of the breath; the gums are red and swollen, and he states the pain to be most severe at the top of the œsophagus, diminishing as it descends to the stomach.

George Buchanan.—Pain in the region of the stomach abated; throat very painful, with tumefaction of the tonsils, and relaxation of the uvula; the palate is suffused with a slight vermilion blush, deepest towards the fauces. The irritation of the bladder not so great as in Pollock; pulse 108, full; tongue furred and red at the edges; great difficulty of swallowing, with occasional eructations of sour frothy mucus, tinged red.

R. Mackintosh.—Gastric pain moderated; throat very painful, with difficulty of swallowing; tonsils swollen; uvula lengthened as in cases of catarrh; foul tongue with red edges; pulse 104; no fever; strangury very troublesome; continues to spit constantly, with an occasional eructation of frothy mucus, which he says is as sour as "lime-juice." The oil was repeated—fomentations to the region of the bladder, with opiate clysters, and warm bath.

19th.—Pollock.—Pain of stomach much relieved; throat swollen, and on the back part of the fauces there is an aphthous ulcer the size of a sixpence, covered with a whitish adherent crust; another of the same description on the side of the right tonsil. Sublingual

glands slightly inflamed and tumefied; gums red, with a copious secretion of saliva, which causes his mouth to stream as if he was under a course of mercury. Pulse 98, soft and full; strangury excessively severe; he passes his urine guttatim, and, to use his own expression, it burns him " like a pepper."

Mackintosh.—Describes the pain of stomach to be rather severe to-day; pulse 120, with thirst; no febrile heat; gums and inside of the mouth inflamed and ulcerated, and he says that he feels exactly the same as he did when lately salivated with mercury, only has no coppery taste in his mouth; throat still inflamed; difficulty of swallowing great; strangury troublesome.

Buchanan.—Feels better; throat still inflamed; pulse 100; completely salivated, with ulcerated gums; strangury much alleviated.

20th.—There is an amelioration of all the symptoms: the strangury has left Buchanan; the other two are still slightly affected with it; the salivation continues in all.

21st.—They are improving.

24th.—Strangury gone; salivation abating; throat and mouth getting better. From this time they gradually recovered, and in two weeks were perfectly well.

The recovery of these persons from the effects of a highly corrosive substance, may be considered a fortunate circumstance, and in a great measure may be attributed to the speedy ejectment of the poison from the stomach. Not less than a drachm of cantharides, mixed with about six ounces of rum, could have been taken by each of them, but as they filled their stomachs with their favourite supper meal, its effects were not so immediately palpable as it otherwise would have been. They admitted that the stolen liquor had rather a nauseous taste, but as it was given to them as bitters used by the white people, they shared the prize, and soon afterwards began to suspect that they had been imposed upon. The pain increased rapidly, but, conscious of their guilt, they were afraid to call for assistance till they could no longer conceal it, but so soon as severe vomiting commenced, they suspected that they had drank a poisonous liquid, became alarmed, and told the truth.

The antidote, a favourite remedy amongst negroes for all such accidents, was administered, and its over-dose acted as a powerful emetic, relieving the stomach of its entire contents. Not satisfied with what had been done previous to my arrival, I immediately gave an emetic, to ensure the complete ejectment of the poison, and directed large quantities of the bland mucilaginous infusion of ochro to be liberally used, with oil to sheath the coats of the stomach from the escharotic effects of the cantharides.

Magendie has warned us against exciting the activity of the absorbents by venesection, in cases of poisoning, and following the recommendation of that distinguished physiologist, I abstained from abstracting blood, till I had reason to think that the powdered cantharides were effectually removed by vomiting and purging. It was not till towards night, when the gastric symptoms assumed an alarming aspect, and threatened active inflammation, that I had recourse to the usual efficient means of subduing it. The throat was early affected with inflammation, and the red excoriated state of the tongue may have been occasioned by a few particles of the powder having lodged there.

It is a singular circumstance, that previous to venesection, not one of them had the least irritation of the urinary organs; but very soon afterwards they were seized with a very severe form of spasmodic strangury, attended by severe scalding, long before it could be presumed that the blisters could effect such a result.

It is well known to those conversant with negroes, how partial they are to blisters, for the most trifling ailments, and how little susceptible they are of strangury; for in ninety-nine cases out of a hundred, that highly painful affection is unknown amongst them from the absorption of blister-flies. It is reasonable, therefore, to infer, that the application of the blisters had little, if any, share in exciting the vesical irritation, but that the agency of the absorbents was increased by lessening the circulating fluids, so as to produce the phenomena in question.

I may here remark, how fallacious are all those agents, so confidently recommended for alleviating the excruciating sufferings from strangury. I never found camphor, either sprinkled on the blister, or given in large doses,

of the least utility. The only remedies which offer any positive relief in severe cases of strangury, are anodyne clysters, flannel wrung out in hot water, to which laudanum has been added and applied over the pubis, with the administration of an opiate, where circumstances will admit*."

CASE OF

DISLOCATION AND FRACTURE OF THE SPINE;

TERMINATING FAVOURABLY.

By Gardiner Dorrance, M.D.
Of Amherst, Massachusetts.

In this journal, for May, 1835, a case of fractured spine is reported, in which depression of the spinous process was removed by an operation. The case proved fatal; but the operator thinks an elevation of the depressed process furnishes the only hope for the patient, and therefore recommends it.

About six years since, Amos Marsh, of Sunderland, while at work in the woods, was struck by a falling tree, and bent to the ground. I saw him soon after he was removed to his house. I found him in bed, saying, that both his thighs were broken. Finding them straight and firm, I suspected loss of sensation in them from injury of the spinal cord. Turning him on his side, I found an angle at the eleventh dorsal vertebræ, of forty-five degrees.

It looked like so easy a thing to make the spine straight, that I could hardly resist the inclination to put it so; and the by-standers were impatient at my hesitation to do it. I supposed there was partial dislocation of the vertebræ, which any attempt at reduction would probably make a perfect one. I knew, too, that dislocation could not take place without fracture of the spinous or transverse process, and that loose spiculæ of bone would very possibly be driven into the spinal marrow, and cause instant death. A consulting physician, who saw the patient some hours after, was anxious to attempt a reduction; and when dissuaded from that, proposed cutting down and removing the broken and probably depressed fragments of bone. It was, however, concluded to trust the patient to nature, using bleeding and low diet, to prevent, as far as possible, inflammatory action in the injured part.

Mr. Marsh had for a number of weeks almost perfect paralysis of the lower limbs, and of the lower abdominal viscera. Urine was drawn off by the catheter, and the bowels moved by stimulating injections. By degrees, sensibility and mobility returned to the limbs, and the bladder and rectum resumed their functions. In four months the patient walked with crutches, and in six with a staff. In less than a year he resumed his trade, that of a cooper, and he now performs as much labour, sometimes in his shop, and sometimes in the field, as most men of his age. There is a stooping of his back, and a sideway motion to his gait. The vertebræ are not in place, though more so than at first, and I believe much more so than art could have placed them.

The palsy of the parts below the injury shows that the spinal marrow was compressed, either by displaced vertebræ, or by the depression of their spinous processes. Nature has by some means gradually removed the compression. In the hurry and agitation of such an accident, the physician wants some rule of practice to guide him. From the fatality or permanent paralysis of the lower extremities, which have attended all the cases I have known of, where reduction of dislocation or removal of depression have been attempted, I consider the favourable result in the case of Mr. Marsh may encourage us to wait and hope. The curative powers of nature are often greater than we are disposed to believe them*.

SPASMODIC ASTHMA:

WHETHER RELIEVED BY DESCENT IN A DIVING BELL.

To the Editor of the Medical Gazette.

Sir,

There was lately pointed out to me at Portsmouth, a man who has been engaged in raising various valuable stores out of vessels which have sunk in the

* Jamaica Physical Journal, May, 1835; and American Journal of Medical Sciences.

* Amer. Journ. Med. Sciences, March, 1835.

channels leading into the harbour; and a circumstance was related of him which has excited so much my curiosity, that I am anxious, through the medium of your valuable journal, to get at the particulars from some medical practitioner resident near the spot.

It appears that the man was originally a pilot, and from his childhood suffered from spasmodic asthma; but that since he commenced his present occupation, in fact after his third submarine trip, the disease has *totally* disappeared. Now that the disease was *bonâ fide* spasmodic asthma, Dr. Bree's disease, is the first fact that I wish clearly to ascertain; next, that after he had been three times immersed in the condensed atmosphere of the diving-bell, a complete remission of the symptoms ensued.

I need not offer any apology for troubling you; the least glimpse of anything which can mitigate so very disagreeable a disease is too important to be overlooked.—I am, sir,

Your obedient servant,

R. TEMPLETON.

Woolwich, March 22, 1836.

ANALYSES AND NOTICES OF BOOKS.

"L'Auteur se tue à allonger ce que le lecteur se tue a abréger."—D'ALEMBERT.

A Practical Essay on the History and Treatment of Beriberi. By Assistant-Surgeon JOHN GRANT MALCOLMSON, Madras Medical Establishment.

Essay on the Origin, Progress, and Treatment of Cholera; with Remarks on Berriberri and Diet. By JAMES BANKIER, M.D. of the Royal Navy, Madras. 1835.

BERIBERI, the very remarkable disease of which an account is given in the above essays, has for some years attracted a large share of attention from our brethren in India; except cholera, no malady has given rise to more difference of opinion either as to its pathology or its treatment. The former of the works before us contains by much the more full and satisfactory history of beriberi, Dr. Bankier having only appended some remarks upon this disease to a volume professedly upon cholera.

Mr. Malcolmson has both enjoyed extensive opportunities of investigation, and has brought to the task an intelligent and cultivated mind: his essay was printed by order of the government at Madras, and as a copy has by good fortune fallen into our hands, we are sure our readers will be glad to be put in possession of information which probably very few, if any of them, would otherwise be able to obtain. Nor let it be supposed that what follows relates only to a disease which is limited to tropical climates, and therefore cannot come under our observation in temperate regions;—cholera has shewn the fallacy of trusting to such grounds of exemption; besides which, beriberi bears a very close resemblance on the one hand to paralysis, and on the other to rheumatism; presenting as curious a subject of study and investigation as any we are acquainted with in the whole range of nosology.

Beriberi [or Berriberri] is attended by a peculiar affection of the lower extremities, sometimes acute and sometimes chronic—often extending to other parts, particularly the chest. The following is a general account of the symptoms:—

"It usually commences gradually, with a feeling of numbness, sense of weight and slight weakness and stiffness below the middle of the thighs, sometimes preceded by muscular pains. There is slight œdema of the feet and legs, especially along the tibiæ, often found to come on after the other symptoms. The walk is unsteady and tottering even when the patient is not aware of weakness in the limbs, which are occasionally tremulous; spasms occur in the calves and soles of the feet, sometimes becoming general, and occasionally shooting to the chest and larynx, obstructing respiration and speech. The want of power often rapidly increases to almost total palsy, especially of the extensor muscles, and, in a few cases, the patient after slight indisposition suddenly loses the use of his legs. Rigidity and various painful affections of the nerves accompany the paralytic symptoms; and there is sometimes pain along the spine, commonly at the two last lumbar vertebræ. In some cases the disease goes no further, and a cure is effected: but, more frequently, the numbness extends upwards towards the abdomen, there is general sense of lassitude and aversion to motion, and the

hands, arms, and chest, (and in a few cases even the neck and lips) are gradually benumbed. There is oppression and weight at præcordia, dyspnœa on slight exertion, diffused and irregular pulsation in the cardiac region, and the face and hands are puffy and œdematous. The patient is often found dead in bed, or sinks after several fainting fits or throbbings at the heart; or the œdema rapidly increases and extends up the trunk, violent dyspnœa and inability to lie down in bed comes on, with anxiety, cold sweats, cold extremities, rapid feeble pulse, urgent thirst, and partial suppression of urine. At the commencement the urine is always scanty, of a deep red colour, without cloud or sediment, and possessing very peculiar properties; in some old cases it becomes copious, turbid, and pale, with a large white deposit, and is passed, with pain, from an irritable bladder. The stomach is irritable in many bad cases, and pain and tenderness in the epigastrium is sometimes complained of; there is, in a few, pain in the abdomen, or a sense of heat is diffused over it and the chest. Effusion takes place into the chest, and more rarely into the abdomen, and there are now and then some signs of inflammation of the pleura or bronchi. In the early stage, the pulse may be full, hard, and frequent, or little altered; when the face is puffy, and there is weight and oppression at the præcordia, it is quick, often irregular, and usually small, although it is occasionally strong.

"Various dyspeptic symptoms occur; the bowels are often costive, the stools green and variously disordered, and the eyes are often tinged yellow. The skin is rather cold, unless there is pyrexia, which is often present in the evening. The disease is sometimes fatal in a few hours, but is often chronic, and in these, the patient is liable to sudden death, to rapid aggravation of the symptoms, or supervention of new and more formidable ones, by which he is soon carried off; and if he survives these, he may live for a long time, bed-ridden, dropsical, and a true paralytic."

Of the causes of the disease so very little is known that we need scarcely do more than allude to them. It prevails more among the Mussulmauns than the Hindoos, and evidently runs in families, but it is not suspected of being contagious. Something, however, seems to have been made out by Mr. Malcomson as to the pathology of beriberi, which he has rendered it probable consists in disease of the lower portion of the spinal cord; indeed, he has given some dissections which clearly prove the parts to have undergone a kind of wasting. He says, describing such a case, "the substance of the cord was perhaps softer than usual, and the nerves of the cauda equina appeared red, but on examination it was found that this arose from an almost total decay of the white nervous matter, allowing the vessels of the membrane to shine through, and not from any unusual vascularity. There were no marks of previous severe inflammation of the membranes." This state of the spinal cord will satisfactorily account for the paralytic condition of the lower extremities, as well as for the remarkable changes which the urinary function undergoes.

The paralysis is thus described—

"Palsy appears to be the most constantly present of all the muscular symptoms, generally coming on slowly, but now and then very rapidly, the knees being suddenly so weakened, that the patient on awaking from sleep has been unable to rise without assistance; in the majority of recent cases spasmodic rigidity is also present, and in a few, the flexor muscles were permanently contracted. The recti muscles of the abdomen, in one instance, felt hard and contracted, the patient comparing them to sticks. The cramps are most distressing in the calves of legs and soles of feet, and in one example the muscles of the back were thrown into such rigid contraction as to give the appearance of opisthotonos."

And again, in a report by Dr. Herklots, a surgeon in the Indian army:—

"Though the patients are sometimes first affected with a sense of stiffness of the joints, previous to any other symptom supervening, yet in the disease as it usually occurs here, the patient generally first complains of pain, occasionally confined to the joints but more frequently affecting the muscles of the legs and feet; (or he is first of all affected with spasms); to these in a few days succeed œdema, then numbness, spasms, diminution of muscular power, &c. But this is not the course the disease invariably follows; at times the patient gets up from his sleep of a morning with a slight numbness in his feet, which some-

times very gradually, at other times rapidly ascends upwards, occasionally accompanied with œdema, which then keeps pace with the numbness, extending in some cases throughout the whole body. At other times, again, the patient goes to bed in perfect health, and awakes in the morning, to his great surprise, with a weakness or total loss of power in the motion of his limbs. A few days after, the patient complains of more or less violent spasmodic contractions of the muscles and tendons, in the generality of cases confined to the hamstring muscles, calves of the legs, or tendines achilles; but sometimes affecting all the muscles of both the upper and lower extremities, and in a few rare instances, almost every muscle in the body. In one case, the patient complained of spasm at the pit of the stomach; in another, of a feeling of hardness in the calves towards the evenings, which grew soft again in the mornings following; during this period sometimes, the patient complains of pain in the affected parts, or of all the *joints* of the body or throughout the whole system; at other times only of soreness on pressure.

"The diminution or loss of power of voluntary motion occurs in very different degrees; in the case of Cumboo, the patient had not the slightest power to move himself; could not raise an arm or a finger, much less turn himself from one side to the other: in this case it was something more than paraplegia; it consisted of total loss of power (we may say) of almost every muscle in the body. More frequently, however, the weakness is in the hip, knee, ankle, finger, or toe joints, either singly or in combination. If œdema is present, these commonly don't show themselves till the swelling goes off; and as the latter symptom gradually disappears, the former progressively increases. If the loss of muscular power exists in the loins or hip-joints, the patient is unable to get off his cot or move his lower extremities; if lifted up by a couple of people he is like a piece of dead weight, perfectly unable to afford the smallest support to his body by his legs. If the loss of power is not quite so great, when raised by assistants, it is as much as he can do to lift his legs alternately and place them forward as in progression. When the loss of power exists in the knee joints, it is then only that the patient has the gait of the sheep; and if in that case it exists in a still severer degree the patient has a tottering in his walk; and th ugh he uses a stick as a prop, he is every moment in danger of falling, unless he obviates the tendency by particular attention, for kis knees give way under him. The loss of power is sometimes confined to the ankle-joints; in which case, as the patient lifts his legs off the ground, the feet dangle from the ankles, as a broken bone does from a fractured part; the patient has no power to bend the feet, and on resting the body on one foot it bends outwards and gives way under the weight; in several of these cases, the patients felt exactly as if, at every step, some one gave them a push from behind and they were in danger of stumbling. Loss of muscular power in the finger and toes is generally most severe in the thumbs and great toes; these have in several instances been perfectly immoveable. Sometimes all the fingers and toes of both hands and feet have lost their power of motion, (the patients being perfectly unable to bend their fingers so as to approximate them with the palms of their hands, or raise their toes off the ground); while at other times, only one or more are so so affected. Some of these have also acquired, in a few cases, a considerable degree of stiffness, and they could not be brought in contact."

This will serve as a description of the characteristic symptoms of this singular disease.

The changes in the urine are analogous to those which take place in other cases of spinal disease. It is usually more or less turbid when voided; but sometimes becomes more so after having stood for some time. The presence of ammonia soon becomes evident. The complication of affections of the heart and dropsy are elaborately detailed, and we know of no account of this disease in which nearly so much information regarding it is to be found; our space, however, only admits farther of a short analysis of the treatment, the account of which is far from satisfactory—not as to Mr. Malcolmson's details, but as to the general results of the remedies adopted.

Bleeding, which has been very strongly recommended by some, is held by our author to require very great caution, and only to be admissible at the onset of the disease; if the case be chronic, "the relief from bleeding is little mani-

fested, notwithstanding the hardness of the pulse." Mercury seems only to be useful when there exist inflammatory symptoms, requiring bleeding: the nervous symptoms are increased by it, and the general inference deduced from actual experience is much against it.

Purgatives are often of essential service, and of these the compound powder of jalap has been found to answer best.

Diuretics are of much assistance in removing the œdema, and especially in preventing the spread of the anasarca upwards; but "to this their usefulness is limited." Of diuretics the supertartrate of potass is most strongly recommended; and this, it will be observed, is in keeping with the purgative above recommended. Stimulants, particularly laudanum in combination with ether and ammonia, are to be diligently employed where there are paroxysms of dyspnœa coming on suddenly, especially if they be of spasmodic character, and attended with feeble pulse: the extremities and trunk are also to be rubbed with stimulating liniments. Cinchona has no power upon the proper symptoms of beriberi, but is sometimes useful in those cases in which ague is combined with them. Counter-irritation constitutes a very important part of the treatment. Blisters to the spine are spoken of with more commendation than cupping, or than other forms of exciting cutaneous inflammation.

Exercise and change of air do very little for the patient, and no more for his convalescence than on the general principle of tending to remove the debility which remains after any severe disease.

Galvanism, from want or imperfection of instruments, has been used but seldom, and the results are only such as show the propriety of making farther trials of this agent.

In conclusion we may remark, that the work contains a great deal of original and valuable information, but that it is very indifferently arranged, and embarrassed by digressions,—imperfections which the author might remove in a future edition, without much trouble to himself, and with great advantage to the reader.

The subject is interesting, and one about which we have very little information of a satisfactory nature—at least in this country.

Examen des Doctrines Médicales et des Systèmes de Nosologie de MM. Laennec, Louis, Andral, Lallemand, Bouilland, &c. Par M. F. J. V. Broussais. Deux tomes. Bruxelles. Dulau.

We have here a handsome reprint, in two very neat little volumes, of the celebrated critique by the founder of what is called *physiological medicine* in France. The name of Broussais is famous far and wide; yet the appearance of the *Examen* in its present popular shape cannot fail to extend the author's celebrity further still. The work itself is well known, being an able *resumé* of the views of almost all the eminent French nosologists of modern times, shewing how far they coincide with the Broussaian principles. Prefixed to the critical portion of the work we have a lucid exposition of the doctrines of the author, stated in 468 propositions, or aphorisms; so that the whole may be looked upon as a compendium of physiology, pathology, and therapeutics, peculiarly French.

MEDICAL GAZETTE.

Saturday, March 26, 1836.

"Licet omnibus, licet etiam mihi, dignitatem *Artis Medicæ* tueri; potestas modo veniendi in publicum sit, dicendi periculum non recuso."

Cicero.

HOW TO DEAL WITH QUACKERY.

We are not very sanguine, nor ever have been, about the possible extirpation of quackery from this or any other country. Let laws do their utmost—let pains and penalties be enacted, till there scarcely seem a loop-hole left for the most clever charlatan to gain admittance into the community,—still, as long as there are knaves and dupes in the world, charlatanism, under that or some other name, must flourish. Perhaps when Astræa returns with another golden age, quackery will be seen and heard of no more: but we fancy we must wait patiently until then.

It is rather amusing to notice the

grand scheme of a worthy contemporary of ours, by which he intends to give a complete *coup de grace* to quackery in England. Medical practitioners throughout the country are to enter into a solemn league and covenant to give the rampant monster no quarter; they are to have a grand central Board in the metropolis, always sitting, attended by well-paid functionaries, for the despatch of business; and that business is chiefly to consist in the gathering of information against the enemy, publishing that information, and disseminating it throughout the land in the shape of tracts, like those scattered abroad with such signal success by the Anti-gin-drinking societies. For this laudable purpose, and in furtherance of so wise an expedient, all the legally qualified medical men in England are to *subscribe*, and to form a body, the ramifications of which may be found everywhere, — the whole to be most appropriately designated by the imposing title of the Anti-Medical Quackery Society.*

A more ridiculous project, we believe, has rarely been devised. Legitimate practitioners, forsooth, are to form themselves into a regular *union* for the suppression of charlatanism, and to don the title of the Quackery Society—anti-medical, too. Was ever such stupid short-sighted blundering? Why, if the great body of quacks were themselves to combine against the profession, they could not adopt a more appropriate title than the Anti-medical Quackery Society. Yet such is the sort of union of practitioners seriously recommended by our contemporary, and for the organization of which a liberal subsidy is to be raised. Who but the wiseacre who suggests the scheme is ignorant of what would be the result of such a combination? A rich harvest to every daring empiric, bold enough to face the league of medical practitioners. To those other merits and qualifications which ground his present claims to public patronage, would then be added that most captivating one—of persecution. The quack would immediately hold the respectable rank of a martyr to the public cause, and would reap his profits accordingly. The practitioner, on the other hand, acting in concert with the Anti-quackery Society, would stand in the odious light of a monopolist—an enemy to improvements in science—and, worse than all, a self-interested opponent of what would be insisted on as the people's right to choose whom they please for their medical attendants. A brighter prospect could not be desired by quacks, charlatans, and empirics of every description, than this sort of conspiracy against them by regular practitioners. The tables would be completely turned: the Morisons, the Eadys, the successors of St. John Long, and the whole tribe of mountebanks, would then be in all their glory---raised to a pitch of importance, which they never before even dreamed of. Yet the wight who has found out this wonderful scheme of putting down quackery — by instituting his Anti-Medical Quackery Society—cannot see so plain a result. Can any mole be blinder?

It is too absurd to waste time in pointing out the silliness of such a project. Quackery can never be suppressed by authority, by persecution, or by force of law. The experiment has been tried again and again. There are strong laws against quackery in France; yet no where does the charlatan thrive with more baleful luxuriancy. In countries, too, such as Prussia and Austria, where the form of government is in a great measure despotic, at least decidedly military, enactments have been made for the extirpation of quackery;

* See Lancet, March 12, 1836.

yet all to no purpose. Certain simple-minded persons, however, think that the object might be accomplished in *this* free country—where it is the jealous pride of all ranks to do as they will with their own—and particularly, to be "doctored" in whatever way they think proper!

What, then, it may be said,—is nothing to be done to check the monstrous evil? Are the growth and spread of quackery to proceed without any effort to control them? Our views on this head have often been stated to our readers. We are the last to advocate anything like a passive indifference on the subject, and more than once have we maintained that the prominent position occupied by the charlatans of the day is mainly owing to the apathy of the regular practitioner. This, however, is far from holding that medical men should combine, in the form of quackery or anti-quackery societies. It by no means follows that because we would have the legitimate member of the profession up and stirring for the maintenance of his rights and the repression of intruders, we should therefore urge him into the extreme foolish predicament of *conspiring* to put down his opponents—conspiring with his brethren in the face of the public—and thereby giving the strongest possible hold to the obnoxious party to secure their usurped position.

What we have all along recommended is this—that medical men should no longer make a merit of their contemptuous indifference towards quackery: reason there is none why their voices should not be uplifted on every occasion where charlatanism in any shape comes before them; and they should remember that if *they* are silent, there is no other source of censure to keep the mischief in abeyance.

The proper mode, therefore, of proceeding, naturally suggests itself: in the first place, it would seem advisable to put off that apathetic bearing which is so detrimental to the cause; and, secondly, for each member of the profession *individually* to exert himself within the sphere of his particular influence. Let the legitimate practitioner avail himself of the occasion, whenever it offers, of shewing up charlatanism in its true light; let him point out the dangers which attend the destructive use of nostrums; and, above all, let him clearly demonstrate the folly and wickedness of those who pretend to cure all diseases, in all constitutions, and in all circumstances, by one universal remedy. It is in this way only that that sort of knowledge can be diffused, which, if anything can, will put quackery to the rout. It is useless to expect aid from the legislature, encouragement from the press, or any immediate support from the "expanded intellect" of the age. *Aide-toi* should be the maxim of every medical man, when he has quackery to deal with; and the true way of putting the maxim into practice, is not by seeking vicarious assistance, but by using every rightful personal exertion.

Another rule of conduct we would also beg to suggest, though it is properly, perhaps, only a corollary of the preceding; and that is, never to give countenance to any *patent* nostrum—never to grant permission to a patient asking our leave to take, or continue to take, any pills, boluses, cordials, essences, or plasters, sold stamped in the shops as specifics. We are convinced that the easiness with which medical men are induced to give their consent, and to tolerate such practices, has a potent effect in the extension of the mischief.

Such are the short and easy methods which we recommend for dealing with the quack nuisance. We have long since advocated the plan; and are led to recur to it now, partly on account of

the formidable advances lately made by the evil in question, and partly because we perceive a foolish guide attempting to mislead the profession. To all such as might be simple enough to fall into the snare, we beg to repeat our warning: if they would contribute to the suppression of quackery, let them really take the matter into "their own hands;" let them employ *personally* their best zeal and diligence, but take heed how they patronize anti-quackery unions.

PARLIAMENTARY MEDICAL COMMITTEE.

We understand that the Medical Committee will be revived, in order to enable Mr. Warburton to report upon the evidence taken during the last parliament. That gentleman is again engaged on the subject, and is collecting further information. We hear that some difference of opinion exists between him and the government as to the new arrangements connected with the medical profession.

APPOINTMENT OF DUPUYTREN'S SUCCESSOR.

M. SANSON.

The important professorship of *Clinique Externe* has at length been committed to M. Sanson, after a well-contested *concours* of about two months' duration. This gentleman was all through the popular favourite, and acquitted himself in every stage of the contest with marked success. Yet the mode in which the judges voted for his appointment is curious. At the first *tour de scrutin*, MM. Marjolin, Roux, Lisfranc, and Murat, voted for M. Blandin; MM. Velpeau, Richerand, P. Dubois, and J. Cloquet, for M. Berard; MM. Chomel, Gerdy, and Reveillé Parise, for M. Sanson. But the second *tour* brought all M. Berard's votes to M. Sanson; M. Murat also gave his vote in M. Sanson's favour: so that the election was carried by eight to three against M. Blandin. It is worth adding, that M. Lisfranc supported the latter candidate throughout. Such is a specimen of the *concours*.

M. PARENT DU CHATELET.

Medical science has suffered a severe loss in the death of this gentleman, which took place recently at Paris, from an affection of the brain. M. Parent du Châtelet was chiefly distinguished in the department of public *hygiène;* his works insured for him a high and extended celebrity; and his reputation, founded on his persevering and indefatigable labours, will long remain permanent and solid.

CRUSHING THE STONE IN THE BLADDER.

To Sir Benjamin C. Brodie, Bart.

My dear Sir Benjamin,

I am very much obliged to you, and I shall do what you suggest.

In our day it is very difficult to perform our duty to the hospital pupils, and to be on our guard lest we hurt some one's feelings by having our unpremeditated expressions printed.

Ever truly yours,

Charles Bell.

Brook-Street,
March 23, 1836.

On reading your note a second time, I think you should permit me to send it to the Gazette, it is so creditable to the Baron.

To Sir Charles Bell, K.H., &c. &c.

My dear Sir Charles,

I have been reading your very interesting lecture on the operation of crushing stones in the bladder, in the last number of the Medical Gazette; and I think it right to trouble you with the following observations on one or two points referred to in it.

In the case which I attended with Dr. Hume, and in which we afterwards called you to assist us in consultation, Baron Heurteloup used, not the three-clawed instrument, which you so justly reprobate, but the curved sliding forceps, similar (as I apprehend) to those which you recommend, and which you employed in the operation described in your lecture. Unfortunately the steel had not been properly tempered, and, in consequence, instead of completing the work of crushing the stone, the blades bent. I am sure that you will agree with me in thinking, that it would be hard to throw the entire blame of such an accident on the operator. I have never heard of a similar accident having occurred since.

The first person who actually applied

the curved sliding forceps to the purpose of crushing stones in the bladder, was Baron Heurteloup. This I know to have been the case; and I cannot but think that the profession and the public are very much indebted to him for this very great improvement in the operation. In fact, he made that easily practicable, which was the next thing to being impracticable in M. Civiale's operation with the straight three-clawed forceps.

The principal improvement made by Mr. Weiss is that of appling the screw instead of the hammer, for the purpose of crushing the stone; and in my judgment a great improvement it is; but still the principle of the operation is the same, wheth r it be performed with Mr. Weiss's screw, or Baron Heurteloup's hammer.

Being fully assured as I am, that it is your earnest wish to deal justly and generously by every one, I do not doubt that you will excuse the liberty which I take, as an old friend, in correcting these mistakes into which you have accidentally fallen.

Believe me to be always,
Yours most truly,
B. C. Brodie.

14, Saville Row,
March 22, 1836.

INTERVIEW

OF A

DEPUTATION OF THE BUCKS MEDICAL ASSOCIATION WITH LORD JOHN RUSSELL.

PROTEST AGAINST THE REPORT OF THE POOR-LAW COMMISSIONERS.

To the Editor of the Medical Gazette.

Sir,

I beg permission to forward you, for early insertion in your journal, an abstract of the minutes of a meeting of the Bucks Medical Association, a copy of a Protest drawn up at the desire of that meeting, and a brief account of the reception and proceedings of a deputation which waited on Lord John Russell with the Protest.

I have the honour to be, sir,
Your most obedient servant,
Robert Ceely,
Hon. Sec.

Aylesbury, March 19, 1836.

At a meeting of the Bucks Medical Association, held at Aylesbury, on the 23d November last, — Richard Steel, Esq., Berkhampstead, in the chair,—it was resolved —

"That a Protest against the 25th section of the first Annual Report of the Poor-Law Commissioners should be drawn up by the committee, which, after signature by members of the profession *in* and *near* the county of Bucks, should be presented by a deputation to Lord John Russell."

It was also resolved—

"That Mr. Nathaniel Rumsey, of Beacon field; Mr. William Robarts, of Burnham; and Mr. Robert Ceely, of Aylesbury, do form the deputation for presenting the said Protest."

In compliance with these resolutions, the following Protest was drawn up, and received the signatures of 115 medical practitioners, including those of several physicians:—

To the Right Hon. Lord John Russell, His Majesty's Principal Secretary of State for the Home Department, &c. &c.

We, the undersigned, practitioners in medicine and surgery, in and near the county of Buckingham, beg permission to submit to your lordship our PROTEST against certain statements contained in the 25th section relative to medical relief) of the first annual report of the Poor-law Commissioners for England and Wales; on the following grounds:—

1. Because those statements are calculated unjustly to impress the public mind with sentiments of disapprobation, jealousy, and suspicion, towards the medical profession.

2. Because they are intended unfairly to reconcile the community to the adoption of a system of "medical relief" incompatible with a humane consideration for the sick poor, and subversive of the proper ends of medical exertion.

3. Because, instead of impugning the character of the medical profession in general, and of the class of parochial surgeons in particular, candour and justice would have made some mention of the vast amount of their laborious and gratuitous services.

4. Because the measures adopted "as a check to the general expense of medical relief" are not warranted by the previous well-known inadequacy of the remuneration for medico-parochial duties; neither are they calculated "to secure to the paupers the best treatment."

5. Because the omission from the old medical contracts, of any provision for

non-parishioners, arose from the absence of any legal claim for such provision, and from the consequent want of concert and reciprocity of arrangement among parishes, and not from any "expressed or complied condition" with the contractors.

6. Because we are satisfied that there is no ground for alleging, that the charges for non-parishioners, "in the great majority of instances," were at the "highest rates," but that, on the contrary, they have been much below what professional usage and public consent have long and equitably sanctioned.

7. Because, if in very rare instances, exorbitant charges were made for non-parishioners, adequate protection existed; while, on the other hand, our moderate claims for such cases, promptly attended without a written authority, (to avoid serious inconvenience to the patient from delay,) have been in a great proportion of instances dishonourably refused, and, therefore, irrecoverable.

8. Because the alleged disgraceful collusion with inferior parish officers is unknown to us, and we believe can be completely disproved by the profession at large; which is entitled to require not only the ground of such a charge, but the exposure of any perpetrator of so nefarious a practice.

9. Because the inference drawn in the report, that a medical education recently acquired at the schools is equivalent to the knowledge resulting from long practice and experience, is not sanctioned by reflection, courtesy, or justice; but is strangely opposed to universal practice and opinion.

10. Because right feeling and sound policy alike dictate that medico-parochial duties, from their variety, importance, extent, and great responsibility, should be entrusted to those who have previously established their moral and professional character, and have local ties in the neighbourhood.

11. Because it is not true that "the interests of the public and of the profession" are consulted by the system of medical contracts by "tender," since it affords no evidence of the general fitness of the "tenderer" for so important an office.

12. Because although it is asserted "that the candidate in fixing his own terms consults his own interest," yet it cannot be admitted "that he considers the interests and advantages of his profession;" especially, when influenced by the inducement of "obtaining more profitable practice," unjustifiably held out in the report, he is led to attempt an invasion of the private rights and justly acquired interests of his medical brethren.

13. Because the mode of obtaining the appointment, and the degrading conditions of its tenure, divest it of all the supposed "credit," and render it to the experienced and established practitioner merely a choice of evils.

14. Because the "inducements to the acquisition of the appointment," by so reprehensible a mode, are calculated to allure a succession of unknown and needy adventurers, rather than to encourage established and experienced practitioners.

15. Because the assumed analogy between the duties and advantages of an "appointment to any of the chief medical institutions of the country," and those of a medico-parochial office, is entirely fictitious.

16. Because lawful associations for the maintenance of private rights, and the protection of legitimate interests, arbitrarily assailed by authority, ought not to be libelled with the opprobrious epithet of "combinations."

17. Because we are prepared to prove that the system of "medical relief," adopted so much to the satisfaction of the Poor-law Commissioners, is lamentably deficient, as a prompt, humane, and effective provision for the sick poor.

18. Because such a provision can be devised only by the aid of those who have been long and practically acquainted with the subject; and we, having known and deprecated the abuses of the old system, venture to claim a right to express our opinion, that it is not consistent with policy or justice to attempt the partial removal of those abuses, by the introduction of a new system abounding with still greater evils, and fraught with injury to the poor of a nation long celebrated for its justice and philanthropy.

(Signed)

Nathl. Rumsey, Beaconsfield; Wm. Robarts, Burnham; Wm. Rose, High Wycombe; Harry Lupton, Thame; Wm. Stowe, Buckingham; Robt. Ceely, Aylesbury; Wm. Hayward, Aylesbury; Ed. Tallent, Amersham; H. W. Rumsey, Chesham; J. H. Ceely, Aylesbury; W. Edmonds, M.D., Aylesbury; Hy. Hayward, Aylesbury; James Flesher, House Surgeon to the Bucks Infirmary; Hy. Rumsey, Chesham; Wm. Sutthery, Chesham; Wm. R. Sanders, Chesham; James Rumsey, M.D., Amersham (although not personally interested; but in justice to the Poor, to the Science, and to the Profession; and with the hope that the subject will be re-considered by the Authorities); Thos. Nathl. Gray, Amersham; Thos. Brickwell, Amersham; Lovel Smeathman, Great Missenden; Frederick Wright, Great Missenden; Thos. Camps, Berk-

hampstead; Rd. Steel, and Fs. Burlin, Surgeons to the West Herts Infirmary; Henry Hugh Hilder, Berkhampstead; Thomas Brown, Berkhampstead; John Mackintoy, M.D.; C. Sweeney, Market-Street; T. Sweeney, Market-Street; Rd. Wootton, King's Langley; Robert Merry, Hemel Hampstead; Thomas Eagles Prentice, Hemel Hempstead; John C. Rumsey, Beaconsfield; John Hutchinson, Beaconsfield; R. B. Slater, M.D., High Wycombe (though not personally affected); John Turner, High Wycombe; G. Faithorn, High Wycombe; George Hickman, Great Marlow; William Hickman, Great Marlow; T. G. Brinsden, Great Marlow; Robert Colbourne, Great Marlow; T. J. Hammond, Eton; R. S. Morison, Datchet; Robert Mason, Slough; William Fergusson, M.D., Windsor; J. A. Stanford, M.D., Windsor; Frederick Fowler, Windsor; George Crutch, Loud Water; John Bartlett, Dropmore; Daniel Macknamara, Uxbridge; James Stillwell, Uxbridge; Robt. Norton, Uxbridge; Wm. Rayner, Uxbridge; Chas. Patten, Uxbridge; Saml. Blount, Uxbridge; J. Crisp, Chalfont St. Peters; J. L. Norris, Princes Risborough; J. T. Savory, Wendover; Rd. P. Dewesbury, Tring, Herts; R. J. Moody, Tring, Herts; S. B. Lupton, Thame, Oxon; Hy. Wells Reynolds, Thame, Oxon; Rd. Lee, Thame, Oxon; George Wakeman, Thame, Oxon; Thos. Knight, Brill; George Wills, Whitchurch; Jno. Cowley, Winslow; George Cowley, Winslow; Jno. W. Thos. Wynter, Winslow; Edwd. Pope, Winslow; Jno. Scott, Buckingham; Thos. Perkins, Buckingham; E. W. Haslop, Buckingham; Edwd. Southam, Buckingham; J. T. Kirby, Buckingham; James Blundell, Buckingham; Willm. Davis, Bicester; Wm. Dawson, Bicester; Thos. Dawson, Bicester; Geo. Woodward, Bicester; Chas. Jno. Brickwell, Banbury; Chas. Brickwell, Banbury; Thos. Brayne, Banbury; Hy. Robt. Brayne, Banbury; Robt. Brayne, Banbury; Danl. F. Tyerman, Banbury; Jno. Wize, Banbury; T. H. Lewis, Stony Stratford; Robt. Marriett Freeman, Stony Stratford; Chas. Lee, Stony Stratford; Jno. Rogers, Newport Pagnell; Edwd. Daniel, Newport Pagnell; C. H. Kipling, Newport Pagnell; James Arrowsmith, Newport Pagnell; R. Collison, Newport Pagnell; Fredk. Gee, Brackley; Robert Gee, Brackley; Thos. Collier, Brackley; Rd. Jones, Brackley; Saml. Gauntlett, Olney; Chas. Aspray, Olney; Thomas Aspray, Olney; Geo. H. Grindon, Olney; Wm. H. Green, Woburn, Beds.; Thomas Parker, Woburn; Edgar Olley, Leighton Buzzard; Phillip Wagstaff, Leighton Buzzard; Surtees W. Clarance, Thaxted, Essex; John Green Bishop, Maidenhead; Charles Williams, Maidenhead; Richard Goolden, Maidenhead; John Westell, Maidenhead; Matthew Cooper, Windsor.

The deputation, which was kindly accompanied by Sir William L. Young, Bart. M.P., and William Rickford, Esq. M.P., had the honour of an interview with Lord John Russell, on Thursday last, the 17th instant, at the Home Office.

After stating to his Lordship the strong feelings excited in the profession by the tenor of the 25th section of the Commissioners' Report, the deputation proceeded to explain the most important points in the Protest. They then detailed the various grievances existing under the arrangements of the Commissioners relative to "medical relief," as they affected the sick poor, the community, and the profession.

They particularly dwelt on the evils arising out of the appointment, in many instances, of a reduced and insufficient number of medical officers to greatly extended districts;—on the various modes of contracting, especially by tender;—on the general inadequacy of compensation allowed;—on the injuries inflicted on practitioners who declined to accede to the terms and conditions imposed;—on the unjust and injurious subjection of the medical to the relieving officer;—on the utter disregard of the representations and advice of medical men;—on the unmerited and degrading treatment they have generally received from the authorities;—and on the injury that must inevitably accrue to the poor, to the public, and to medical science.

Facts were then stated in illustration and confirmation of the several points alluded to.

The deputation disclaimed being under the influence of factious or unworthy motives; strongly insisted on the sacrifices continually made by the profession in behalf of the needy and indigent portion of the community; and declared the willingness of medical practitioners to acquiesce in arrangements not involving a compromise of the respectability of the profession, or the sacrifice of its just interests.

They contended and proved that the evils arising out of the present scheme of medical relief are of such magnitude and importance as to claim his Lordship's attention; and they were willing to hope that this earnest appeal for his Lordship's interference would be successful in procuring the substitution of a measure founded on liberal and enlightened principles.

Sir William Young, though approving of the general operation of the New Poor-Law Amendment Act, thought the medical arrangements under it stood in great need of revision.

His Lordship, having perused the Pro-

test, and partially attended to the observations and arguments of the deputation, stated, "*that he had not heard of such complaints from any other quarter;*" but promised to see the Commissioners, and make inquiry into the subject.

The deputation assured his Lordship that the medical journals teemed with such complaints, and that he might take the present manifestation of them as a sample of the general feelings of the profession. They accounted for the delay in the presentation of this their Protest; begged permission to leave with his Lordship a concise "statement of the complaints of medical practitioners against the provisions for medical relief adopted by the Poor-Law Commissioners;" and thanking his Lordship for his polite attention, withdrew.

SUGAR POISONED WITH OXIDE OF LEAD.

By C. T. Jackson, M.D.

During the past winter and spring season, a number of persons, said to amount to upwards of a hundred in number, in the town of Calais, Maine, have suffered from a disease of the bowels, of a violent character, resembling *colica pictonum* of the severest kind. Three of the individuals have died in consequence of this disease, after a protracted and most distressing illness; several others are still in a very critical state, and have suffered more or less from paralysis of the extremities. Through the kindness of one of the sufferers, I have been favoured with the names of forty-eight individuals who are still sick with this disease.

The cause of this distressing malady has been carefully examined into by Dr. S. S. Whipple, of Calais, and through his exertions suspicions were finally fixed upon the sugar which had been used in the families of those who suffered; and it was ascertained that the only article of which they had all partaken, was sugar obtained from one importing house at St. Stephens, N. B. It was furthermore observed, that those persons in the families where the disease prevailed, who did not make use of sugar, escaped altogether, while those who indulged most freely in its use suffered the most severely. Thus the chain of evidence was complete against the sugar, and the disease, supposed at first to be an epidemic, is in the end proved to arise from poison. Five or six of those persons who were subject to this colic, set out for Boston for the purpose of obtaining medical advice; one, a young lady by the name of Darling, died on board the packet, under the most distressing symptoms, attended with paralysis of the limbs. The other passengers are undergoing medical treatment in this city, and still bear the marks of great suffering and extreme emaciation, the countenance in every one whom I have seen showing that peculiar expression which accompanies disease of the abdominal viscera.

I have minutely examined four of the sufferers, and from them have learned the foregoing particulars. It appears that the sugar was brought from Barbadoes late last autumn, and was sold by an importer at St. Stephens, who supplied the trade at Calais. It was also ascertained that the captain of the ship who brought out this sugar had a small adventure of the same kind, and that he, and those to whom he sold his sugar, suffered from this disease.

After collecting the above evidence against the suspected sugar, it was thought advisable to make a chemical analysis of it. Four parcels, consisting of about a pound each, were put in my hands by Mr. Lee and Captain Rodgers, with a request that I should make an analysis of each of them, and ascertain positively whether they contained poison or not.

The parcels were marked Nos. 1, 2, 3, and 4, and were subjected to analysis in the order of their numbers.

My suspicions and those of Dr. Whipple were fixed on oxide of lead as the poisonous ingredient, and the results of the analysis prove that this opinion was well founded.

They also prove that a small quantity of this poison, when taken daily, although no immediate disturbance is felt, produces great derangement of the system, and induces a most dangerous and painful disease, which lingers long in the constitution after the use of the deleterious article has been suspended. How often do people exclaim that certain articles are not poisonous, because they have sometimes partaken of them with impunity, when we know that if persevered in, disease and death must be the consequence of their temerity!

I annex the subjoined extract from my laboratory notes.

June 7th, 1835.—Four parcels of brown sugar were handed to me by Captain Rodgers and Mr. Thomas Lee, for chemical analysis. They are marked Nos. 1, 2, 3, and 4, and weigh about a pound each. No. 1 is evidently from a different lot from the other samples. It is of a lighter yellow colour, and coarse grained; while the others are much darker and smaller grained, and in lumps of a still deeper colour. There is nothing peculiar in the taste or appearance of any of the samples

that would cause any suspicion to be raised against the quality of the sugar.

Analysis.—The object of the analysis is to determine if the sugar contains any oxide or salt of lead or copper.

Five hundred grains of the sugar No. 1, burned to cinders in a platina capsule, the cinders crushed to powder in a Wedgewood mortar, and then burned to ashes in the capsule. The ashes were placed in a green glass flask, and digested with nitric acid, and evaporated to dryness; then treated with water and filtered. The filtered solution was placed in a flask, and a current of sulphuretted and hydrogen gas passed through it until the liquor was saturated. No precipitate took place, from which it will appear that this sample does *not* contain any lead or copper.

I have since learned that this sugar was sent for the purpose of ascertaining if it were free from poison, and was not of the kind used by the family at the time they suffered from the disease.

Five hundred grains of No. 2, which came from the house of Mr. Lee, were treated exactly as No. 1; and when the sulphuretted hydrogen gas was passed through it, a copious precipitate of sulphuret of lead took place, which being collected on a filter, washed, dried, and weighed, amounted to 1·6 grains; equal to 1·38 grains metallic lead, equal to 2·337 grains oxide of lead. This will give nearly 38 grains of oxide of lead to the pound of sugar.

Five hundred grains of No. 3, treated in like manner, gave a precipitate of sulphuret of lead, the weight of which is precisely the same as that obtained from No. 2.

No. 4, sugar from Mr. Darling's family. Five hundred grains, treated like No. 1, gave, when sulphuretted hydrogen gas was passed through it, sulphuret of lead in equal weight to that from No. 2. The sulphuret of lead obtained from Nos. 2 and 4 was reduced before the blowpipe to metallic lead. A portion of each of the precipitates was examined by tests for copper, and none discovered.

The lead in this sugar may be either in the state of acetate, malate, or saccharate of the oxide of lead, the sugar combining with it so as to form a chemical combination. How this sugar became contaminated with lead, I am unable to say. There is no suspicion of criminal design attached to any one, and it is probable that leaden reservoirs were used for the syrup, on account of the comparative cheapness of the metal, and that the free acids in the juice of the cane corroded the lead, and thus produced the poison, which crystallized in combination with the sugar. The dreadful effects of this poison should by all means reach the sugar planters, who distribute so noxious an article to the people of many countries, and must produce consequences at which humanity shudders. If the planters continue to manufacture this poisonous compound, and send it abroad regardless of the consequences, after they learn how much suffering it has caused, (which I am not willing to suppose they will do), they will become criminal in the eye of British law, and liable to the severest penalties.

Indeed, we may feel assured, that as soon as they know the effects of their sugar, they will immediately examine into the source from whence the poison was derived, and prevent a continuance of the evil. Their own *interest* would cause this to be done, even if they were not impelled by higher motives; for their sugar would soon have a bad reputation, which would destroy its sale in the market. The researches into the cause of this disease eminently show the advantage of rational medicine over empiricism; for the empiric would never have traced the disease to its remote cause by a connected mode of research, and consequently would have been unable to learn the cause of the malady, and its method of cure. The symptoms in the cases all pointed to lead as their cause, and chemical analysis has confirmed the truth of this opinion. The cause is thus found out and removed; and rational medical treatment will soon restore the surviving sufferers to health.

It is surprising that *colica pictonum* is not a more frequent disease than it is, considering the numerous applications of lead to domestic use. Indeed, I have several times been able to trace the origin of this disease to the use either of leaden reservoirs for water, or leaden suction tubes in wells, where the water was charged with carbonic acid. Such wells are common in Boston, and I have several times been called to witness the effects of water charged with this gas, on lead pipe, which had been corroded entirely through in the course of two years after it was placed in the well. Whenever water contains carbonic acid, lead suction pipe should be carefully avoided, and block tin substituted in its place; for lead is not only soon destroyed by such water, but a dangerous poison is produced, capable of slowly undermining the most vigorous constitution*.

* Medical Magazine, June 15, 1835; and American Journal of Medical Sciences.

NEW METHOD OF OPERATING FOR CATARACT.

Dr. Jungken makes the following remarks:—Guided by experience, which has shown that the chrystalline lens is frequently absorbed when deprived of its capsule, and freely exposed to the action of the aqueous humor, I have been led to try a new method of operating, which, with less lesion of the parts than is required for extraction, is calculated, neverhteless, to give us surer results than follow the breaking down of the lens. The principle of this operation consists in removing the anterior part of the capsule, and bringing it away entirely from the eye, while the crystalline lens itself is left *in situ*, the whole of its anterior surface being thus exposed to the action of the aqueous humor.

The operation was performed, for the first time, on a weak and emaciated girl. The cornea was divided, by means of a cataract knife, near its external edge, at half a line from the sclerotica, by an incision of from three to four lines, directed parallel to its circumference. By this opening, a small fine penetrating needle was introduced through the pupil (which had been previously dilated) into the posterior chamber; the capsule was seized at its inner edge, and an attempt made to tear off round its circumference, with a view of extracting it through the wound. This last part of the operation succeeded but imperfectly, and it was necessary to introduce a small pincers, by means of which the capsule was removed in portions. The immediate consequences of the operation were unsatisfactory, but the lens was ultimately absorbed. The particulars are detailed in *Rust's Magazin.*

NITRO-MURIATIC ACID BATH.

M. Schlesinger has recently employed this remedy in chronic disease of the liver, as well as in a variety of cases dependent upon defective biliary secretion. His formula is—muriatic acid, ℥iij.; nitric acid, ℥ij.; water, ℥vj.; to be divided into three portions, one of which is to be mixed with from 15 to 50 lbs. of warm water. This is used as a foot-bath, for 20 to 25 minutes before going to bed. Gentle laxatives are used at the same time.—*Hufeland's Journal.*

TREATMENT OF EPILEPSY BY INDIGO.

M. Noble is at present engaged in experiments on the treatment of this disease by the above method. By aid of this substance administered gradually in doses of three or four drachms, under form of an opiate, three persons seriously affected with epilepsy have been cured. The first subject was a young man, aged 18 years. The fits occurred every eight or ten days, for ten years. Two months have now elapsed without their recurrence. The second person was a young woman, aged 20, who had been affected for sixteen years, and in whom the fits occurred ten or twelve times a day since the period of menstruation. In six days the fits disappeared under the treatment by opiate of indigo. The third subject was a woman, aged 58, suffering under epilepsy for twenty years; the fits occurring four or five times a day. In four days the treatment by the opiate of indigo triumphed over the disease. In the first patient the indigo, raised to three or four drachms, produced some spasms analogous to those produced by strychnine; with two others an active diarrhœa set in, when the medicine was carried to the above dose.—*Arch. Gén.* Nov. 1835; and *Dublin Journal.*

PROFESSOR DEWEES.

This eminent practitioner and teacher, who has for several years filled the obstetric chair in the University of Pennsylvania, with the highest honour to himself and advantage to the school, has been compelled by illness to resign his professorship. He is at present in the Island of Cuba, in the hope of benefitting his health by a winter's residence in a warm climate; and we are happy to state that recent letters from him afford the pleasing prospect of these hopes being realized.—*American Journal.*

DR. HOSACK.

The *American Journal of Medical Sciences* records the death of Dr. David Hosack, whose name was so long connected with the medical literature and practice of America. He died of apoplexy on the 22d of December last; and it is said that the fatal event was induced by the calamitous fire which desolated New York about that date.

ANATOMICAL WAX MODEL OF THE EYE.

At the Royal Institution, on Friday evening, Mr. Schloss, of Great Russell-Street, exhibited a very beautiful wax work of the human eye, enlarged to four diameters. It is not a mere section, vertical or horizontal, but the entire eye, with its membranes, all capable of demonstration by being taken to pieces. The material is of an elastic nature, transparent where it should be, and capable of being handled without any detriment. It appears to be well adapted for the lecture-room.

ANNUAL METEOROLOGICAL REPORT FOR THE YEAR 1835,

Kept at EDMONTON *and* CHELTENHAM.

EDMONTON.

Month.	Thermometer. Highest	Thermometer. Lowest.	Thermometer. Mean.	Thermometer. Range.	Barometer. Highest	Barometer. Lowest.	Barometer. Mean.	Barometer. Range.	Rain. Inches.	Winds. N.	S.	E.	W.	N.E.	S.E.	N.W.	S.W.
January...	55	16	39·63	39	30·63	29·09	29·9180	1·54	·975	4½	3	..	10	1½	1	4	7
February .	52	24	41·53	28	30·33	29·16	29·6890	1·17	2·9	2½	..	..	4	..	..	4½	17
March	55	25	41·10	30	30·47	29·04	29·8053	1·43	2·625	3½	1	2	4	6½	..	3½	10½
April	66	25	47·41	41	30·36	29·51	30·0131	·85	1·2	8½	..	2½	3½	3½	1	3½	7½
May	71	37	55·15	37	30·11	29·40	29·7958	·71	2·625	3½	..	1	6½	3	0½	9	7½
June	84	41	62·80	43	30·29	29·18	29·9115	1·11	2·275	9	1	3	½	5½	1½	5	4½
July.......	85	45	65·92	40	30·19	29·66	29·9979	·53	·45	1½	5	2	5½	6	1	0½	9½
August....	85	41	64·31	44	30·28	29·53	29·9119	·75	·1875	4½	1	8	5	4½	1½	1½	5
September.	76	37	55·69	39	30·11	29·10	29·7010	·92	3·625	0½	6½	4	6	..	5	1	7
October...	62	27	48·42	35	30·31	28·80	29·7307	1·51	4·785	2½	5	5	9	2	1	2	4½
November.	55	26	43·76	29	30·31	29·14	29·5255	1·17	2·35	6½	5	1½	3½	2	5½	2	4
December.	51	10	33·94	43	30·45	29·34	30·0781	1·11	·225	3½	3½	1	8	2	2	2	7
Year	85	10	49·96	75	30·63	28·80	29·7503	1·83	24·2225	50½	31	30	65½	38½	20	38½	91

CHELTENHAM.—KEPT BY MR. SAMUEL MOSS.

Month.	Thermometer. Highest	Thermometer. Lowest.	Thermometer. Mean.	Thermometer. Range.	Barometer. Highest	Barometer. Lowest.	Barometer. Mean.	Barometer. Range.	Rain. Inches.	Winds. N.	S.	E.	W.	N.E.	S.E.	N.W.	S.W.
January ...	55·5	20·0	35·231	35·5	30·55	28·75	29·81	1·78	1·025	2	5	3	2½	2	6	1	9½
February ..	52·5	29·0	42·566	23·5	30·21	28·81	29·575	1·37	3·35	1½	5	..	6	..	..	5½	10
March	59·5	30·5	42·274	29·0	30·31	28·72	29·718	1·62	3·3	3	5	3½	4	1	..	6½	5
April	69·0	29·0	48·32	40·0	30·25	29·35	29·689	·9	1·7	3	3	1½	½	1½	2½	9	6
May	67·5	41·0	52·192	26·5	29·91	29·21	29·623	·73	4·315	½	2½	3	6½	3½	2	..	13
June	81·5	41·0	59·805	40·5	30·13	29·12	29·82	1·01	3·2	1½	1½	1	1½	6	7½	1	1
July	81·0	51·0	62·742	31·0	29·97	29·51	29·792	·46	1·025	3	2½	3	3	2½	3½	½	13
August	80·5	48·0	62·637	32·5	30·07	29·28	29·71	·79	1·225	1½	6	2	8½	1½	6½	1½	3½
September .	71·5	42·0	56·208	29·5	29·93	29·86	29·153	1·07	3·17	..	8	..	0½	..	9	1	5½
October....	61·0	35·5	48·734	25·5	30·17	28·70	29·525	1·47	6·03	½	7	½	3½	½	7½	4½	7½
November..	55·5	31·5	44·923	24·0	30·20	28·83	29·682	1·37	3·325	4	1½	½	2½	4	11	½	3½
December.	53·0	16·0	36·927	37·0	30·53	29·06	29·937	1·27	·88	2	10	1	4	5	3	..	6
Year.....	81·5	16·0	49·305	65·5	30·55	28·70	29·716	1·85	33·25	24¼	60	19	48½	33½	58½	31	86
Year 1835	81·5	16	49·305	65·5	30·55	28·70	29·716	1·85	33·445	24½	60	19	48½	33½	58½	31	86
1834	78·5	30	51·983	48·5	30·57	28·91	29·813	1·66	31·50	20½	53½	23	35	45½	18½	22½	116½
1833	79	27	50·297	52	30·33	28·60	29·639	1·78	33·155	33	43½	28½	61½	28½	58	28½	83½
1832	76·5	28·5	51·206	48	30·26	29·01	29·725	1·25	..	33	5½	22½	45	31½	57	32	90½
1831	77·5	25	52·205	52·5	30·20	23·52	29·61	1·74	..	36½	54½	32½	43½	27	48½	21	101½
1830	81·5	10	49·275	71·5	30·38	28·73	29·65	1·62	..	12	41½	30	50	15½	51	18½	113½
1829	79·5	17	47·707	62·5	30·32	28·71	29·593	1·61	..	57½	38½	41½	35½	52½	36	28½	75
1828	78·5	27	51·75	51·5	30·23	28·82	29·683	1·41	..	30½	48	35	38½	26	53	20½	114½
1827	80·5	16	49·158	64·5	30·37	28·62	29·693	1·75	..	32	44	25½	40½	41	43	32½	100½
1826	86	18·5	50·385	67·5	30·24	28·87	29·725	1·37	..	36	46½	21	30	50	52½	47	79
1825	93·5	24	58·75	69·5	30·52	28·76	29·64	1·76	..	50½	44	38	49	25	46	22½	88

1835.—THERMOM.—Highest, 81·5, June 10. | Lowest, 16·0, Dec. 25. | RAIN, 33·445. | BAROM.—Highest, 30.55, Jan. 2 | Lowest, 28·70, Oct. 10.

GENERAL OBSERVATIONS.

THE mode of keeping these registers is as follows:—

At *Edmonton*, the warmth of the day is observed by means of a thermometer exposed to the north in the shade, standing about four feet above the surface of the ground; the extreme cold of the night is ascertained by a horizontal self-registering thermometer, in a similar situation. The daily range of the thermometer and barometer is known from observations made at intervals of four hours each, from eight in the morning till the same time in the evening. The weather and direction of the wind are the result of the most frequent observations. The rain is measured every morning at eight o'clock.

At *Cheltenham*, the temperature is ascertained by a horizontal self-registering thermometer, suspended about five feet from the ground, in a north-east aspect; and the observations made at eight o'clock A.M. The pressure of the atmosphere, and the direction of the winds, are registered at eight o'clock A.M., and eight o'clock P.M. The quantity of rain which has fallen is registered at eight o'clock A.M.

CHARLES HENRY ADAMS.

A GENERAL BILL

OF THE

BURIALS, WITHIN THE CITY OF LONDON, AND BILLS OF MORTALITY,

From Dec. 9, 1834, to Dec. 15, 1835.

DISEASES AND CASUALTIES OF THE YEAR.

Diseases.

Abscess	176
Age and Debility	2345
Apoplexy	437
Asthma	879
Cancer	101
Childbirth	277
Cholera	5
Consumption	3662
Constipation of the Bowels	16
Convulsions	1955
Croup	167
Dentition or Teething	445
Diabetes	8
Diarrhœa	36
Dropsy	810
on the Brain	700
on the Chest	85
Dysentery	5
Epilepsy	31
Erysipelas	52
Fever	420
(Intermittent or Ague)	7
(Scarlet)	445
(Typhus)	77
Fistula	7
Gout	93
Hæmorrhage	38
Heart, diseased	144
Hernia	18
Hooping-cough	652
Indigestion	15
Inflammation	1714
Bowels & Stomach	226
Brain	162
Lungs and Pleura	406
Influenza	1
Insanity	165
Jaundice	37
Jaw, locked	9
Liver, diseased	300
Measles	734
Miscarriage	12
Mortification	224
Paralysis	162
Rheumatism	24
Scrofula	12
Small-pox	863
Sore Throat and Quinsey	52
Spasm	71
Stone and Gravel	22
Stricture	12
Thrush	90
Tumor	38
Venereal	6
Worms	13
Unknown Causes	582
Stillborn	966

Casualties.

Drowned	109
Died by Visitation of God	45
Excessive Drinking	19
Found Dead	11
Killed by various Accidents	169
Murdered	3
Poisoned	10
Suicides	41

Buried { Males ...... 10,064 / Females .... 10,451 } Total 21,415

Of the number buried were,

Stillborn	966
Under 2 years of age	5416
2 and under 5 years	2319
5 and under 10	1011
10 and under 20	754
20 and under 30	1424
30 and under 40	1640
40 and under 50	1892
50 and under 60	1816
60 and under 70	1769
70 and under 80	1613
80 and under 90	685
90 and under 100	103
100	3
101	1
102, 104, 105	3

Decrease in the burials reported this year, 264.

APOTHECARIES' HALL.

LIST OF GENTLEMEN WHO HAVE RECEIVED CERTIFICATES.

March 24, 1836.

Samuel Gillett Gilbert, Blofield, Norfolk.
Henry Rickards, Hereford.
George Ambrose Cope, Melbourne, Derbyshire.
John Alfred Lush, Berwick, St. John, Wilts.
Thomas Fletcher Tyeman.
John Wordsworth Savage, Hull.
Robert Latten, Colchester.
John Gurney, Sheffield.

NEW MEDICAL WORKS.

Elements of Medical Jurisprudence. By Alfred S. Taylor, F.L.S., Lecturer on Medical Jurisprudence at Guy's Hospital. Vol. 1, 8vo. 15s.

Report on the Medical Management of the Native (Indian) Jails. By James Hutchinson, A.M., M.R.C.S. 8vo. Calcutta, 1835.

Elements of Agricultural Chemistry; in a Course of Lectures, by Sir H. Davy, Bart. LL.D. F.R.S. Fifth Edit. 8vo. 15s. bds.

Practical Observations on Midwifery. By James Hamilton, M.D., &c. Part I. 8vo. 7s. 6d. bds.

The Physical and Intellectual Constitution of Man considered By Edward Meryon. 8vo. pp. 240.

A Treatise on the Chemical, Medicinal, and Physiological Properties of Creosote; being the Harveian Prize Dissertation for 1836. By J. Rose Cormack. 8vo. pp. 154.

WEEKLY ACCOUNT OF BURIALS,

From BILLS OF MORTALITY, *Mar.* 22, 1836.

Age and Debility	43
Apoplexy	1
Asthma	11
Cancer	2
Childbirth	3
Consumption	51
Convulsions	30
Dentition or Teething	7
Diarrhœa	1
Dropsy	8
Dropsy on the Brain	16
Erysipelas	1
Fever	9
Fever, Intermittent, or Ague	1
Fever, Scarlet	3
Gout	4
Hæmorrhage	1
Heart, diseased	1
Hooping Cough	1
Inflammation	15
Bowels & Stomach	2
Brain	2
Lungs and Pleura	5
Insanity	2
Liver, diseased	2
Measles	1
Mortification	3
Paralysis	2
Scrofula	1
Small-pox	12
Sore Throat and Quinsey	3
Spasms	2
Thrush	1
Worms	1
Unknown Causes	1
Casualties	23

Decrease of Burials, as compared with the preceding week } 1

METEOROLOGICAL JOURNAL.

Kept at EDMONTON, *Latitude* 51° 37′ 32″ N. *Longitude* 0° 3′ 51″ W. *of Greenwich.*

March, 1836.	THERMOMETER.		BAROMETER.	
Thursday . 17	from 32	to 52	29·89	to 30 02
Friday . . 18	45	60	30 19	30 24
Saturday . 19	41	63	30·07	30·09
Sunday . . 20	32	65	30 02	30·14
Monday . . 21	40	53	30 07	29 94
Tuesday . . 22	42	52	29 87	29 83
Wednesday 23	23	53	29·72	29 46

Prevailing winds, W. by S. & S.

Except on the 19th, and morning of the 20th, generally cloudy, with rain on the 17th, 22nd, and 23rd.

Rain fallen, 225 of an inch.

CHARLES HENRY ADAMS.

WILSON & SON, Printers, 57, Skinner-St. London.

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END OF VOL. XVII.

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